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Interactive comment on "Modelled present and future thaw lake area expansion/contraction trends throughout the continuous permafrost zone" *by* Y. Mi et al.

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General comments:

Comments: The paper by Mi et al., titled "Modelled present and future thaw area expansion/contraction trends throughout the continuous permafrost zone" applies a stochastic modelling approach to explore trends in the growth and drainage of thermokarst lakes. The topic is of broad relevance and the modelling provides a method to explore the landscape evolution of lake-rich permafrost environments. Modelling thaw lake evolution at a broad-scale is novel and has potential to advance scientific understanding of the impacts of permafrost thaw

on Arctic landscapes and feedbacks associated with carbon flux.

The paper requires significant editorial attention and lacks sufficient background information to warrant publication in its current form. The scientific quality of the paper requires attention prior to publication. In my view there are several model limitations that are not adequately discussed in the manuscript. It is important to rigorously address these to help readers understand the applicability and limitations of this study.

In my view, the mechanics of the model are not sufficiently explained here. Given that the modelling approach is relatively new I would like the Authors to provide a more rigorous account of how the model works rather than directing readers to another publication. This would allow for some review discussion on the physical basis for the model, whether modelling assumptions reflect conditions which exist in the natural environment (at the field study sites), or whether the relevant controls that can explain the behavior of thaw lakes in different geographical environments are accounted for in this approach.

The Authors chose to model thaw lake processes for different Arctic sites. I find it problematic that there is very little information provided on the physical environments that characterize these different sites other than mean ice content and climate. Landscape factors such as topography, surficial geology, ground temperatures, spatial variation in ice content, would seem to be fundamental controls on the rates and nature of thaw lake processes. It is not clear how these are integrated into the model. The paper would benefit greatly from a section on model limitations. This is very important because as written the study purports to "reproduce recent thaw lake dynamics on four representative Arctic sites". It is not clear that the paper does this.

Validation of the results also requires some clarification. The Authors select four sites for their modelling, and results are rationalized as being realistic because

they fall within the (very large) range of empirical observations made in studies from across the circumpolar North. To make such an assertion given that the Authors have chosen to simulate particular areas but cite a broad range of field based observations as a validation of the model results is rather tenuous. I recommend major revision prior to publication. I have provided several recommendations for the Authors to consider when revising their manuscript.

Reply: The referee states that our paper lacks sufficient background information to warrant publication in its current form. The lack of background information specifically pertains to the limitations and the description of our model, background information on the sites that we studied, and validation of our model.

We agree that the model limitations need to be discussed more prominently. The model description section has be extended strongly to include the modelled processes that are necessary for understanding the limitations of our modelling approach and the simulation results. However we will not repeat the full model description here since this has been adequately published in our first paper on the model (Van Huissteden et al., 2011). We also extended the background information on the sites that we studied insofar this information is available (which is not always the case, in particular for the Russian areas).

We also agree that the model-data comparison could be improved considerably. We have improved this by including additional data on lake expansion, including an extensive Russian remote sensing study on lake expansion in a large number of sampling areas by Kravtsova et al. (2009). Some of the areas studied by Kravtsova et al. overlap or are situated close to our study areas. However, it should be realized that validation of the model is difficult, and is not solved by testing the model against historical air photos or remote sensing data (remark on P3906, L6-L12). Studies on lake area change use remote sensing material of various spatial and temporal resolutions. Low spatial resolution imagery may miss the increase in small lake and pond area and the retreat of lake banks, and will 'see' only the large lake area changes by lake drainage. Moreover,

most of these studies are snapshots in time, which may be influenced by inter-annual variability in water balance and river discharge (e.g. Plug et al., 2006) and the incidental, infrequent drainage of large lakes (e.g. Jones et al., 2011; Kravtsova et al., 2009). We have paid attention to these caveats in the Discussion section of our paper. In addition we have added a section on the effects of the model limitations, as suggested by the referee.

We also have carefully processed the editorial and spelling remarks by the referee, for those parts of the text that have not been changed considerably.

Further remarks by the referee:

Abstract, The abstract requires editorial attention. Several statements made in this section require further development. Critical context that summarizes the processes of thaw lake development and physical controls on these processes should be at least mentioned here. It would be useful to explain some of the geomorphic, geological and hydrological factors that contribute to variation in lake response to permafrost warming/thaw.

P3604 L4: Clarify why a drained lake basin is a carbon sink due to "sedimentation". Are the Authors referring to processes related to organic accumulation and permafrost aggradation?

P3604 L5-8: Editorial modification required.

P3604 L10: Editorial modification – change "increases" to "increase"

P3604 L17-20: The sentences here require editorial attention. How do the Authors know they have reproduced recent thaw lake dynamics at the four study sites? Is there any field evidence provided to support this? It is somewhat misleading to state that the simulations are comparable with data. This implies that the modelling was validated by field data from the sites, however I believe that the Authors are referring to rates of change from empirical studies from around

the circumpolar north which show that lake responses have been variable. The field data in fact indicates significant regional variability that is likely driven in part by differences in permafrost, surficial geology, topography and other environmental variables. The last sentence requires additional words of clarification.

Response: Abstract, P3604 L4, L5-8, P3604 L17-20: Text has been changed following the suggestions of the referee. See also above.

P3605 L17: add and "s" on "system"

P3605 L18: "permafrost will aggrade" is more appropriate

P3605: Geology, topography and permafrost conditions (ice and temperature) control thaw lake processes. The Authors should show some appreciation for this in the background section.

Response: Text has been changed. A paragraph on the relation between ground ice volume and geology has been added.

P3605 L29: See Marsh et al., 2009 for process oriented perspective on lake drainage.

Pg 3606 L2: The process that will dominate are likely related to landscape context and will vary from one region to another.

Response: The text of this paragraph has been changed to accommodate these remarks.

P3606: Why does the model assume linear relations between lake expansion and climate? Some rationale or clarification behind this statement would be useful because one should assume that due to latent heat effects and phase change that relations between climate warming and permafrost thaw would not be linear. It would be useful to explain what a linear relation between thaw lake expansion and ice content means.

Response: See added text on the model description. Indeed, on a smaller scale (e.g. soil column, lake bank) the relation between climate warming and permafrost is not linear and needs modelling using surface energy balance, vegetation cover heat transfer, and soil thermal properties. However, on a large landscape scale, which is the scope of our model, this detailed modelling is not feasible and a more simplified relation between climate change and the amount of ground ice thaw needs to be defined. The linearity of this relation is a first approach.

P3606: Is the modelled data compared with field observation? If it is, the method of comparison between model performance and reality must be explained.

Response: See added text in the Results section, under 'Model-data comparison'.

P3607: Clarify what this means – "Stochastic processes in the model are introduced in the selection of ice-rich grid cells where new lakes are created, and the incidence of lake drainage." Is this process related in any way to the distribution of ground ice over the modelled surface? Based on the description of factors contributing to lake drainage (distance of lake shore to drainage system and precipitation) the Authors should clearly explain how lakes drain in the natural environment. What is the rationale/physical basis, with supporting references that justify the model assumptions?

Response: See added text in Section 2.1, with model description and a short justification of the assumptions in the model. We added a justification for the stochastic components. The distribution of ground ice over the modelled surface can be prescribed but may vary between grid cells according a uniform random distribution within a specified range. For a detailed description of the rationale and model assumptions we refer to the original article describing the model for the first time, Van Huissteden et al., 2011. See also answers to the comments of referee 3 below.

Remarks on simulation setup:

If the simulations aim to reproduce region specific conditions, then appropriate discussions on physical, permafrost, climate, hydrological conditions in those regions should be provided. In this case the reader is not provided with necessary background that describes the modelled environments. This seems critical because it is well known that physical characteristics of permafrost terrain influence the nature and trajectory of landscape response to disturbance or climate perturbation.

Response: Text on the physiography of the test regions has been added in section 2.2.

Clarify how the model treats spatial distribution of ground ice across the landscape? Clarify how topography is treated in the model? It would seem that topographic relief would have an important influence on the rates of lake expansion. Are all of these regions characterized by similar topography? What are the surficial deposits in the respective study regions?

Response: Text on the physiography of the test regions has been added in section 2.2. Text on how the model treats distribution of ground ice and topography is added in section 2.1. As explained there, the model assumes flat topography; topography is not modelled in detail. All regions are characterized by similar topography as described in section 2.2.

P3609 L6-12: There are environments where data can be extracted from historical air photos on rates of lake enlargement and on drainage rates. It would seem prudent to test the model against some sort of comparable data derived from either historical air photos or remote sensing imagery for the study area. What was the magnitude of change found by Labrecque et al 2009?

Response: Text has been adapted, magnitude of change included.

P3609 L13-27: The contrasting response of thermokarst lake expansion and

drainage in the cited literature very likely reflects the importance of landscape context (nature and ice content of permafrost, ground temperatures, surficial materials, physiography etc.). How does the modelling approach deal with these controls on lake expansion or drainage? This should be considered when explaining the modelled results.

Response: See the added section with model description. The main landscape context is ice content, which determines – next to climate parameters – the rate of new formation of lakes and lake expansion.

P3610 L9: What drives the differences in simulation results between the respective environments?

Response: See added text in the discussion section, under 'Model-data comparison'

P3610 L22 to P3611 L3: This section requires reworking due to several issues of writing. For example, I don't think that "contract" is an appropriate verb to describe a decrease in thaw lake size. The word model is used 3 times in one sentence.

Response: Style issues have been addressed in this paragraph.

P3611 L1 – This line implies that model performance was evaluated, which I don't believe it has been. While the model used a realistic lake and fluvial system configuration, it is difficult to understand how other landscape factors are integrated into the simulations. The Authors must seriously consider how the modelling results are informing us about the respective behaviour of the respective environments.

Response: See added text in section 2.1 and the discussion section, under 'Model-data comparison'. We do not quite understand why the model should inform us on the respective behaviour of the respective environments. It should be the other way round, the behaviour of the environment should inform us whether the behaviour of the model

is correct given the range of environmental conditions modelled.

P3611 L10: Remove the word "depth". The sentence should read "and the thickening of the active layer, the, .." The process being described here is unclear and required better explanation and citation of field based literature. I assume the Authors are referring to water flow through a thickening active layer which may result in drainage. There are also documented examples of lakes disappearing due to subsurface conduits resulting in drainage of lakes to the groundwater system. Again, the specific processes that operate in a particular area will vary as a function of lake morphology, ground temperatures, permafrost thickness and surficial geology.

Response: See adapted text in the discussion section, under 'Model-data comparison'

P3611 L21 to P3612 L2: A more rigorous discussion about why the different climate scenarios produce such different results would be useful here. For example, why does the model not perform realistically as the Authors suggest when the MIROC simulation is applied. The discussion that links modelled results to mechanisms of change is quite weak in this section.

Response: See adapted text in the discussion section, under 'Future climate simulations' and in the new section 'Model limitations' the limitations of the model are extensively discussed.

Conclusions:

Authors state that the model captures realistic thaw lake growth and drainage cycles, but published literature indicates a wide range of variability between studies. This is most likely attributed to inherent landscape factors which characterize different regions and influence processes of thaw lake expansion and drainage. New knowledge that will assist in understanding landscape responses at intermediate scales presumably requires the factors that influence processes

of thaw lake expansion and drainage to be integrated into the model. This seems to be an important limitation of the model which isn't adequately addressed by the Authors in current version of the manuscript.

Response: The effects of variations in landscape factors have been included in the new Model-data comparison section. We added an extensive discussion on the model limitations. We agree with the referee that new knowledge on landscape responses needs to be integrated into the model (which already was stated in the last paragraph of the discussion section). We have adapted the conclusion section to state this more firmly.

Interactive comment on The Cryosphere Discuss., 8, 3603, 2014.