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Interactive comment on "Observation-based modelling of permafrost carbon fluxes with accounting for deep carbon deposits and thermokarst activity" *by* T. Schneider von Deimling et al.

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We thank the referees for their constructive comments which were very helpful for improving our manuscript. By having performed additional model simulations and by showing additional model output (as suggested by both reviewers) we now provide additional information for the interpretation of our model results. This information allows to illustrate the role of individual carbon pool contributions and of model dynamics from hydrologic and depth changes.

In the following we reply to all referee comments (in italics) point by point.

1) What segregates mineral vs. organic pools? In the original version of the model, the organic pools were referred to as peatlands. What really constitutes the difference between the "mineral" and "organic" pools in this version? If we think about the analysis of Harden et al. (2012), which segregates the permafrost domain into turbels, histels, and orthels, how does mineral vs. organic correspond to these designations? Are you referring to mineral horizons and organic horizons of turbels, histels, and orthels that are not yedoma and refrozen thermokarst?

We allocate soil carbon contents according to the inventories estimates of the Northern Circumpolar Soil Carbon Database (Hugelius et al., ESSD, 2013). Hereby, we describe the mineral soil pool by the sum of SOC contents from orthels and turbels, and the organic pool by the SOC content from histels. So far we only had referred to this segregation in section 2.1. of the supplement and in table 1. To clarify our classification, we now mention the segregation of organic and mineral pools in section 2.2 in the revised manuscript. To allocate SOC for the Yedoma and refrozen thermokarst pools, we assume that these inventories are largely dominated by mineral horizons and we discuss the overlap of pools in the supplement (section 2.1).

2) A better description of transitions involving thermokarst lakes and wetlands It is not clear what pool is lost as the thermokarst lake and wetland pools expand. It is also not clear what pool gains when thermokarst lakes contract. Normally, when wetlands can be derived from permafrost degradation of permafrost plateaus or from the contraction of thermokarst lakes, but the carbon dynamics of these two transitions are quite different in my experience. It is also not clear to me what happens to carbon after a transition. Is the carbon pool simply transferred to the new landscape type and subject to the C dynamics of that landscape type depending on depth/latitude band?

Each soil pool (mineral, organic, Yedoma, refrozen thermokarst) is subdivided into an aerobic and two anaerobic compartments. Given the large-scale dominance of aerobic

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over anaerobic landscapes (considered from a full circum-Arctic perspective), we assume that any increase in the area of anaerobic pools (wetland or thermokarst lake) will lead to a decrease of the aerobic pool fraction in each latitude band (and vice versa a decrease in anaerobic pool fractions will result in an increase in the aerobic pool fraction). Carbon is transferred from the decreasing to the increasing pool according to the change in area fractions and is subject to the environmental control of thaw and decomposition of the corresponding new pool. We do not consider the separate, more complex case in which thermokarst lake areas, which were newly formed during our simulation period, develop into a wetland by terrestrialization (also within the time horizon of our simulations). We neither consider the reverse case of a wetland becoming a thermokarst-affected terrain. We consider these transitions an issue for future model extensions.

To clarify our underlying model assumptions for thermokarst lake and wetland dynamics we now discuss the transition of pools in the revised manuscript in section 2.1 (page7) and in the supplement (section 2.3).

3) An improved justification for the substantial depth of thaw in thermokarst lakes in response to future changes in climate. The results of this study are dominated by the methane loss associated with the substantial depth of thaw in thermokarst lakes in response to future changes in climate. The justification of this is from the modelling studies of Kessler et al. (2012) and Ling (2003). But the dynamics in the lower panels of Figure 2 don't make sense to me. I wouldn't expect that the high latitude thaw depths would expand beyond the initial low latitude thaw depths. There seems to be something wrong and unrealistic with the formulations used to model the thickening of the thaw bulb in thermokarst lakes.

Figure 2 shows a two-stage process: 1) a slow deepening of the active layer in sediments overlain by non-thermokarst ponds (until the year 2000), and 2) a strong increase in thawing rates after the pond deepens enough to prevent winter refreeze, effectively initiating a new thermokarst lake (around the year 2000). A strong talik

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deepening in continuous permafrost at stage 2 (Fig.2, lower panels, blue curves) below the initial active layer depth of southerly permafrost at stage 1 (Fig.2, lower panels, red curves) is not at odds with model physics. It rather describes the potential of abrupt and continuous thaw after deep thermokarst lakes have formed. In contrast to cold surrounding ground temperatures, a warm lake bottom supports strong and sustained thaw of thermokarst affected sediments. Therefore, high latitude thaw after thermokarst formation can reach deeper into the ground than at southerly permafrost regions which are not affected by thermokarst.

So far we had only discussed the two-stage description in the supplement (bottom of page 6). We now emphasize this aspect in the revised manuscript in section 3.1 and in the legend of Fig.2.

4) The need to run an ensemble of control simulations for each RCP: One question that I have (and that I think will be of interest to others) is the degree to which the results are driven by the transitions vs. the depth dynamics. To answer this question it would have been helpful to have had a set of control simulations in which (1) there was no consideration of deep carbon, (2) the thermokarst lake and wetland areas were static, and (3) the combination of the two.

We agree that additional control simulations will provide valuable information not included in the current manuscript. We now have performed additional sensitivity simulations for each RCP to illustrate the role of dynamics resulting from transitions vs. depths changes (see additional discussion in section 3.3 of the revised manuscript, and new figure S4 in the supplement).

5) The need to report the amount of carbon lost from each pool I would have found it helpful to have documented the amount of carbon lost from each pool for each scenario (perhaps arranged somewhat like Table 2) reported in the supplementary information. This would help to support the text on the contribution of deep deposits on pages 16617 and 16618.

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To better support our conclusions we now address the issue of individual pool contributions by showing the amounts of carbon lost from each pool under all RCP scenarios (as suggested by the reviewer) in figures S2 and S3 of the supplement. We also added a discussion of the individual carbon contributions in more detail in section 3.3 of the revised manuscript.

6) The need to completely revise the discussion: I found that the discussion largely repeated what had already been stated in either the results or the limitations subsection of the methods.

We have re-structured the "Model results" and "Discussion and conclusion" sections.

What I found missing were two issues: (1) how does this study compare with the first version of the model published in 2012,

We now discuss the differences in simulated carbon fluxes and in the inferred temperature feedback compared to our previous study in the revised manuscript (section 3.4).

and (2) how does this study contrast with that of Gao et al (2013).

We now also discuss in detail the differences in approach and conclusions compared to Gao et al. (2013) in section 4 (page 21) of the revised manuscript.

For the RCP 8.5 scenario, the previous study had lower C losses through 2100, but higher C losses through 2300. However, the estimated additional warming through 2100 and 2300 was higher in the previous study than in this study. I recognize that different model changes besides the additional pools/processes probably explain this paradox. But the differences at least need to be discussed, and the control simulations I've suggested above will help sort out the issues of the relative importance of deep carbon vs. thermokarst transitions. With respect to the comparison to Gao et al. (2013), I think it is quite important to identify the differences in approach as well as conclusions.

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As mentioned above, we now discuss in detail the differences in approach and conclusions.

Specific Comments Page 16600, line 23: Change "the mid of" to "the middle of". Page 16600, line 25: Change "accounted for" to "taken into account" (don't end with a preposition. Page 16601, line 3: Change "amounts about" to "amounts to about".

Modified accordingly.

Page 16602, lines 15-18: It is not clear what is meant by "mineral" vs. "organic". My first reaction in reading this sentence was that mineral soils, like yedoma, tend to have larger ice content than peatlands when considering the entire profile. Need to revise the sentence so that it makes sense to the reader at this point in the manuscript.

We have modified the corresponding section to make clearer the differences between mineral and organic soils.

Page 16604, line 7: delete "in order" – just extra words that are not needed. Page 16604, line 10: Change "for abrupt thaw processes" to "for some abrupt thaw processes". Page 16604, lines 16 and 17: Many of the models that consider permafrost carbon with depth are considering methane now, so I don't think it is fair to say that methane is neglected in these suites of models. Page 16604, line 18: Change "not accounted for, although first modelling" to "not taken into account, although first-order modelling". Page 16605, line 21: Change "Our proceeding" to "Our analysis". Page 16605, line 23: Change "identifying" to "identification of". Page 16605, line 24: Change "for shaping" to "in affecting".

Modified accordingly.

Page 16606, line 10: Define what you mean by mineral and organic surface pools.

We now refer to the subsequent section of the manuscript where pools are defined. Further, we added a "terminology and definitions section" in the supplement.

Page 16606, line 12: Change "By taberal deposits we understand" to "We define taberal deposits as".

Modified accordingly.

Page 16609, line 12: Change "frozen grounds" to "frozen ground". Page 16609, line 25: Change "who are" to "which". Page 16613, line 10: Change "mid of" to "middle of". Page 16614, line 2: End of first sentence needs a period.

Modified accordingly.

Page 16614, lines 4-9: See my general comments on this issue – this doesn't make sense to me. There has already been strong surface warming in the southern permafrost zone, and thaw depths in lakes are generally thicker than they are in the continuous permafrost zone. So – how could the thaw depths in lakes of the continuous permafrost zone warm up more than the current thaw depths in the southern permafrost zone (especially under an RCP 2.6 scenario). In my opinion, something is seriously wrong with the physics in the model.

See our comments above (point 3).

Page 16617, line 18: Change "per-industrial" to "pre-industrial". Page 16618, line 26: Shouldn't you cite Figure 5 and Table 2 at the end of this sentence. I don't think that Figure 5 is cited in the manuscript, at least not in section 3.4 where it should be cited. Page 16619, line 2: Change "Despite of methane release" to "Despite methane release". Page 16620,line 18: Change "carbon can be released as" to "carbon was released as". Page 16620, line 20: Change "can reach 87" to "reached 87". Page 16620, line 22: Change "Modelling studies estimated" to "Other modelling studies have estimated". Page 16622, line 19: Change "Despite of assuming" to "Despite assuming".

Modified accordingly.

Interactive comment on Biogeosciences Discuss., 11, 16599, 2014.