

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

Y. Mi et al.

y.mi@vu.nl

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General comments:

Comments: Mi et al. apply a stochastic model, THAWLAKE, to describe thermokarst lake dynamics for four different locations in the Arctic that vary by their ground-ice content for the period 1963 to 2012. The authors indicate that model simulations over this period are in agreement with remote sensing data for the four locations. This then provides the impetus for modeling future thermokarst lake dynamics out to 2100 using a series of GCMs. The authors conclude that lake drainage will be the dominant landscape-scale change mech-

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anism operating at the four sites in the future.

While this manuscript attempts to fill some critical gaps in our understanding of thermokarst lake dynamics in the Arctic, it is not publishable in its current form. It is difficult to determine how the findings of this study differ or build upon those presented by van Huissteden et al. (2011), the paper in which the THAWLAKE model was introduced and applied to a fictitious landscape. There are several places in the manuscript submission where Mi et al. compare their study to this 2011 paper:

1. Page 3609, lines 6-7: “results for all four study regions are similar to those of van Huissteden”

2. Page 3611, lines 5-6: “results are similar to those of modeled by van Huissteden”

3. Page 3611, lines 11-12: “model simulations also show a similar pattern of lake change as modeled by van Huissteden”

4. Page 3612, lines 4-5: “an effect that was also noticed by van Huissteden”

5. Page 3612, lines 28-29: when discussing why Mi et al. did not translate thermokarst lake dynamics into CH₄ fluxes....”there are no GHG emission measurements that would support a better estimate than of calculated by van Huissteden”

Mi et al. attempt to distinguish their findings from those of van Huissteden by focusing on four different locations in the Arctic and making measurements of water body surface area and drainage density derived from imagery available in Google Earth (as opposed to a fictitious landscape). While this approach sounds good on paper, the data for this analysis are not presented and it only takes into account one aspect of the model simulations, the “modern” snapshot of the landscape configuration. The model is then deemed to provide realistic results

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based on change detection analysis from different Arctic locations (other than those selected by Mi et al.) for validation. Instead of this cursory comparison, Mi et al. should conduct a change detection analysis for their four study sites to serve as validation for their model results. This should be possible given that there is likely historic imagery dating to the early portion of the time domain for sites in Siberia (Grosse reference on CORONA imagery) as well as in Alaska (Hinkel et al. 2007). In addition, the authors need to double check the date of the imagery used for their “modern” day landscape configuration. What are the specific dates for the Landsat image acquisitions? Knowing the exact data of image acquisition is seemingly critical for validation of model output from 1963 to 2012 as well as information on how the remotely sensed imagery was actually used in this study.

The lack of a description of the THAWLAKE model is another limitation of the Mi et al. submission. The authors state, that “a full description is given by van Huissteden et al.” on page 3606, line 22. Again, this makes it difficult to see what Mi et al. have done here that builds upon the work of van Huissteden et al. This aside, a better explanation of the assumptions and limitations associated with THAWLAKE are required. For example, the model assumes that thermokarst lake expansion is directly linked to climate (T and P) and ground-ice content. However, there are no references provided for this assumption. In addition, Mi et al., do not appear to take into account lake depth, permafrost temperature, ground-ice distribution, lake expansion mechanism, or topography. A discussion as to why these important parameters were ignored is warranted. In addition, Mi et al. need to compare and contrast THAWLAKE to other thermokarst lake modeling efforts (for instance - Plug, West, and Kessler). Here are a few other locations where more information is needed:

1. Why do the authors use the dry and cold Pleistocene glacial reference climate (Page 3607, line 1) when focused on modeling the period 1963 to 2012?

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2. In section 2.2, Mi et al., provide information on ground-ice content at their four study sites by providing ranges based on field measurements but they do not indicate what they actually used in their simulation set-up.

3. The authors state that they used climate data from NOAA for the climate forcing over the historic period but they do not provide the actual locations that were used. Mi et al. need to include more information on their model, assumptions, and data input than has been provided instead of referring those interested in thermokarst lake dynamics to the work published by van Huissteden et al. (2011).

Figures 3-6 could be improved upon. Since THAWLAKE produces two-dimensional model output it would be useful for the authors to take advantage of this and provide change-scape maps over the time series (hindcast – forecast) instead of simply providing line graph plots for each of the four study sites. The manuscript would also benefit from a targeted editorial review as a courtesy to reviewers/readers and to ensure that the proper messages are being conveyed. A few of these instances are pointed out below in the detailed comments section.

Reply: The referee values our application of the THAWLAKE model because it is the first time that the model is tested against real-life lowland permafrost landscapes, instead the fictitious landscape in the paper in which the model was introduced. However, the referee objects that “the data for this analysis are not presented and it only takes into account one aspect of the model simulations, the “modern” snapshot of the landscape configuration”, and that the model is then deemed to provide realistic results based on change detection analysis from different Arctic locations (other than those selected by Mi et al.) for validation. The referee then proposes that we should conduct a change detection analysis for their four study sites to serve as validation for their model results.

We agree that in the first version of our paper the validation of the model was based on too few data. However we do not agree that a full scale change detection analysis

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for all four sites. The referee appears to underestimate the task of obtaining an accurate change detection analysis based on historic images, of which the resolution and antecedent data (e.g. yearly water balance and flooding variability) is often not sufficient to determine lake expansion rates accurately, and changes in the large number of smaller lakes and ponds. This would have been a study deserving a separate paper. Rather, we preferred to correct this by drawing in more site studies, amongst which a valuable study by Kravtsova et al. (2009) who studied a large number of sites throughout Russia. This study also covers two of our study sites (see also reply to referee 1). We have added a discussion on the caveats of lake change studies for data-model comparison. In the caption of figure 1, the dates of the images on which the analysis was based had been added.

The referee considers the lack of a description of the model as another drawback of our paper, states that we refer too often to Van Huissteden et al., in which the model has been described extensively, including a discussion of the underlying assumptions. We agree that the paper would benefit from a concise description of the model, insofar it allows a discussion of its behaviour in the model tests described here. Therefore a short description of the model has been added to the methods section (see also response to referee 1). A comparison with the models of West and Plug and Kessler has been added to the introduction section.

Further remarks to the referee:

1. Why do the authors use the dry and cold Pleistocene glacial reference climate (Page 3607, line 1) when focused on modelling the period 1963 to 2012?

Response: In the model a reference climate is needed in which thaw lake initiation is deemed to be negligible. A further justification has been added to the model description. See also response to referee 1.

2. In section 2.2, Mi et al., provide information on ground-ice content at their four study sites by providing ranges based on field measurements but they do not indicate what

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they actually used in their simulation set-up.

Response: the ranges based on field measurements have been used in the model. The distribution of ground ice over the modelled surface varies between grid cells according to a uniform random distribution within a specified range.

3. The authors state that they used climate data from NOAA for the climate forcing over the historic period but they do not provide the actual locations that were used.

Response: the closest possible weather station was used, and the station locations have been added to the text.

Figures 3-6 could be improved upon. Since THAWLAKE produces two-dimensional model output it would be useful for the authors to take advantage of this and provide change-scape maps over the time series (hind cast – forecast) instead of simply providing line graph plots for each of the four study sites.

Response: Forecast – hindcast maps may illustrate the simulated lake configuration, but since this is a stochastic model, the hindcast maps would be different for every model run and would not convey much essential information. The line graphs, indicating the trend in lake area development are more informative for our data-model comparison, which focuses on these trends.

Example maps showing the model output have been added as a new figure, to show start and final result of the simulations.

Detailed edits, comments, suggestions:

The authors should consider changing “thaw lake” to “thermokarst lake” throughout the manuscript. See van Everdingen, glossary of permafrost terminology.

Response: Although ‘thermokarst lake’ is the official permafrost terminology, the term thermokarst is a very awkward one, since ‘karst’ suggests chemical dissolution, while

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a phase change (thaw) is the true physical process behind the lake formation. “Thaw lake” has been an accepted and well understood term to describe these lakes in the past, and is still being used by various authors on the subject. We prefer “thaw lake”.

The abstract needs to be reworked to provide more useful information pertaining to the study at hand. There is currently too much introductory material in this section. The authors state here that model simulations are comparable with data on thermokarst lake cycles. This statement is inaccurate as the reported timing associated with thermokarst lake cycles are on the order of 1000s of yrs and not 10s of yrs.

Response: Some of the introductory material has been deleted. The abstract has been thoroughly modified, also following suggestions of referee 3. The term ‘cycles’ has been removed.

The references provided in the second sentence in the introduction do not appear to describe the conditions that occur in Siberia and Alaska as indicated in the first sentence.

Response: ‘Alaska’ changed in the wider ‘Arctic North America’.

Page 3605, line 29: Is there evidence for vertical conduits of drainage in continuous permafrost regions?

Response: Reworded to ‘surface and subsurface conduits’.

Page 3606, line 13: Please explain how the four sites are distributed along a climatic gradient.

Response: Climate description added.

Page 3607, line 1: Why is the model referenced to a Pleistocene climate?

Response: See answers to referee 1, and the model description.

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Page 3608, line 6: lake expansion and drainage?

Response: Reworded to ‘lake area change’.

Page 3608, line 25: The model does not simulate thermokarst lake drainage cycles over the time period at hand. A cycle would include initiation, growth, drainage, and initiation. It appears like the model just captures a portion of this.

Response: Reworded, ‘cycle’ removed.

Page 3609, lines 13-27: This section does not provide validation to the model results. Mi et al. need to take more care here and provide change detection information for their four study sites.

Response: As explained in the response to referee 1, this section has been thoroughly rewritten and based on more data.

Page 3613, lines 3-9: This paragraph should be removed unless more information on the model limitations and assumption are provided. It is simply impossible to determine if THAWLAKE provides a functional framework that describes mechanistic processes when none of this information has been provided.

Response: As explained in the response to referee 1, information on the model structure has been added, and a new section has been added to the discussion section, discussing the model limitations.

Page 3613, line 14: The model does not appear to capture thermokarst lake cyclicity, suggest rewording.

Response: Reworded.

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

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