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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: Readers of this review should note that the reviewer's field experience is limited to North America. None of what follows should be interpreted as an authoritative statement regarding the context of thaw lakes and thaw lake development in Eurasia.

Stochastic models describe environmental systems from two perspectives. First, they may simulate systems that have genuinely stochastic properties and behaviour. Second, they may represent a fiction of convenience. This latter ap-

proach may be adopted for several reasons, for example, a multivariate system may be more efficiently simulated as stochastic behaviour than as the deterministic behaviour of many, interrelated variables. Similarly, precise local conditions may not be known, and so the variability of field contexts may be simulated by stochastic functions.

In cases where a stochastic approach is being used to describe the outcome of a deterministic system, it is critical to examine the assumptions used to develop the stochastic variables. Commonly, the physical basis of the variable may be established, but possible results are specified over a range of outcomes with associated probabilities. In other cases, dummy variables may be used, which have limited physical basis, but significant correlation with the phenomena under investigation.

The paper under review requires examination in this context, because its title is breathtaking. It aims to assess present and future thaw lake expansion and contraction THROUGHOUT the continuous permafrost zone. In other words, the paper addresses the vast majority of the circumpolar region, through a range of sedimentary and geologic settings, a range of climatic regimes, and an area with a range of Quaternary histories. The climates, just in North America, range from the Arctic maritime environment of northern Alaska, through to conditions south of Hudson Bay. The sedimentary environments range from unglaciated terrain, of Beringia, through the land covered by the Cordilleran and Laurentide ice sheets, and the multitude of settings created during deglaciation. Of particular interest in this case is the legacy of ground ice residual from the ice cover, and the frost susceptibility of glacial soils that have created environments favourable to the presence of ground ice. Thaw lakes do not exist where there is no ground ice, because subsidence is required for impoundment of water.

The editors of the journal should also consider carefully the recognition by Lachenbruch et al. (1962) that in permafrost regions, the thermal regime of lakes

represent the greatest local departure from conditions determined by climate. Thaw lakes expand because permafrost in their vicinity is no longer sustainable, due to the thermal effect of the water body. In some cases the expansion is enhanced by hydrodynamic effects. Lake development is not sensitive to climate, because lake temperatures vary little over a wide climate range. The statement is based on the small amount of data that is available on this topic, mostly from northwest Canada and Alaska, but throughout the permafrost zones of this region.

Thaw lakes in continuous permafrost are in many cases in equilibrium with their environment. Numerous lakes on the North Slope of Alaska, and in the Canadian western Arctic, have wide littoral terraces, beneath which permafrost is preserved. The area of the lake beneath which the talik develops is restricted to a central portion of the lake. Many of these lakes do not expand, unless terrestrial effects, such as thaw slumps, affect the shoreline. Drainage of such thaw lakes commonly occurs catastrophically. Catastrophic drainage has been described in some detail by Mackay (1988, 1992) (not Mackay et al. 1992, as cited in this paper), and the potential for drainage rates to increase or decrease has been considered. Of key importance to evaluation of the paper under discussion is the observation that few lakes drain as a result of proximity to a river. Drainage occurs by overtopping of an interfluve, normally during snow melt but sometimes after intense summer rain. Lakes commonly drain in isolation, and in the Canadian western Arctic, Mackay (1992) reports that about one or two have drained on average each year for the last 4000 years. Mackay did not report an increase in lake drainage following the Little Ice Age when the regional climate warmed significantly. In some cases, the discharge from a drained lake fills another, which in turn drains. The factors causing the second to drain are then quite different from those that simulated the first drainage. The second is not stimulated by climate, just by receipt of drainage water from above.

The paragraphs above indicate why I believe the editors should be extremely cautious about this paper. In short, I am not convinced that the variables selected to drive the simulation of thermokarst development are sufficiently physically associated with thaw lake development to be informative.

Reply: We agree with the introduction of the referee on the reasons for using stochastic models. Our model would fall in the referee's category 'Stochastic model by convenience', since we have to deal with a system that can be described as the deterministic behaviour of many interrelated variables, while the precise local conditions may not be known, in particular not on the scale at which we want to study the system. Therefore the variability of these conditions may be simulated by stochastic functions.

As the referee puts it, the title of our paper is 'breathtaking', and clearly the referee in particular has objections against the word 'throughout'. The reason for this 'throughout' in the title is, that we have selected our model test sites in such a way that they represent the representative and well-studied thaw lake areas on the Eurasian and North-American continent. However, we see the point of the referee, it can indeed be taken as representing the entire permafrost zone in all its physiographic variability. Moreover, the scope of our model is sedimentary lowlands with minor relief. Therefore we change the title to: "Modelling present and future thaw lake area expansion/contraction trends in representative sedimentary lowland permafrost areas"

The referee is not convinced that the variables selected to drive the simulation of thermokarst development are sufficiently physically associated with thaw lake development to be informative, and underpins this with several arguments, partly based on her/his experience with North American thaw lakes. From the discussion that follows we understand that the referee doubts the link between the major categories of thaw lake processes (initiation, expansion, drainage) and climate.

First, based on the paper by Lachenbruch (1962), the referee argues that the existence of a lake is a local departure of the thermal regime of the permafrost as governed by climatic conditions. Thaw lakes expand because the permafrost is no longer sustainable, not because of climate, but due to the thermal properties of the water body. Then the referee proceeds by stating that lake development is not sensitive to climate, because lake temperatures vary little over a wide climate range. Unfortunately we could not trace the very old publication of Lachenbruch (1962) but a similar description of thaw lakes is found in Lachenbruch and Marshall (1969) and the review by Gross et al. (2013).

It may be true that because of this small variation in lake temperature the link between lake expansion and climate is poorly defined. First, one has to ask then what is meant by lake temperature since this may vary strongly throughout the water body and over time. Is this an average over time, depth, and lake area, surface temperature in summer, bottom temperature? We understand that the referee wants to point out that lake expansion may be an autonomous, climate-independent process due to the transfer of heat from the lake water body to the surrounding permafrost. However, this autonomous lake expansion does not exclude a climate-related component, as lake expansion is the result of multiple geomorphological processes (wave erosion, thermal erosion, mass wasting of banks, redistribution of sediments) which in turn may be enhanced by temperature and precipitation. On the other hand, this autonomous component of lake expansion may be small, since the referee states for instance (on page C2880) that the thaw lakes in Alaska are essentially stable.

The referee also states that in some cases the expansion is enhanced by hydrodynamic effects. Probably the referee means the effects of surface and bottom currents, waves, thermal erosion etc. To our experience (from thermokarst lakes in the yedoma areas of northern Siberia), these are the effects that may link lateral expansion of lakes to climate. For instance, warm days in northern Siberia cause the surface layer of lakes to warm, together with wind-driven surface currents this enhances thermal erosion of lake banks, often resulting in large sediment plumes emanating from the banks. We also measured the rate of thaw slumping and erosion on a thaw lake bank (not cited

in our paper because data analysis is still in progress). These measurements show that movement rate of the bank material is strongly enhanced on warm or rainy days. In a dry and cool summer, mass movement on the same location came to a complete standstill. Concluding, there are several processes and combinations of these processes that link erosion of lake banks to summer temperature, precipitation and eventually the length of the summer season. In addition, the creation of new ponds has been linked to climate change (e.g. Jorgenson et al., 2006). We added references to the paper to support our assumption that lake initiation and expansion may be related to climate. The wide littoral terraces of the thaw lakes in North America appear less common in the thaw lakes that we have seen in Siberia. To accommodate the remarks of the referee, we have re-run the model with a lower value for the sensitivity of lake expansion to summer temperature (parameter b in equation 2, new version of the paper). This has been reduced by a factor 8.

Next the referee discusses lake drainage, again reasoning that there is no link between lake drainage and climate. However, the referee mentions a potential link with climate when it is stated that “Drainage occurs by overtopping of an interfluve, normally during snow melt but sometimes after intense summer rain.” In our model, the probability of lake drainage increases in years with higher precipitation, which is an implementation of this climate-related process. The fact that MacKay (1992) did not report an increase in lake drainage following the Little Ice Age, does not disprove a link with climate. The argument that there is no link with climate when two lakes drain subsequently is simply far-fetched, of course there may be causes of lake drainage other than climatic. By contrast, MacDonald et al. (2012) link both lake expansion and lake drainage to climate change (precipitation increase) based on analysis of sediment cores form a thaw lake.

There appears to be a misunderstanding of the processes in our model, when the referee declares that “few lakes drain as a result of proximity to a river”. Proximity to a river is not the main reason for drainage, but overtopping of an interfluve or drainage thresholds as mentioned by the referee. Additional mechanism often mentioned is erosion along ice wedges. However, this overtopping or erosion becomes more likely when a lower base level for erosion is nearby.

The referee concludes that he/she is “not convinced that the variables selected to drive the simulation of thermokarst development are sufficiently physically associated with thaw lake development to be informative.” We are convinced, that on a large scale, the processes of thaw lake initiation, expansion and drainage at least in part are sensitive to features of climate, because many of the physical interactions, also when studied in detail, are sensitive to higher temperatures and precipitation. For instance the overtopping of interfluves is sensitive to higher precipitation or snowmelt (as mentioned by the referee). In response to the objections of the referee against climate sensitivity of lake expansion and drainage, we have added a short discussion of this climate sensitivity to the section that describes the model. In that discussion, we have added references to support our assumption.

Detailed remarks by the referee:

p. 3604. Line 17. The statement that thaw lake cycles and expansion rates are comparable with data is taken to mean that the simulations are validated with respect to field evidence. This is not the case as far as data from Alaska are concerned, and will be addressed below.

Response: We have considerably improved our model-data comparison, as discussed above in more detail.

p. 3604, line 18. The text gives the impression that this paper is a preliminary progress report. It is an editorial matter to decide if the journal exists to publish work that can clearly be improved.

Response: This part of the abstract has been reworded. The adjective ’preliminary’ is unjustified, since a full description and test of the model already has been published

(Van Huissteden et al., 2011). Moreover, all modelling work is work in progress, and in a paper on the test of a model, it is fully justified to suggest improvements of the model.

p. 3604, line 25. The authors state that the sediments are usually supersaturated with ice. What proportion of the time are these sediments not saturated with ice? It is not at all clear what the authors are attempting to convey. It would be appropriate to cite a report of permafrost sediments in which the ice content fluctuates over time.

Response: It is unclear what the referee suggests here with ’ proportion of the time are these sediments not saturated with ice’. Ice-rich sediments have accumulated ice over time, until they thaw. No changes made to the text.

p. 3604 line 26. Mackay et al. 1992 appears to have been cited using an automatic system. There is only one author of this paper. It is Professor J. R. Mackay. The correct citation is:

Mackay, J.R. 1992. Lake stability in an ice-rich permafrost environment: examples from the western Arctic coast. In Arctic ecosystems in semi-arid regions: Implications for resource management. Robarts R.D., Bothwell M.L. (eds). Environment Canada: Saskatoon, Sask.; NHRI Symposium Series 7, 1-25.

Response: This is indeed an error in the citation, changed.

p. 3605, line 10. Citation for Lenz et al. 2013. This paper does not demonstrate cyclic behaviour of thaw lakes, in support of which it is cited. The paper documents the occurrence and existence through the Holocene of one lake. p. 3605, line 13. The authors state that thaw lakes form as a result of climate change, but provide no citations in support of this point, which is key to the argument. There are very few papers that demonstrate this, but there are some, and they should be cited here.

Response: Citation of Lenz et al., 2013 changed; papers demonstrating climate change as a cause of lake formation added.

p. 3605, line 16-17. The authors state that "As lakes grow, they often coalesce or connect with surface or subsurface drainage system and eventually become drained lake basins." As described above, drainage of lakes is the permafrost region is commonly catastrophic. Coalescence is one way in which lake levels may change, but when this occurs, the level is adjusted; it is uncommon for the lake to drain completely. Mackay (1988) described in detail how lakes drain, and this explanation is reiterated in the incorrectly cited paper Mackay (et al.) (1992). Mackay emphasized catastrophic drainage by breaching of a snow dam, or along ice wedges, in the vast majority of cases for which we have field data. Neither of these mechanisms is cited by the authors, probably because neither of these is amenable to simulation with the variables presented.

Response: Lake level adjustment after coalescing of lakes has been added to the text. Incomplete drainage has been discussed in the Model description and the Discussion sections. The list of mechanisms causing lake drainage has been added to the text. However, the model does not simulate these in detail because of the scale level at which the model operates. In the model the drainage of lakes is a stochastic process, with a higher rate of occurrence in high precipitation years.

p. 3605, line 17. Climate is but one of the variables required to re-establish permafrost in a drained lake basin. Jorgenson and Shur have also made clear the need for appropriate ecological conditions in many cases.

Response: That is correct, but the model is designed for modeling lake change in continuous permafrost where permafrost would be established irrespective of ecological conditions.

p. 3606, line 24-25. In 1962, Lachenbruch et al. indicated that lakes provide the greatest local departure of ground temperature from patterns determined by climate. Thismodelling statement was reiterated recently on p. 333 of the

excellent review of thermokarst lakes published in 2013 by Grosse et al., and can be found in several other publications. It should make the authors wonder whether their premise that there is a linear relation between lake expansion and climate variables is valid. Lake temperatures are insensitive to climate variation because (a) in winter the water body is disconnected from the atmosphere by the ice cover; (b) in summer, evaporation influences the surface energy balance. In a long-term reconstruction of lake development in central Yukon Territory, Burn and Smith (1990) indicated that the rate of growth of the thermokarst lakes they were studying was constant over a period of about 100 years.

Response: This largely has been discussed above. In the review by Grosse et al., 2013, indeed the paper by Lachenbruch et al is re-iterated, but nowhere Grosse et al. say that this precludes a relation of lake expansion with climate. Elsewhere in their review (page 338, 340) they discuss papers relating lake expansion and shrinkage with climate, and cite evidence for a relation of lake drainage with climate.

p. 3607, line 2. It is not clear why permafrost that is today at a mean annual ground temperature of -6 C or below is not stable. Pleistocene permafrost may have been stable, but much of it is stable at present.

Response: This has been discussed above, answers to referee 1 and 2.

p. 3607, line 5. Drainage in the model is a stochastic function of the distance to the drainage system. In the field, the work of Mackay has shown drainage unrelated to a stream network. All of the field data we have on lake drainage points to other variables being important; none of it has cited the distance to the drainage system, unless the coastline is considered a drainage system.

Response: In the model drainage of lakes can occur when a connection is established (by expansion of the drainage network, or by expansion of the lake). However, this is enhanced when yearly precipitation is high. Although drainage may be unrelated to a stream network, drainage by erosion enhanced by weather conditions and proximity to stream channels is common, and often a drainage channel is created, which essentially expands the stream network (examples by Grosse et al., 2013). Moreover, from basic geomorphological principles drainage that is unrelated to a stream network is less likely than drainage related to the drainage network.

p. 3608, line 22. Statistical significance of time series is a mystery to this reviewer. The time series are not samples, they are documentation of past conditions. It is not possible to resample the record and obtain different coordinates. Therefore whatever change in temperature or precipitation is observed over time is what it is.

Response: This is statistical significance of the trends in the input time series of the model, not the statistical significance of the time series itself.

p. 3608, line 21 and p. 3609, line 1. Mackay (1992) discussed lake stability and climate change in pp. 19-21 of his paper. It is not clear why the authors have decided to offer a different version of the relations between thaw lake area and climate. There are several points: (1) If thaw lakes are a disturbance, because the annual mean temperature in the lake is above 0 C, then they are expected to thaw the permafrost around them and expand, whether or not the climate is changing. In other words, what rate of expansion of lake area do the authors anticipate without climate change? How do we know that the rates cited here are anything other than what would be expected without climate change? (2) The model indicates the average lake expansion rate in Alaska is 0.15% per year. If the radius is expanding at a constant rate, due to thaw of adjacent permafrost, then the areal expansion will increase with time as the circumference of lakes increases. Is this, in fact the case? (3) The model indicates the average lake expansion rate in Alaska is 0.15% per year. However, on the same page, geomorphological the authors cite Riordan et al. (2006), who indicate no change in area; Hinkel et al. (2007) conclude the same; and Labrecque et al. (2009) in the adjacent Old Crow Flats of northwest Canada find a reduction in surface area.

Only Jorgenson et al. (2006) show an increase in area of ponds. How such varied field information can be used to state that "thaw lake cycles and expansion rates are comparable with data" as on p. 3604, line 17 is not clear. The same phrase is repeated on p. 3613, lines 14-15.

Response: Point 1 has been discussed above, with the general remarks of the referee. Point 2: The expansion is an areal expansion, by incrementally adding grid cells to the lake surface, it is not a radial expansion. The added model description in the Methods section may clarify this. Point 3: The results and discussion sections have been rewritten, based on remarks of referee 1 and 2, to provide a better model-data comparison. In this section, it is shown that our model results in the lowest expansion rates for the Alaska (Barrow) region. This expansion includes both area expansion of existing lakes, and the creation of new lakes. In agreement with Jorgenson et al. (2006), much of this lake expansion results from the creation of new ponds.

p. 3610, line 25. The authors state that "the model can model thaw lake dynamics on a landscape scale". For the case of Alaska, the data presented do not justify this statement.

Response: see point 3, previous comment.

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