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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.

Anonymous Referee #2

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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 mechanism 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 pre-

sented 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 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 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

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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?

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).

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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.

Detailed edits, comments, suggestions:

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

- 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.

- 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.

- Page 3605, line 7: sentence is awkward as written.

- Page 3605, line 15: change icy to ice-rich

- Page 3605, lines 18-21: sentence is awkward as written.

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

- Page 3606, line 3: sentence is awkward as written.

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

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

- Page 3608, line 6: lake expansion and drainage?

- Page 3608, line25: 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.

- 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.

- 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.

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

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