The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-248-RC2, 2020

The Cryosphere Discussions

Interactive comment on "Thermokarst lake development in syngenetic ice-wedge polygon terrain in the Eastern Canadian Arctic (Bylot Island, Nunavut)" *by* Frédéric Bouchard et al.

Anonymous Referee #2

Received and published: 10 January 2020

The manuscript "Thermokarst lake development in syngenetic ice-wedge polygon terrain in the Eastern Canadian Arctic (Bylot Island, Nunavut)" presents a careful study of thermokarst lake initiation outside of its main distribution area. Both sedimentation regime and ecology are reconstructed in this highly relevant study from the Canadian high Arctic, a region that is still vastly understudied, mainly due to its remoteness and challenging accessibility. The manuscript is very well written and well structured and presents its findings in a clear and concise way. The study works without an age depth model, but that cannot always be forced, especially in Arctic thermokarst lakes. The way the authors deal with this issue may be the most honest way to present the radiocarbon dates. The dates still give a general indication of the ages of the strata.

The authors present a new conceptual model of late Holocene thermokarst lake development. This landscape type and region is indeed strongly underrepresented in the thermokarst literature. I have listed a few general comments, detailed comments and minor edits below and advise to accept this manuscript after minor revisions.

General comments

1. The discussion should focus on/refer to the actual results more obviously. Large parts of the discussion are quite general.

2. I generally like that there is a section in the discussion dedicated to the wider implications of your findings. This section does, however, need some work still. My main concern here is that you are comparing syngenetic permafrost with Yedoma (which is also syngenetic permafrost). I am not convinced that the difference lies primarily in syngenetic vs. epigenetic permafrost. It is more a question of wetlands vs. non-wetlands. Formerly glaciated terrain often develops into wetlands studded with lakes, but there are also regions which were never glaciated and are rich in lakes and wetlands, e.g. the Arctic coastal plain of Alaskan or its continuation into Canada, or generally Beringian coastal lowlands. To me, the main difference lies in minerogenic vs. organic/peat deposits. Also, I am not sure the entire terrace you are studying is homogeneous in its organic matter content. Fortier and Allard, 2004, covered two low-centred ice-wedge polygons from the terrace, in which high organic matter contents can be expected, as these are usually wetlands. Your study looks at oone particular thermokarst lake. The findings are relevant, and it is also important to place the finding in a wider context, I am just not too happy with the emphasis on the quantitative comparison.

3. It should be made clearer what is new about the conceptual model. This can be done by editing the text only.

Detailed comments

Line 33: "remarkably" sounds a bit weird in this context. Please also reconsider the

phrase "circumpolar regions“, as thermokarst lakes are strictly speaking most abundant in terrestrial Arctic lowlands.

Lines 42-44: Unglaciated ice-rich terrain is not necessarily Yedoma, it includes ice-wedge polygon peatlands and lowland thermokarst. Also, if you are categorizing lakes, you might have to be more explicit. Lakes in Yedoma terrain might still be thermokarst lakes, even if they tend to have a different morphology. Not sure this categorization is needed here.

Lines 50-51: "When thaw depth exceeds the maximum thickness of winter ice cover, [...]“ - this is ambiguous. Please rephrase.

Line 60: "drawdown“ - this might not be the most appropriate term here, especially when you are also using it for lake infilling, could use "lowering“ or "decreasing lake depth“ instead

Lines 82-83: your third objective could end with "specifically for syngenetic ice-wedge polygon terrain“ or else convince me that your conceptual model is universal

Line 99: Did glacier retreat stop for good or is it retreating again now? Also, please give a reference for the date.

Line 106: not sure "off-shore“ is the appropriate term here, it sound like way off the sea shore

Line 122: Consider indicating that this publication describes the method. It sounds like the results have already been published.

Lines 175-176: could refer to the full diatom data set in the database

Lines 192-193: Consider indicating that this is data, not a published study. It sounds like the results have already been published.

Line 228: Do you trust the age at this depth? It sounds a bit old for a depth of 10cm, and as you dated bulk sediment, how can you be sure this is not, at least in part,

relocated old material? Please comment shortly.

Line 233: Is “major taxa” a known term? If the 5% abundance in at least one sample (?) is a commonly used standard cutoff, please give a reference. I know this is done to minimize statistical issues arising from extremely low counts and too many zero entries (”not present”) in the dataset, but you could consider stating this in the text.

Line 234: “one level” – please specify what you mean here, one zone, one sample or something else?

Line 315: You could start this section with “Based on our findings on the geomorphology and palaeolimnology of. . .” to make it clear that you are talking about the new results rather than about findings from the literature. Or if it is both this study’s findings and the results of your lit review, you can say so. This would make it clearer what you have added to the knowledge on thermokarst lake evolution in the region. You can also use active voice in the statement following in line 318 (“we summarized the initial conditions . . .” or something similar).

Line 323: “must have” – I am not too happy with this absolute phrasing. Please also prove this and give references

Line 336: Give reference. Also, consider changing “high-centered polygons” to “ice-wedge polygons” in general, all types of which provide a mosaic of terrestrial and freshwater ecosystems in very close proximity of each other:

e.g. Bliss L.C. 1956. A Comparison of Plant Development in Microenvironments of Arctic and Alpine Tundras. Ecological Monographs 26, 303-337, 10.2307/1948544.

something newer:

from Siberia: De Klerk P., Teltewskoi A., Theuerkauf M. & Joosten H. 2014. Vegetation patterns, pollen deposition and distribution of non-pollen palynomorphs in an ice-wedge polygon near Kytalyk (NE Siberiawith some remarks on Arctic pollen morphology. Polar Biology, 1393-1412, 10.1007/s00300-014-1529-3.

From western Canada: Wolter J., Lantuit H., Fritz M., Macias-Fauria M., Myers-Smith I. & Herzschuh U. 2016. Vegetation composition and shrub extent on the Yukon coast, Canada, are strongly linked to ice-wedge polygon degradation. 2016, 10.3402/polar.v35.27489.

This is also found in palaeostudies using biological proxies, including diatoms: Fritz M., Wolter J., Rudaya N., Palagushkina O., Nazarova L., Obu J., Rethemeyer J., Lantuit H. & Wetterich S. 2016. Holocene ice-wedge polygon development in northern Yukon permafrost peatlands, Canada. Quaternary Science Reviews, 10.1016/j.quascirev.2016.02.008.

Line 352: Is this really typical of thermokarst lakes? See if other references state other accumulation rates, perhaps check

Biskaborn, B.K., Herzschuh, U., Bolshiyanov, D. et al. J Paleolimnol (2013) 49: 155. https://doi.org/10.1007/s10933-012-9650-1,

Klein et al., 2013 - https://doi.org/10.1016/j.palaeo.2013.09.009 or similar.

There is slightly more data on carbon accumulation rates in thermokarst lakes (e.g. Anthony, K., Zimov, S., Grosse, G.Âăet al.ÂăA shift of thermokarst lakes from carbon sources to sinks during the Holocene epoch.ÂăNatureÂă511,Âă452–456 (2014) doi:10.1038/nature13560)

Sediment accumulation in thermokarst lakes has been shown to be messy and not at all constant. It can also be fairly high. See for example :

Schleusner et al., 2015 doi:10.1111/bor.12084

or Lenz et al., 2016 https://doi.org/10.1007/s41063-016-0025-0

or Wolter et al., 2017 https://doi.org/10.1177/0959683617708441

Line 356-358: You are introducing new results here (technically). Perhaps mention this earlier in the manuscript in the appropriate sections (methods/results)? I haven't seen

it there. This is not major new data, though, so you might also keep it as it is. Also, the results section did not state clearly that an unfrozen zone could not be detected from GPR data.

Lines 372-376: Some questions from my side: Do you mean that the lake cannot become any deeper once it hits the glacio-fluvial sand because of its lower ice centent? And do you think it could also stay there without disappearing? Is it certain the lake would drain because of topography or could it also coalesce with the lake next to it? Do you think lake infilling is really an option, as accumulation rates are low and the lake might grow both laterally and vertically in the future?

Line 377: Infilling by aquatic and semiaquatic plants is, to my knowledge, more likely in smaller ice-wedge ponds than in lakes. I agree that basins usually fill up with sediments over time, but this might takes a very long time. Jorgenson and Shur, 2007, are talking about infilling ponds along the margins of drained lake basins (while large thaw lakes may form in their centers) on the Arctic Coastal Plain of Alaska. The question is the balance between accumulation and decomposition or transport out of the system (e.g. via a stream), or in this case, possibly also lake deepening through additional thaw subsidence. Lake infilling would likely be a very slow process, especially given the low sediment accumulation rates.

Line 379: This argument is not super-convincing. Lacustrine sediments normally accumulate in a lake, so that you found them does not necessarily prove terrestrialization.

Line 391: give reference(s)

405-410: You could state here for which depths the Yedoma TOC contents were calculated, so it is comparable to your findings. Generally, I think you should not extend the carbon contents of the upper 3-5 m to greater depths, as TOC generally decreases strongly below the first meter or so. Also, to compare a point measurement from organic-rich sediment in a relatively small feature in a heterogeneous landscape with averaged values for the entire Yedoma domain is a bit misleading.

Line 415: Please avoid citing articles in review, as they are not available for checking.

Lines 424-427: This could be 2 sentences. Be extra careful in your phrasing here: “the entire Yedoma complex” sounds like you mean all of the Yedoma there is (its entire area). Also, as commented above, be careful with that comparison. It is valid to say that there are landscapes in the arctic that contain more organic-rich sediments than found on average in Yedoma, but this quantifying comparison is going too far for my taste, see my general comment above. Consider citing GHG emissions from Arctic wetlands. And how do your findings relate to findings from other Arctic wetlands or other low-centered polygon fields? You could for example compare your findings to those from ice-wedge polygon wetlands in the western Canadian Arctic, i.e. on the Yukon Coastal Plain, the eastern part of which used to be glaciated, or on the Tuktoyaktuk Peninsula .

Line 439: Thermokarst lake cycles would take far longer to develop. There simply wasn’t enough time for that. The study design was thus not suitable for testing whether thermokarst lakes develop cyclically or unidirectionally.

Lines 437-438: Not all Pleistocene-age permafrost deposits are Yedoma.

Lines 441-444: “regardless of climate” – that might be a bit too much. In your next sentence you rightly state that precipitation (which is also climate) and snow distribution (which has a geomorphological component) are more important than temperature development. Stick with that.

Technical edits

line 43: replace "since“ with "in“

line 54 and elsewhere: is the "in prep.“ manuscript published now? If not, omit reference.

Line 81: omit "a“ before "syngenetic ice-wedge polygon terrain“

Line 87: developed into

Line 91: total precipitations -> a total precipitation

Line 93: perhaps better to separate this insert with commas instead of brackets

line 105: "before 3700 years ago“ - sometime before? Just before?

Line 128: not sure one can "conduct“ survey lines?

Line 198: the lake bottom

Line 218: "..., which are both dominated by peat“?

Line 254: “in average” should be “on average”

Line 268: separate the insert however by commas (was, however, strongly)

Line 269: Better use “entire” instead of “whole”?

Line 297: Perhaps better to say “During” or “At the beginning of the” late Holocene

Line 323: “found at the lake bottom”

Line 337-338: “were a significant source of latent heat to extract in autumn” – this needs some rephrasing. I do not much like the use of the word significant outside when not talking about statistical significance. And I do not quite understand the word “extract” in this context.

Line 354: the lake bottom

Line 386: water balances -> water balance

Line 392: tapping

Line 408: “presents slightly over” could be “contains more than”

Line 411: “are comparable to other circumpolar regions”?

Line 418: formerly glaciated terrain ?

Line 442: “self-enhancing”?

Figure 5: Is it possible to add ecological interpretation/groups on top of the taxa? That might help readers.

Figure 6: Is this conceptual model really new? It looks a lot like the existing models. The only immediate difference I see are the ponds forming on top of the ice wedges instead of between ice-wedge ridges. This might be because of climatic warming and subsequent ice-wedge degradation. How can you prove that the ponds were on top of the ice wedges? Is there a difference in diatom flora between intrapolygonal and interpolygonal ponds?

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-248, 2019.