We thank the two anonymous Reviewers and the Editor for their reviews of our manuscript and their useful comments. Below are point-by-point responses to all of the comments and questions. The original reviewer' comments are shown in *grey (italics, smaller font)*, and our responses are presented in black (normal font).

Reviewer #2 (Anonymous)
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.

Thank you for these positive and useful comments. We answered to all the comments (general, detailed and minor edits) made by the reviewer.

<u>General comments</u>

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

We modified and added several sentences in the discussion section, as suggested by the reviewer in his/her detailed comments. However, this comment is general and does not focus on specific sections. We would welcome any specific comment, including line numbers, about the discussion. Where exactly could we « refer to [our] actual results more obviously »?

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.

We do not argue that that the difference lies in syngenetic *vs*. epigenetic permafrost. Both our site and Yedoma regions are affected by syngenetic permafrost. See our reply to the next comment just below.

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.

From a geomorphological perspective, we chose to compare soils that have experienced similar pedogenetic and geomorphic processes, as the processes of soil organic carbon inclusion into permafrost strongly influence their concentration relative to depth (Bockheim 2007; Tarnocai et al. 2009). Since permafrost at Bylot Island developed syngenetically during the Holocene (Fortier and Allard 2004), we chose to compare it to Yedoma deposits, which also developed syngenitically but over the late Pleistocene (e.g., Schirrmeister et al. 2011; Strauss et al. 2017), allowing a comparison of sites with similar permafrost development and pedogenetic/geomorphic history. This is also justified by comparable ground-ice content (in volume) and soil bulk densities (see section 5.3 in the discussion). We are aware that this comparison might not be ideal had we wanted to compare the site with others of similar concentrations, but from a geomorphological and geocryological perspective, they are very similar and highly comparable.

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 one 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.

Gull lake is located on the highest benches of the ice-wedge polygon terrace of the valley. The ice-wedge polygons studied by Fortier and Allard (2004) are located very close (< 50 m) to Gull Lake and at the same elevation as the lake shore. These polygons showed similar sedimentation rates, ground ice, organic and sediment content. Given the close proximity between Fortier and Allard's study and Gull lake, and the geomorphological similarity between the ice-wedge polygons on this level of the terrace, we are confident that the organic and ground ice contents are similar between these two sites due to similar sedimentary conditions (eolian silt deposition), vegetation type (graminoïds and bryophytes), humidity (wetland) and climate. Several permafrost cores have been drilled in the surroundings, giving comparable values (Godin, unpublished data; Veillette, 2019; see cited references at the end of this report).

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

See our reply at the end of this report (last comment about Figure 6).

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.

We changed 'remarkably' to 'notably'.
We added the phrase 'especially in terrestrial lowlands' in that sentence.

Lines 42-44: Unglaciated ice-rich terrain is not necessarily Yedoma, it includes icewedge 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.

We agree that detailed lake categorization is not needed here. We just want to specify that 'Yedoma lakes' represent a relatively minor group of lakes, notably deeper and with a different history during the late Pleistocene, compared to the much more abundant lakes developed during the Holocene, in formerly glaciated terrain. We slightly modified the sentence, and added a new one: « However, the vast majority of thermokarst lakes across the Arctic are shallow (a few meters) and were formed in formerly glaciated terrains during the Holocene (Grosse et al., 2013). »

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

We agree that this was ambiguous. The new sentence is: « When <u>lake depth exceeds</u> the maximum thickness of winter ice cover, <u>water stays unfrozen throughout the year and</u> mean annual lake-bottom temperature remains above 0 °C, resulting in the formation of a talik (thaw bulb) underneath the lake (Burn, 2002) ».

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.

We are not using 'drawdown' to refer to lake infilling (involving inputs of sediments), but rather refer to increased evaporation, which is really a loss of water volume, thus we had to keep the expression 'drawdown' (i.e. « withdrawal of water from a reservoir »).

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.

We added the phrase « in syngenetic ice-wedge polygon terrain », as suggested.

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

Yes, glaciers are still retreating up in the valley nowadays. This has been documented by Dowdeswell et al. (2007) for the majority of glaciers on Bylot Island. Their study indicates that overall glaciers have retreated from 0.9 to 1.8 km since about 120 years ago, with most retreat occurring between 1958/1961 and 2001.

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

We assumed that the correct line number referred to is rather 116, and we changed for 'from the central zone' to clarify this point.

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

We added 'as in' in front of the reference, so it's now clearer that it refers to the method.

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

Good suggestion. We added the sentence: « The complete diatom dataset is available in open access (Pienitz et al., 2019). »

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

We modified the sentence accordingly (« raw data can be found in data repository; Fortier et al., 2019).

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.

We cannot totally rule out some remobilization of materials, as bulk sediment sample was dated (no organic debris found in that layer). However, we are confident in that age because: 1) short-term dating (^{210}Pb) of surface sediments was conducted and showed that at that depth (10cm), there was no more supported ^{210}Pb, indicating sediments older than 150 years; 2) dating of this layer was also dated in the other core (from 2014), giving the same age.

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.

In this case, the 5% relative abundance in at least one sample cutoff was not based on a statistical criterion (like, for example, for the lower weight assigned to rare species to limit their influence within multivariate statistical approaches). We rather took a practical decision to show within the limited space of the summary diatom diagram (Figure 5) only the dominant and most relevant taxa. We modified the text to make it clearer: « Among these, <u>the 15 most frequently encountered taxa</u> (species or species groups) representing more than 5 % in relative abundance in at least one sample were selected to show major ecological changes that occurred in the past (Fig. 5) ».

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

It means one sample, and we changed the text accordingly.

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).

This section 5.2 reports on our new results, while findings from the existing literature are given in the previous section 5.1 (about the Holocene history of the valley). We now start the sentence as suggested (« Based <u>on our findings</u>… »), and we also used the active voice at the beginning of the next paragraph (« <u>We</u> summarized… »).

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

This was also underlined by Reviewer #1. The reasoning is as follows:

In the sediment core from 2015, collected at ~ 4 m depth, we sampled about 0.7 m of silty peat. This unit is currently unfrozen. We know that the surrounding frozen ground of that unit contains over 50 % of ice by volume (Fortier and Allard, 2004). Hence, considering thaw settlement and consolidation, the silty peat layers found in the core must have made at least twice their current thickness when they were still frozen. That makes about 1.5-2 m thick of frozen silty peat before the lake started to form. Even if we assume that the thawing of the underlying glaciofluvial material may have caused some minor subsidence (because of a negligible excess ice content), there is still nearly 2 m of material missing (i.e. 4 m minus 1.5/2 m). Hence, we assumed there was a 1-2 m pre-existing depression.

We modified the text to make it clearer. For example, we added lake maximum depth (~ 4 m) in the sentence, and we added the following sentence: « Since this silty peat unit is about 1.5-2 m in thickness when still frozen (Fortier and Allard 2004), and since the underlying glaciofluvial unit is ice-poor (thus negligible subsidence upon thaw), there is 1-2 m elevation gap which can be explained by the presence of a preexisting depression. The latter is interpreted as a channel in the glacio-fluvial outwash underlying the silty peat. »

Line 336: Give reference. Also, consider changing "high-centered polygons" to "icewedge 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.

This sentence is based on field observations. We added such a mention in the text (« […] observable today in the valley »). See the picture below (high-centered polygons).

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. et al. 'A shift of thermokarst lakes from carbon sources to sinks during the Holocene epoch. Nature 511, 452–456 (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

We agree that sedimentation rates in thermokarst lakes can be quite variable and sometimes very high, as the reviewer points out. High accumulation rates are usually associated with active shore erosion and slumping along several meter-high bluffs. This is not the situation at Gull Lake. Although we observed signs of shore erosion such as drifting peat blocks, the shores are less than a meter high and much of the thermokarst activity around the lake occurs as subsidence (see the now-submerged peripheral platform in the accompanying video). Moreover, dissolved organic carbon (DOC) concentrations, as well as nutrients and suspended solids are rather low in this lake, indicating that erosion and slumping is not significant along the shores. Finally, there is no inlet to this lake. For all these reasons, we conclude that accumulation at Gull Lake is rather slow, as measured from sediment traps deployed in thermokarst systems of the sub-Arctic (Coulombe et al. 2016).

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.

These data were not planned in the design of our study, and they have been used by colleagues among our extended research group (e.g., Preskienis et al., in review). We included them as a supplement, and as the reviewer points out these are minor data, so we decided to keep the text as it was.

Also, the results section did not state clearly that an unfrozen zone could not be detected from GPR data.

In the results section we report that the shallow peripheral platform appears to overlie completely frozen ground: « The signal velocity (> 0.13 m ns^{-1}) based on the shape of some hyperbolas suggests that they occur in frozen material. ». There is uncertainty about the deeper central basin, due to signal attenuation. We added a sentence at the end of the paragraph to clarify this point: « Their occurrence at shallow depths beneath the central lake basin suggests that the lake does not possess a deep thawed zone (talik) as is often the case underneath deep water bodies. »

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 content? 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?

In response to these questions:

1) Yes, since the glacio-fluvial unit has a low ground-ice content (limited subsidence upon thaw), the maximum depth of the lake (in its central basin) might not evolve significantly in the future. The stratigraphy under the glacio-fluvial sands is unknown. Marine clay was observed under glacio-fluvial sand a few hundred meters away, on a lower elevation bench of the terrace. It is possible that this layer is present under the glacio-fluvial sands of gull lake. Further deepening (thermokarst) will occur within the peripheral platform.
2) We do not know how long the lake will remain, but we suggest two possible outcomes in the discussion: infilling or drainage. It is worth noting that other lakes were drained (partly or totally) within a 1 km radius of Gull lake and we therefore estimate that the drainage scenario is very likely.
3) Coalescence was observed within the valley among smaller and shallower ponds. However, we also observed direct clues of partial lake drainage in the lake next to Gull Lake (informally named Gull Lake-2 or 'GL-2'; see Fig. 1c). Paleo-shorelines could be mapped and showed that this particular lake was larger in the past and

partly drained. Topographically, it is located between Gull Lake and the proglacial river just north. There is a stream between the two lakes (Gull and GL-2).

4) We do not think that complete « lake infilling is really an option ». As the reviewer points out, sedimentation rates are fairly low, and the lake is growing laterally (peripheral platform). Hence, as we state in the modified sentence: « <u>Some</u> partial infilling might have time to occur, <u>but natural landscape evolution is likely to result in partial lake drainage, as suggested by the presence of erosion gullies in the valley and by evidence on such a partial drainage in a nearby lake</u> ». In short, we think that partial drainage of the lake is the most likely scenario (see next paragraph). Erosion gullies have been observed elsewhere in the valley, and as stated above, paleo-shorelines mapped around the nearby 'Gull Lake -2' show that this is a likely scenario for 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 take 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.

We agree. This is why we start by mentioning lake (partial) infilling as possible, but not as probable as partial drainage (next paragraph). We end our reasoning by this sentence: « Such a partial drainage is likely to happen to Gull Lake in the future, affecting at least the shallow peripheral platform and leaving a residual smaller lake corresponding to the current deeper basin. »

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.

We removed that sentence. For a better transition with the following sentence, we added: « We did not observe direct signs of terrestrialization at our study site. »

Line 391: give reference(s)

We added a reference (French, 2017).

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.

As stated above (reply to general comment #2), our point here is to present a site containing organic-rich syngenetic permafrost of Holocene age (less commonly reported) and to compare it with Yedoma sites that are much more represented in the literature. Results from Holocene syngenetic ice-rich and carbon-rich permafrost is very poorly reported in the literature and it is not, for the moment, possible to compare averaged Yedoma with Holocene values such as at our site.

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

We expect that the Tank et al. (in review) paper will be released by the time that our manuscript is (hopefully) published.
Note to the Editors: if this is not the case, then we can remove this citation and replace it with another one.

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).

We modified the sentence accordingly ('entire' removed): « In other words, to obtain the same amount of organic carbon released from thawing of the upper 3 m permafrost terrace on Bylot Island, an equivalent of 30 m of Yedoma complex would have to thaw, which is extremely unlikely in the foreseeable future ».

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 lowcentered 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.

We want to draw attention to syngenetic permafrost in organic-rich Holocene deposits, which is under-represented in the literature. After all, Yedoma complexes are not that carbon-rich, and large areas where peat accumulation and burial are important show much greater organic carbon concents (such as in Bylot). See our reply to the general comment above.

If the Editors agree with that suggestion, we could add to this paragraph a comparison to some other ice-wedge polygons sites (e.g., from the Yukon Coastal Plains) as suggested by the reviewer.

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.

Agreed, and we removed this sentence.

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

But Yedoma deposits are all of Pleistocene age, so we 'flipped' the sentence around to make it clear: « […] which is dominated by Yedoma deposits (Pleistocene-age ice-rich permafrost) ».

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.

We changed the sentence accordingly (« […] regardless of air temperatures. »).

Technical edits

line 43: replace "since" with "in"

Done.

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

It has been submitted and is now 'in review'. We modified the text here and elsewhere.

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

Done.

Line 87: developed into

Done.

Line 91: total precipitations -> a total precipitation

Done.

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

Done.

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

This is a minimum age (14C). We changed to '<u>at least</u>' 3700 years ago.

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

Agreed, we changed to 'done'.

Line 198: the lake bottom

Done.

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

Done.

Line 254: "in average" should be "on average"

Done.

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

Done.

Line 269: Better use "entire" instead of "whole"?

Done.

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

Done ('During the late Holocene…').

Line 323: "found at the lake bottom"

Done.

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.

We removed 'significant' and the allusion to heat 'extraction'. The modified sentence now reads as follows: « In autumn, heat loss from these small water bodies to the atmosphere and subsequent phase change of water to ice delayed the freezing front propagation in the underlying ground (Kokelj and Jorgenson, 2013) (stage 2; Fig. 6c) ».

Line 354: the lake bottom

Done.

Line 386: water balances -> water balance

Done.

Line 392: tapping

Done.

Line 408: "presents slightly over" could be "contains more than"

Done.

Line 411: "are comparable to other circumpolar regions"?

Done.

Line 418: formerly glaciated terrain ?

We are talking about numerous sites here, so we need to keep the plural form (terrains).

Line 442: "self-enhancing"?

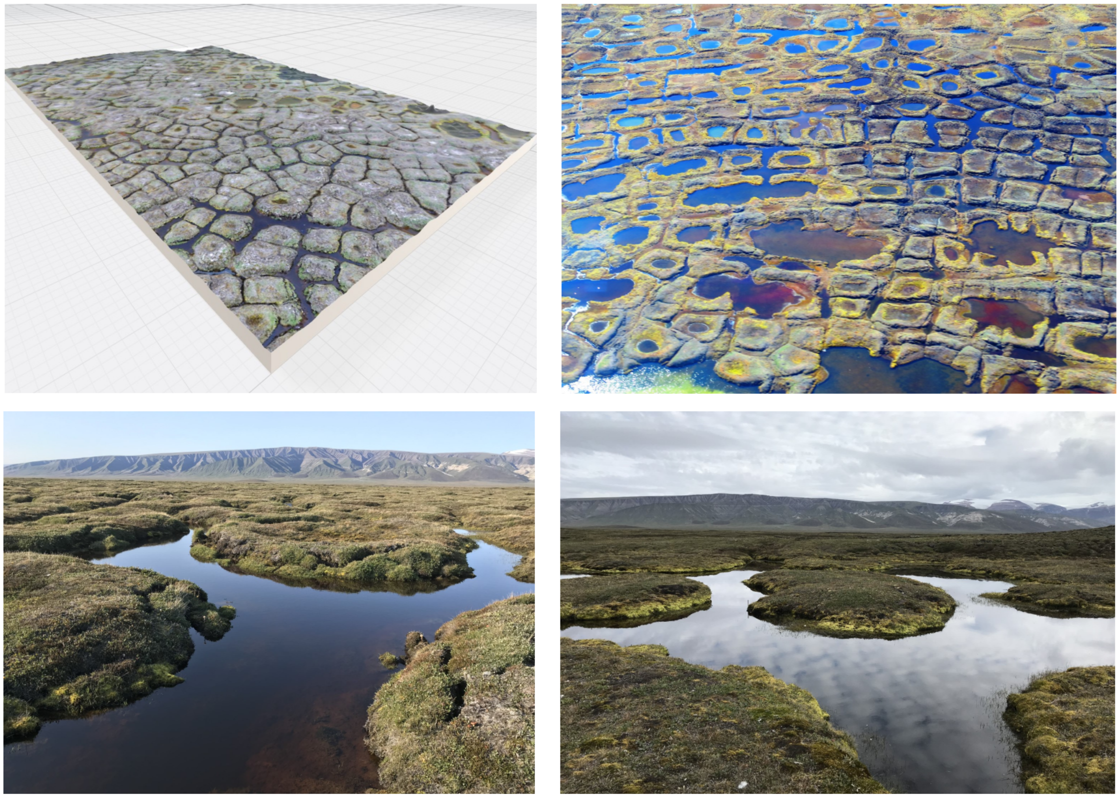
Done.

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

This is unfortunately not possible because species or species groups are presented according to their occurrence at the site along with ecological/habitat changes through time (from bottom to top of the sediment core).

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?

One major difference between our model and previously published models is that ground ice melting and the initiation of thermokarst start from the top of ice wedges, either at ice wedge junctions or along longitudinal segments. In previously published models thermokarst initiated in the center of ice wedge polygons, which is counter-intuitive since the ground ice content is much lower than below ice wedge ridges (close to 100% ice). We attached a field photo (below) showing ice wedge degradation with intact polygon center. This situation was common at our study site. Deep ponds at ice-wedges intersections are the weak spots in ice-wedge systems that become especially vulnerable with warming climate. Numerous observations confirm that ice-wedge thermokarst commonly starts at these locations and continues rapidly along ice-wedge troughs. These degraded ice wedges are usually covered by longitudinal ponds of various depth. These 'collapsed trough ponds' eventually merge together and sometimes merge with small ponds in the middle of polygons.



Another novel element of our model is the fact that lake initiation started in late Holocene sediment during a colder climate ('Neo-glacial'), mostly driven by natural landscape evolution and the strong impact of snow accumulation in a pre-existing topographical depression. We added the following sentence in the conclusions to make it more obvious: « Moreover, this model explains the early formation (inception) of a thermokarst lake during a cooling climatic trend (the 'Neo-glacial), underscoring the importance of natural landscape dynamics over temperature only. »

About the last question (diatom species, intra- *vs*. interpolygonal ponds):
No, it is not possible to tease out intrapolygonal *vs*. interpolygonal pond environments with diatoms. The difference is too subtle or non-existent in terms of substrate or habitat.

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