January 13, 2025

For the attention of the Editor,

We have carefully considered the latest reviewer's comments and thank you again for the opportunity to respond. Our responses are detailed line by line in response to each of the reviewer's comments. We have submitted a copy of the manuscript with all changes accepted, for reference to this file. We are very grateful to the reviewer, Peter Morse, for their careful and insightful comments which have allowed us to improve the original submission and to clarify areas where our interpretations were not clearly expressed. As a result, we feel that the manuscript is much improved, and we hope that you will find that it is acceptable for publication.

We thank you for this opportunity to revise the manuscript. Please do not hesitate to contact us should any further clarification be required.

Sincerely,

Shannon M. Hibbard, Corresponding Author *(on behalf of all authors)*

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Reviewer #3

The authors responded to R1 and R2 comments pretty well, but there are still gaps that need to be considered. Most stem from what may be an inadequate appreciation of some of the permafrost process and process rates that are invoked in the "Periglacial" model. This model was developed mainly due to the prodding of R1, but based on the evidence presented I cannot see any strong justifications for development of syngenetic segregation ice or injection ice in coarse sediments/bedrock that would create entire terraces. Rampton's hypothesis on formation of massive icy beds of the western Canadian Arctic required permafrost to be obliterated beneath a "warm-based" glacier, and there does not appear to be any evidence for this on Axel Heiberg Island.

The authors should return to this paper and work further to link their observations and evidence to an origin. I agree with their preference for a "Glacial hypothesis" but they need to demonstrate this better.

Thank you very much for your thoughtful and thorough review of our manuscript. We have considered your suggestions carefully and believe we have adequately addressed your concerns and outlined a strong argument for a glacial origin. Please see our responses below.

Line 20: I agree with R1. These aren't newly recognized, nor unusual, Arctic landforms. Just as ice wedge polygon networks have a range or morphometric expressions that vary according to surficial materials, climate variation throughout the Holocene, etc. this Vermicular Ridge / Involuted Hill / Ridge & Swale morphology has been observed in a variety of configurations in contemporary and past permafrost environments. I would just say that they are "striking" or "uncommon".

To the authors' knowledge, glacial ring forms have not been documented in Nunavut aside from Vermicular Ridges in Dundas Harbour. We have changed "newly recognized" to "striking" in Line 19 but have also included remarks on this being the second documentation of glacial ring forms in Nunavut in Lines 396 and 496.

Line 27: remove "an"; change to "features"

We reorganized and reworded the sentence altogether and checked that it is grammatically correct.

Line 32: capitalize Arctic

We changed this throughout where appropriate. We also double checked that "Canadian High Arctic" should be capitalized.

Line 39: capitalize Arctic

We changed this throughout where appropriate.

Line 43: these two ice types would have formed in place and are not "buried". Please carefully check the manuscript for this error and correct.

Thank you for pointing this important distinction out to us. We changed this throughout where appropriate.

Line 50: Change to “Thus, the”

Changed.

Line 51: Remove “which” and replace with “such morphologically similar features”; remove ”be”

Changed.

Line 53: Replace “Both massive” with “Massive forms of both”

Changed the wording.

Line 55: change “are” to “is”; capitalize Arctic. Arctic Institute of North America: "Capitalization: “Arctic” is capitalized when it is used as a noun (“the Arctic”). Used as an adjective, “arctic” is capitalized when it refers to the geographic region (i.e., Arctic communities) and lowercased when it refers to very low temperatures (i.e., arctic gale)."

Changed and followed this use of Arctic throughout.

Line 58: Change “affects” to “effects” Merriam-Webster: 'Affect is usually a verb meaning "to produce an effect upon," as in "the weather affected his mood." Effect is usually a noun meaning "a change that results when something is done or happens," as in "computers have had a huge effect on our lives."'

Changed.

Line 64: Add “, though this landform expression has also been termed ...” (please list other names from the literature)

We changed this sentence altogether to fit the new “ring form” terminology. We define “ring form” here. Lines 62-64.

Line 112: Please explain what you did in each trench. Did you just take a look, or did you collect samples, did you investigate grain size?

Included more information. Now Line 114.

Line 162: Add “the presence of ice-rich permafrost undergoing”

Added.

Figure 4: This exposure shows the ice matrix has been deformed, and the deformation does not reflect any obvious differential loading by the overburden that is present today.. This is relict glacial ice. If this were "massive-segregated ice" forming at the margin of a receding glacier, the ice layers would not be deformed like this. We see this in the vicinity of Involuted Hill, NT.

Thank you for pointing out this observation. We note this observation throughout the manuscript now. Figure 4 caption and Lines 372 and 507

Line 273: I don't know why these are just not called "ring forms" following Johnson and Clayton 2003. Inventing a new name seems unnecessary.

We have changed this throughout and no longer use the term “Vermicular Ridge Feature”

Line 277: I think some of the R1/R2 comments may relate to the present authors trying to make these landforms stand out with a new name. But from a landsystems prespective these "VRFs" are just part of the family of ring forms that include these morainic rim ridges.

We have clarified this definition of ring forms and its relationship to other types of ring forms documented in the literature. We specify whether we are discussing periglacial or glacial ring forms specifically throughout.

Line 288-289: I agree

Thank you

Line 291: add “lilthalsas, or other frost mounds”; Remove “ and other frost mounds and blisters

Changed. Line 305

Line 295: Add “, and are associated with high local relief (open-system pingos) or drained lake basins (close-system pingos)”; Replace “and” with “,”

Added. Line 312

Lines 296-299: However, what? If there are no pingos in the study area, no need to keep writing about them because related processes are not at play. I would delete.

We reorganized this section to emphasize that pingos are inconsistent with Mokka Fiord observations. However, we retained the sentence on Arctic pingo water sources, as it highlights the possibility of pingo formation in Arctic environments, which justifies considering it.

Line 310: Strictly speaking, if the ice sheet retreated a long time ago, whow can the environment still be a polythermal and cold glaical environment. Needs some wordsmithing. You could just say "... a cold permafrost environment that would not hav epromoted lithalsa development (Wolfe et al., 2014)," ...

Thank you for pointing this out. We removed this sentence and discuss the cold permafrost environment in the next paragraph. Line 337

Line 311-313: Remove “palsas and lithalsas” and change to “Lithalsas” - No need to drag in palsas, too, ; remove “occur” and change to “develop”; Lithalsas develop with permafrost aggradation into fine grained sediments that are undergoing 1-sided, top-down freezing. Unlike pingos that develop with closed-system freezing. Lithalsa formation has nothing to do with the active layer or surface hydrology, adn everything to do with "warm permafost conditions , slow freezing, and lots of water nearby that can migrate upward to the freezing front. In the high Arctic, at this setting, was the glacial ice thick enough to obliterate permafrost? No because permafrost is still several hundred metres thick, so between the lack of drained lakes and the cold climate regime of high Arctic, lithalsas are unlikely.

We have included this in our argument for why these are not lithalsas in Lines 319 and 324

Line 316: capitalize Arctic

Changed.

Line 317: during active-layer freezeback, or with permafrost aggradation? Please be clear.

Clarified this. Ice lens formation. Line 331

Line 320: add “have”; change to “experienced”

Changed and added. Line 334.

Line 325: Change “a much more similar morphology” to “more similar morphologies”

Changed. Line 342.

Lines 325-327: Combine into one sentence.

Combined. Line 342.

Lines 331-333: Sentence needs improvement. It jumps between two different subjects.

We have reorganized the sentence and broke it into two sentences to improve clarity.

Lines 340-341: Early in results you say that these VRFs occur within glacial to glaciofluvial.

This sentence was removed and this paragraph was rewritten. This paragraph now includes arguments against a segregated ice interpretation by referring to our observations, examples in the literature, and many of the arguments you pointed out. Line 357.

How did these taliks form if the glacier was cold-based? Permafrost is still very thick? is there evidence of extensive lacustrine deposits, no.

This paragraph was rewritten. This paragraph now includes arguments against a segregated ice interpretation by referring to our observations, examples in the literature, and many of the arguments you pointed out. Line 357.

If you think that this "meltwater/groundwater' origin is unlikely, then the environment can't have been conducive. You need to work on these arguments carefully.

Agreed. This sentence was removed and this paragraph was rewritten. This paragraph now includes arguments against a segregated ice interpretation by referring to our observations, examples in the literature, and many of the arguments you pointed out. Line 357.

Line 353: I would absolutely rule out a periglacial origin. Look at your exposures in the photographs, and carefully read the literature you have cited, and you can most certainly do this. You just need to pull up the pertinent criteria required for each of these other landforms and say which ones are not met. And don't forget, Mackay and Dallimore's ideas about massive syngenetic ice beds are also being challenged. There is a lot more buried ice in the Tuk Peninsula than Mackay/Rampton/Dallimore recognized at the time.

After reviewing your comments, the literature, and reassessing our observations, we have decided to reject a periglacial origin and reaffirm our original glacial hypothesis. We have significantly revised the

discussion to reflect this and believe we now provide strong, adequate support for the glacial interpretation. Thank you for your thoughtful review.

Lines 355-356: I disagree with R1. These landforms are not explained by any permafrost or periglacial process that has been quantified. There is just no evidence for any pingo, lithalsa, frostblister, or what have you. And there is no evidence to suggest that ice wedges have anything to do with these ring forms.

We agree and believe we have carefully and thoroughly ruled out this hypothesis throughout the discussion section.

Line 359: There are so many competing names! I would stick with one used by Johnson and Clayton 2003, supraglacial ring forms.

There certainly are. Thank you for your suggestion. We have used “ring forms” throughout.

Line 378: I think that Jonson and Clayton 2003 have got this dialed in, except in their model (their figure 10.7) they consider only complete degradation of glacial ice/sediment matrix because this is the view informed by their observations and examples from southern areas that are beyond any contemporary zone . But in cold, permafrost regions this melt out is not complete. However, the same supraglacial features could form and be presented at the "stable surface stage". The "stable surface stage" being partial melt out until the thawed materials insulate the lower ice and permafrost establishes and preserves relict ice.

Thank you for noting this. We have incorporated this into the discussion of our model. Line 457.

Lines 449-450: Do the sandar have ridges? You have to work on using the observations to rule out competing hypothese. Maybe you can't, but I think with careful reading of these other papers that you can. Also , look into Dredge et al., 1999 (https://doi.org/10.1139/e98-087) who observe relict ice preserved beneath some eskers and ice-contact outwash sediments.

Thank you for your valuable input. We have utilized our observations in our favor to make a strong argument for a glacial origin. We do this throughout the discussion in section 5.2, most of which is discussed in Lines 443-455. We reference Dredge et al (1999) now in Lines448. We separate the sandar from the kame terraces from Bednarski’s study and discuss the proposed kame terraces at Mokka Fiord. We think we have made this distinction clearer that will make it easier for the reader to follow. Lines 443-455.

Line 460: Change “formation mechanisms” to “origins”

We have removed this paragraph altogether and merged this section with Section 5.2. We no longer discuss a periglacial model and focus solely on a glacial model.

Line 461: This is four processes or 'formation mechanisms'; They cannot be lithalsas or pingos, but you need to demonstrate why.

We have removed this paragraph altogether and merged this section with Section 5.2. We no longer discuss a periglacial model and focus solely on a glacial model. Additionally, we have clarified the specific origin proposed for the ring forms at Mokka Fiord, addressing the ambiguity in the previous draft.

Figure 9: Was the glacier cold-based or warm based? If cold based, it is unlikely that there was much talik formation. The periglacial model suggests that segregation ice formation created the terraces, but how did the migrating channels provide continuous water to create entire terraces? I don't think this is plausible. Authors need to reconcile this with Rampton's "warm-based" model presented by Figure 58 in his 1988 Memoir 423, Quaternary Geology of the Tuktoyaktuk Coastlands, Northwest Territories?

We agree that a warm-based model (at least periods or localized warm-based events) would have been necessary for a periglacial formation which we agree has serious implications for Holocene climate on Axel Heiberg Island. We discuss Rampton's model in Line 369 and address each of your major questions below.

Major Questions Re Periglacial Hypothesis:

Q1: Setting aside the question about how well developed a talik would be under the polythermal/cold thermal glacialer; How can there be substantive development of segregated ice and heave of Bedrock/Eroded sediments to create a terrace? Neither bedrock nor the gravelly diamict would have high potential for ice segregation and heave. In Stage 1 the Ice is frozen to the bed in both cases, so there is permafrost. Rampton's (1988)model for massive beds of segregated ice require that permafrost is obliterated under a warm-based glacier, with segregated ice formation at the margin of the retreating glacier as permafrost aggrades.

We were suggesting that the terraces would form from normal fluvial terrace formation processes and isostatic uplift, not from ice segregation alone. You are correct that coarse grained diamict would not have a high potential for ice segregation. This periglacial model would need to assume a high silt content exists in the diamict, which we did not observe. However, we did not collect sediment samples to confirm this.

We did not suspect mass permafrost degradation would occur under the glacier. Therefore, a talik would have needed to develop under a subglacial lake or after deglaciation and from meltwater streams and proglacial lakes. This requires high water output which is less likely in a polythermal/cold glacier regime. We discuss these discrepancies in our observations in Lines 357-370.

Q2: If the ice is of segregation origin following glacial retreat, how can it exhibit signs of deformation that are visible in the photographs? Any pingos, lithalsas, etc. that we see in section do not exhibit signs of folding/shearing that are shown in Figure 4.

You are correct. We were hesitant to draw conclusions from this figure as we did not examine the thaw slump in detail on-site. However, you make an excellent point—the sharp, straight contact between the deformed ice and overburden strongly contradicts a segregated ice hypothesis.

Q3: Why would there be "slow" permafrost aggradation? The climate would still be very cold in this high Arctic setting with "fast" aggradation compared to western Canadian Arctic or other localities for example. Of course the rate of aggradation decreases with depth by the square root of time, so in any case we would not massive lenses of segregated; lithalsas form in "warm" permafrost settings where there is "slow" freezing because of the low temperature gradient between the air and ground, and we normally don't see large, multi-decimetre segregated ice lenses until several meters down when the rate of aggradation has slowed.

Correct, we would expect the permafrost to aggrade fast in this climatic environment. Yet, aggradation would need to be slow in order to form this massive ice which was a big concern in the periglacial formation hypothesis. We had originally considered that salts may have played a role since the Mokka Diapir is so close. Yet, a larger water supply is still necessary which is a fatal flaw in the periglacial argument among others. Therefore, we have removed the periglacial model as a possibility.

Line 488: If here is minimal evidence for this, then why open the door to questioning? Use this information early in the background to nail the door shut at the beginning.

Correct. A fatal flaw of the periglacial argument, which we lean on more heavily in our discussion when ruling out periglacial origins.

Line 510: This statement is incorrect. According to the proposed periglacial model, the segregation ice is epigenetic with downward permafrost aggradation into the talik and so is not "remmnant" nor has it been "buried".

Thank you for pointing this out. We have been much more careful in how we refer to the massive ice body throughout the manuscript to address this.

Lines 517-524: These points about timing of permafrost aggradation/degradation are a bit out of context and conflated. The duration of time to aggrade/degrade permafrost to a depth of several hundred metres indeed takes a long time, but aggrading only a few tens of metres does not take so long. And the rate of aggradation is often faster than the rate of degradation because this is a cold climate still and the heat balance in in favour of freezing on an annual basis.

Frost mounds can certainly form in relatively short periods of time, but ALL of the lines of evidence have to be considered, and I am not sure that the exposed ground ice looks like it is of segregation/injection

origin. The caveat here is that there really needs to be much better photographic evidence and interpretation presented on the stratigraphy of the thaw faces.

Thank you for your input. We decided to remove this discussion about timing. There is considerable ranges of time reported in the literature and therefore this discussion did not add much significance. Instead, we focused on other lines of evidence.

If there were injection ice formation, there would be visibly exposed "dykes" of ice, that again would not be deformed.

Great point. We mention this now in our discussion. Line 374

Line 546: No, the segregation ice had to have formed in place and was not buried.

Correct. We have corrected this distinction throughout our manuscript. Thank you.

Line 552: The authors can do more work on this: what do thawed out lithalsas or "segregation ice features" look like? There are reports of these in the literature given the current state of degrading permafrost across the Arctic, and we don't see this morphological expression in contemporary settings.

Do you have any examples of this? As far as we have found, thawing lithalsas become boggy and flooded which we do not see at Mokka Fiord.