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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.

Anonymous Referee #1

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Overall Evaluation This manuscript presents the results of a study to make progress in representing how the consideration of additional pools/processes may influence the permafrost carbon feedback. This work builds upon the model published by Schneider von Deimling et al. (2012) in Biogeosciences. The new version of the model includes explicit representation of refrozen thermokarst and yedoma regions in addition to the “mineral” and “organic (referred to as peatlands in the 2012 paper)” areas considered in the original model. The additional pools considered are deep carbon pools (deeper than 3 m) in Yedoma soils and refrozen thermokarst soils. The new version of the

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model also considers changes in the area (pool size) of thermokarst lakes and wetlands in response to future changes in temperature (only wetlands were considered in the original version of the model, and wetland area was static in that version).

In general, I really like the design of this study, as it makes some stepwise progress towards the consideration of additional pools/processes in the permafrost carbon feedback. However, there are some issues/short-comings in the methods, analysis, and discussion that should be addressed in a revision: (1) better description/justification of what segregates mineral vs. organic pools, (2) a better description of the transitions (changes in area/pool size) involving thermokarst lakes and wetlands, (3) an improved justification for the substantial increase in the depth of thaw in thermokarst lakes in response to future changes in climate, (4) the need to run a control simulation with transitions, (5) the need to better report the amount of carbon from lost from each pool, (6) the need to completely revise the discussion. Below I explain my concerns with these issues and finish off the review with specific comments.

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?

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

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to the C dynamics of that landscape type depending on depth/latitude band?

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 modeling 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.

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.

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.

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. What I found missing were two issues: (1) how does this study compare with the first version of the model published in 2012, and (2) how does this study contrast with that of Gao et al (2013). For the RCP 8.5 scenario, the previous study had lower C losses through 2100, but higher C losses through 2300. However, the

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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.

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”. 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. 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”. Page 16606, line 10: Define what you mean by mineral and organic surface pools. Page 16606, line 12: Change “By taberal deposits we understand” to “We define taberal deposits as”. 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. 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

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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. 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”.

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