

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

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We thank the referees for their comments which were very helpful to improve our manuscript. Two referees asked for a quantitative comparison of our estimates with published studies based on complex models (while the referees acknowledged that such a comparison is not straightforward to perform). We now present such a comparison which we hope will improve acceptance of our quantitative permafrost feedback estimates.

The comparison of our simulated permafrost dynamics with latest model simulations of CCSM4 (Lawrence et al., 2011) has led us to slightly re-adjust our permafrost thaw parameters (see discussion under R-II.9 and R-V further below). We now also compare our model results with latest simulation outcomes from (Wisser et al., 2011) and (Koven et al., 2011).

Another change in the model setting was made to account for the criticism that our assumed

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range for CH₄ oxidation in the soils is likely biased low. We now updated our assumed oxidation rates to allow for larger methane consumption in the soil (see discussion under R-II.6).

We have rerun the whole model ensemble based on the adjusted parameter ranges.

A detailed answer to all referee comment follows:

Referee 1 (R-I)

1. Terminology “permafrost emissions” and background emissions

I think it is important to make it clear that landscapes with permafrost have carbon fluxes while the permafrost is present, and whether or not the permafrost is changing. They may have net CH₄ emissions or uptake, net CO₂ emissions or uptake. I'm sure you are well aware of this, but you might be misinterpreted. For example, in figure 3b and 3c, these are not really 'permafrost emissions'. There is still permafrost present at least until 2150 in the RCP8.5 (though deeper in the soil), and the organic matter in that permafrost is still essentially inert (frozen); the reported emissions derived from organic matter in newly thawed soil. In Fig. 3d,e it is not permafrost induced change, but thaw induced change. Any 'permafrost thaw' emissions will be in addition to the net emissions that were occurring before the thaw, which may also be changing with climate change due to warming, wetting/drying, elevated CO₂, vegetation dynamics, ... (do you account for this potential change in 'background emissions' in the analysis? If not, that should be made clear). I think that 'emissions derived from organic matter in newly thawed soil' is what you intend to report (a shorter name would be better), and, if so, I think that you should state it clearly early in the paper, and make sure your terminology is clear.

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In our discussion paper, by using the term “permafrost emissions”, we referred to carbon emissions which are a consequence of thawing the pool of carbon which is stored in intact permafrost for pre-industrial climate conditions. These emissions are additional to background carbon fluxes from permafrost regions (which result from carbon uptake and release by the active layer above the permafrost body) and from vegetation induced carbon fluxes.

In our revised manuscript we now more clearly define the “permafrost-carbon feedback” and discuss the role of background permafrost emissions in section 2.1., which now reads:

In this study we define the “permafrost-carbon feedback” as the contribution of near-surface permafrost carbon (upper 3m) which is presently stored in perennially frozen ground. The full permafrost-carbon feedback is also affected by climate driven changes in background emissions in permafrost regions. These emissions result from changes in respiration of Arctic soil carbon, which is stored in the active layer under present day climate. Another contribution to background emissions results from vegetation induced carbon fluxes in permafrost regions – through higher CO₂ fertilization and increased CO₂ uptake from vegetation growth.

In our analysis we account for higher CO₂ sequestration through fertilization given the calibration of MAGICC6 to the C4MIP carbon cycle models. As only a few C4MIP models included dynamic vegetation modules, our emulations do likewise not fully capture modified carbon fluxes resulting from vegetation-driven changes in permafrost regions. We also do not account for changes in soil respiration from surface layers above our considered permafrost pool (see section 2.3). Increased respiration from these surface soil layers and increased carbon uptake through vegetation growth cancel out to a certain extent (they might cancel out almost completely, see Koven et al. 2011). A further compensation comes from changes in surface albedo (see discussion in section 4). So we expect that our inferred carbon fluxes which define the “permafrost-carbon feedback” are a good approximation to the overall strength of the full permafrost-carbon feedback.

Furthermore, we now use the terminology “emissions from newly thawed permafrost soils” which better describes the quantified carbon fluxes.

1. The nonlinear patterns in permafrost carbon sensitivity (Section 3.3 and Table 2)

are an interesting result, but the pattern is not easy to understand. Could you expand

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this discussion a bit to explain why (1) the sensitivities increase over time, and (2) are higher for the high (RCP8.5) and low (RCP3-PD) scenarios than the intermediate scenarios (RCP4.5 and RCP6)?

There is a general tendency of an increase in carbon sensitivity γ_{LP} with time. This is a consequence of the slow time scale of permafrost carbon decomposition:

carbon emissions at a later stage in the simulation (e.g. by year 2300) do not only result from thawed soils in the 23rd century but also from carbon in soil bands which have thawed much earlier (e.g. in 21st and 22nd century). These soil bands still contain carbon that has not been fully decomposed and contribute to overall permafrost carbon fluxes in year 2300. Additional to that, carbon sensitivity increases due to higher soil respiration at warmer temperatures (see equations A7, A8).

We have added the following sentence in the new manuscript:

“This increase in carbon sensitivity with time can be understood given the slow decomposition timescale of soil organic matter: carbon fluxes by 2300 are not only a consequence of permafrost thaw in the 23rd century but are also affected by emissions from soil bands which thawed much earlier in the 21st and 22nd century. Furthermore, soil respiration increases with rising temperatures (see equation A6 and A7).”

An exception from this general characteristic is seen for RCP6 by year 2100. In this case carbon sensitivities are almost equal to those inferred from RCP45 (actually they are slightly lower). This can be understood as both forcing scenarios are comparable in magnitude for most of the 21st century. The slightly higher carbon sensitivity for RCP4.5 compared to RCP6 by 2100 results from comparable radiative forcing in both scenarios until ~ 70 , while the larger forcing of RCP6 for year 2070-2100 results in a stronger temperature increase, but not in a comparable increase in newly released carbon (this time span is too short to have a pronounced effect on emitted permafrost carbon).

Consequently the emitted carbon is of rather comparable magnitude in both scenarios until 2100, but the global temperature increase is larger in RCP6 compared to RCP45 (which is used as a normalization factor for calculating γ_{LP} – and hence the carbon sensitivity is slightly lower in RCP6 in 2100).

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3. Equation 3: the latitudinal amplification factor, α – should that be constant for the entire permafrost domain, or should it have a latitudinal gradient? How much would that affect your results?

We do not account for a latitude-dependent polar amplification factor. Although it is likely that high northern permafrost regions experience a stronger temperature amplification (e.g. by the effect of sea-ice decline and transport of the resulting heat anomaly inland), we assume that uncertainty of latitude-dependent permafrost carbon release is mainly determined by the uncertainty in soil carbon densities (which we account for in our setting).

4. I was a bit confused by the ‘summer temperature’ terminology. Is this really an estimate of summer temperature (e.g., temperature of warmest month or June/July/August average or something like that) or just a measure of effective thaw temperature – mean annual temperature above some threshold for each zone? What sort of values do you get for T-summer (1_ would be a guess for warming above a threshold, 10-15_ would be a guess for a real summer temperature)?

We here discuss a threshold temperature above which permafrost thaw is initiated (with typical values being in the range of a few degrees above zero). To avoid misinterpretation we now refer to this temperature as “ T^{max} ” in equation (A3).

5. Table 1: it would be helpful to have citations for the default and/or sensitivity ranges in the table, either as an additional column or as footnotes. In the sensitivity range for the Q10 parameters, you report two parameter values but only one Q10 value. The high end of your sensitivity range for methane oxidation fraction (10-20%) seems low to me; I don’t think this process is that well-constrained by observation at this point.

We will include references into table 1. For Q10 we report both, the lower and upper value in table 1 (separately for aerobic and anaerobic decomposition).

We agree that our chosen range for methane oxidation is likely biased low. Therefore, we now chose to modify the parameter setting to account for higher oxidation rates. We also introduced different oxidation rates for peat (50-70%) and for mineral soils (10-40%). See discussion under

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R-II.6 further below.

TECHNICAL CORRECTIONS

a. In some places you use 'MAGICC' and in others 'MAGICC6'. If these are different, please explain; if not, please use consistent notation.

We refer to the same model version. We now use a consistent notation.

b. lines 179-180: what do you mean by 'temporal' wetlands? Temporary? If so, for how long? Seasonal?

We now removed this part of the sentence to avoid misinterpretation.

c. lines 193-195 or so: I don't think it is a given that precipitation increases will outweigh ET increases everywhere, particularly given the uncertainty in precipitation (relative to temperature, which may be a reasonable predictor of ET). I believe some macroscale hydrology simulations show that runoff changes are relatively small, due to offsetting increases in ET and precipitation (Fekete et al. Global Biogeochem. Cycles, 24, GB0A12, doi:10.1029/2009GB003593, looking ahead to 2050).

In the manuscript we stressed that uncertainties in this regard are large. Given that there are offsetting effects we keep the anaerobic fraction constant in our simulations (assuming no pronounced net change in the area of water-saturated soils).

d. line 206: Heimann, not Hermann.

Corrected for...

e. The thermokarst methane bubble flux results of Walter et al. have gotten a lot of attention, but I don't think that they have been widely replicated. They are worth mentioning, but once is enough.

Taken into account now...

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f. line 432: what do you mean by 'inert'? maybe 'inertia'?

We now call it "lagged response". . .

g. line 524: I don't see a hyphen (i.e., -); do you mean a 'prime' (i.e., ')?

Yes...

h. line 525: since the A term is a fraction, it would probably be better to say 'starting at 1.0' rather than '100%'

We agree. . .

Referee 2 (R-II)

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
See discussion above (R-I.1).

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?

In our manuscript we quantify carbon fluxes from newly thawed permafrost soils. These are on top of total fluxes in permafrost areas. We now discuss this issue more in detail in the revised manuscript (section 2.1). See discussion above (R-I.1).

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

We will include references in table 1 in the revised manuscript.

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

We now motivate the choice of our assumed respiration rates in peatlands and mineral soils by stating:

We assume slightly lower decomposition rates in peatland soils compared to mineral soils (ratios of 0.3 to 0.7) given that moss litter – which is abundant in peatland soils – decomposes rather slowly (Hobbie et al., 2000;Scanlon and Moore, 2000).

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

Yes, we assume uniform distributions (mentioned under 2.2. (“simulation set-up”) and indicated in the heading of table 1).

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.

In our simulation set-up we have made no distinction between oxidation rates in peatlands and in mineral soils. We appreciate the hint that oxidation rates in peatlands can be rather large. Thus we likely underestimated oxidation of methane in permafrost soils in our setting. In our improved model set-up we now assume much higher CH₄ oxidation rates for peatlands (50-70%) than for mineral soils (10-40%) – as compared to our previous setting where we assumed overall oxidation rates between 10-20%.

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

Corrected for. . .

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

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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 has 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.

We emphasized this point in the text which now reads:

...

As past decomposition has left carbon of low quality in the soils before incorporation into permafrost (Schuur et al., 2008), we assume a relatively slow decomposition time of carbon in both soil types compared to high turnover rates of freshly formed organic detritus. We tune the aerobic decomposition rate of the largest permafrost carbon stock, i.e. carbon in mineral soils, to correspond to a turnover time at 10°C of between 30 and 60 years. This results in slightly slower decomposition for the majority of model versions as compared to the 33 year turnover timescale for the intermediate pool used in the Lunds-Potsdam-Jena dynamic vegetation model (Sitch et al. (2003)). We do not explicitly simulate decay of low quality carbon (Schuur et al., 2008) like organic matter bound to clay particles. We implicitly account for this part of the soil carbon pool by discussing uncertainty in the pool size of carbon available for decomposition. The carbon most resistant in our simulations resides in the slow anaerobic peatland pool with decomposition up to two orders of magnitude slower than for carbon in the mineral aerobic

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pool.

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.

In our discussion paper we emphasized that a direct comparison of model results is hindered by different definitions of permafrost degradation in published studies (expressed by differing assumptions about permafrost depth, see e.g. discussion in (Wisser et al., 2011)).

We now have compared our results with latest (climate bias corrected) runs of CCSM4 (Lawrence et al., 2011) for identical forcing scenarios. Hereby we compare thawed permafrost volumes, not areas. This comparison has let us to slightly adjust our model parameters which affect permafrost thaw towards larger values (i.e. the thawing rates β_{ms} and β_{peat} and the temperature threshold T_{max}). We now simulate permafrost thaw with CCSM4 results falling in between our lower and upper quantiles (68% range) for all four RCP forcing scenarios.

Additionally, we now compare our results to latest modeling outcomes of permafrost thaw with GIPL (Wisser et al., 2011). In this study the authors infer an increase of about 20% for the thawed volume of northern American peat (with an average depth of 3.2m) by 2100 under the A1B scenario. This value is comparable to our new median estimate (19%) under the RCP6 scenario (which describes a comparable forcing until 2100).

Latest modeling results by (Koven et al., 2011) suggest a reduction of top 3m permafrost extent by 30% by 2100 under the SRES A2 scenario.

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

Yes, now corrected. . .

Referee 3 (R-III)

1. I was familiar with the many acronyms that the authors use in the paper, but it

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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.

We will account for this in the revised manuscript.

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 CO₂ (or CO₂-equivalent) concentrations or globally-averaged warming associated with the key SRES/RCP scenarios might help make this comparison.

We now added the following information:

These numbers can be put into perspective with fossil fuel emissions from the recent Representative Concentration Pathways (RCPs), which succeed previous SRES emission scenarios. In these new scenarios, GHG concentrations are extended beyond 2100 until year 2300. Overall, RCP4.5 is comparable to the previously lowest scenario SRES B1 and the RCP8.5 is comparable to the previously highest scenario SRES A1FI

Scenario specific CO₂ concentrations (SRES and RCPs) can be found here (slide #57):

www.pik-potsdam.de/%7Emmalte/ipcc/Meinshausen_CMIP5_IPCCworkshop_1Nov2010_FINAL1.ppt

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.

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When discussing these uncertainties, we now refer in the introduction to relevant review papers from (Schuur et al., 2008;McGuire et al., 2009;Davidson and Janssens, 2006;O'Connor et al., 2010;Grosse et al., 2011).

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!

We now state that:

Though projected 21st century permafrost carbon emissions are relatively modest, . . .

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

Modified. . .

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.

We remove the reference at this position in the text.

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?

Yes, by “one-sided” we assume that the net climatic effect of permafrost thaw can be described by an overall positive feedback loop (i.e. warming results in permafrost thaw, which amplifies the warming through the net release of carbon). We are not aware of modeling or observational studies which support the idea of a dominance of overall negative feedbacks initiated by permafrost thaw. (McGuire et al., 2006) conclude that the balance of the terrestrial feedbacks

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in the Arctic is likely to be positive over the 21st century.

When mentioning “one-sided” we now refer to section 4 of the manuscript in which we discuss compensating effects through higher carbon sequestration by vegetation growth in permafrost areas. We now also discuss latest modeling results of CLM4 (Lawrence and Swenson) which show that hypothesized protection of permafrost thaw by shrub expansion (Blok 2010) is likely over-compensated by decreases in surface albedo.

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

Corrected for. . .

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.

Yes, we did conduct simulations with the IPSL setting. However, we excluded those for the following reason:

The IPCL-CM2C emulation by MAGICC exhibits a much larger air-to-ocean flux after 2100 compared to the IPSL results shown by Orr et al. 2002 of the Ocean Carbon Intercomparison Project OCMIP. Other model emulations were approximately in line with the OCMIP results. As for this study, emulations beyond the C4MIP time horizon of 2100 were necessary, we hence excluded IPSL, as has been done before in the MAGICC6 applications (see e.g. Reisinger et al. 2010, page 2, left column, paragraph [9]). We hence ammended the text, which reads now

"Each of the 9 carbon cycle parameter sets contains 17 individual parameters to emulate one of the C4MIP models, as described in Meinshausen et al. (2008). We did not include any IPSL CM2C emulations, as the air-to-ocean carbon flux beyond 2100 (the time horizon of C4MIP and hence the MAGICC calibration period) is emulated substantially stronger than shown for the IPSL previously (Orr et al., 2002)."

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 preindustrial climate. In fact, some permafrost may be relict permafrost, having formed in climatic conditions cooler

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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.

As we only consider carbon release from near-surface permafrost (i.e. the upper 3m) we assume that this permafrost is in equilibrium with pre-industrial climate. A part of deeper soil layers is likely affected by relict permafrost (e.g. in deep loess deposits in the Yedoma permafrost complex – yet we do not consider this soil pool in our model as the majority of the pan-Arctic soil carbon resides in near-surface permafrost).

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

We now removed this part of the sentence to avoid misinterpretation.

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 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.

(Wisser et al., 2011) simulate permafrost thaw in mineral and peatland soils. We now refer to this study which suggests a ratio R_{peat}/m_s consistent with our assumed range (see e.g. their figure 6).

p. 4741: One limitation of the study that you might consider describing is nonrepresentation 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.

We now added this aspect in the discussion of the model limitations (section 4):

... Site-specific thaw also arises from the effect on soil thermal properties resulting from unfrozen water in the ground (Alexeev et al., 2007; Yi et al., 2007; Nicolsky et al., 2007) and vari-

ability in excess ground ice concentration.

Figure 3. Minor typos: “This studys” should be “this study’s”. “Results were obtained form an uncertainty. . .” should be “Results were obtained from an uncertainty...”

Now corrected for. . .

Referee 4 (R-IV)

(1) Similar to the comment by Referee 1, I do think it is important to clarify what you mean by “emissions”. For “Cumulative CO₂ emissions”, I’m assuming that you are representing cumulative GPP-RH so that any plant CO₂ uptake (either implicitly or explicitly) in the model is accounted for in the emissions. I’m also assuming that the emissions are from ecosystems in the permafrost region, and not just from the carbon that is currently locked up in permafrost in the top 3 m of soil in the region.

We have clarified this issue in section 2.1 of the revised manuscript (see discussion under R-I.1)

(2) I’m a little fuzzy on the number of soil carbon pools per band. I understand mineral vs. peatland and aerobic vs. anaerobic pools for 4 pools. But, within each band is there a permafrost and a thawed pool, so that you are essentially tracking the dynamics of 8 pools per band (I’m inferring this from the rate of thaw and refreeze parameters in Table 1)? If so, it might be good to be more explicit about this on page 4734. It actually would be helpful to see the state equations for changes in the soil carbon pools explicitly identified in the appendix. This would also help improve the clarity of point number 1 above.

Yes, technically we are tracking the dynamics of 8 pools per band. As permafrost carbon is only emitted from the thawed pools, we focus our discussion on the four pools described on

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page 4734.

(3) A few grammatical (and other) issues: (a) Page 4729, line 27: Perhaps change “nutrients release” to “the release of plant-available nutrients”. (b) Page 4730, line 2: Perhaps change “around thousand Petagram” to “approximately 1000 Pg (10^{15} g)”. (c) Page 4730, lines 6-11: This is a run-on sentence. Please break it up into two or more sentences to improve clarity. (d) Page 4731: There are a couple of references to Meinshausen et al. (2008) on this page, but that publication is not identified in the References section. (e) Page 4737, line 12: Change “ecosystem” to “ecosystems”. (f) Page 4737, line 15: Change “he majority” to “the majority”.

Now accounted for. . .

Short comment by Kevin Schaefer (R-V)

This is a very nice paper that quantifies the strength and timing of the permafrost carbon feedback. I very much like the ensemble approach to estimate uncertainty, which adds great strength to the results. I enjoyed reading it.

These estimates of carbon release from thawing permafrost are too low because the simple permafrost model did not consider talik formation. In Schaefer et al. [2011] we found that the soil column became thermodynamically unstable and thawed quite rapidly once talik thickness exceeded a critical thickness of about 0.5 m. In terms of the simple permafrost model, rapid vertical thaw would occur once the summer thaw depth exceeded the winter freeze depth by about 0.5 m. Talik formation would thaw the entire stock of frozen carbon in each latitude band at critical temperatures much lower than indicated by the simple permafrost model. After talik formation, the amount of thawed

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permafrost carbon and associated flux to the atmosphere increased by a factor of 2-3. Almost two thirds of the total 190 Gt of cumulative carbon flux in Schaefer et al. [2011] came from regions of talik formation along the southern margins of the permafrost domain. Talik formation explains why Schaefer et al. [2011] started with only 313 Gt of frozen carbon, no more than half of Schneider von Deimling et al. [2011], but estimated two or three times the flux.

We now have compared our simulated permafrost thaw with latest results from CCSM4 (Lawrence et al., 2011) for all RCP scenarios. This led us to slightly adjust our model parameters which affect permafrost thaw towards larger values (the effective thawing rates $\beta_{ms,peat}$ and T^{max} – see discussion under R-II.9). We do not explicitly simulate the effect of talik formation on permafrost thaw, but we capture the effect of accelerated thaw for higher temperatures by our simple parametrization: the thawing depth advances more quickly to deeper layers for increasing warming (see page 4746 of our discussion paper).

Our new model results (from which we infer higher thawed volumes of 20-30%) fall well into the range of CCSM4 model simulations. Under the RCP6 scenario, CCSM4 simulates an increase in the volume of thawed near-surface permafrost of 35% in year 2100 (compared to year 2000, climate bias corrected run). This value can be put into perspective with simulated permafrost thaw in SiBCASA for the A1B scenario (Schaefer et al., 2011), which suggests that “Nearly all the permafrost carbon thawed out before 2100”. As also other recent simulation results (Koven et al., 2011; Wisser et al., 2011) suggest near-surface permafrost thaw in the 21st century below such high rates (see R-II.9 above and next paragraphs), we decided to be more close to conservative estimates and did not further enlarge our thaw parameter ranges.

Additional to differences in assumed thawing rates, we expect differences in simulated carbon fluxes to (Schaefer et al., 2011) stemming from differing model assumptions:

In our setting a fraction of about 20% of permafrost carbon resides in the anaerobic pool with decomposition rates up to 40 times slower than for aerobic decomposition. We further consider permafrost thaw in frozen peatland being slower than in mineral soils. Differing assumptions about decomposition rates of newly thawed permafrost carbon are likely to further contribute to differences in simulated carbon fluxes. Currently we are investigating this aspect and might

decide to slightly adjust our range for soil carbon turnover times towards faster values in the final manuscript – thus reducing the current discrepancy.

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