

Reply to Editor comments on

"An Estimate of Ice Wedge Volume for a High Arctic Polar Desert Environment, Fosheim Peninsula, Ellesmere Island"

by Claire Bernard-Grand'Maison and Wayne Pollard

General comments:

Dear Ms. Bernard-Grand'Maison,

Thank you for submitting your revised manuscript. Both reviewers recommended publication following some revision, and both gave you thoughtful comments that have improved the paper.

I agree with the reviewers that the manuscript should be published after some further revision. Most of the edits are minor and should be quick. I do have some comments that relate to the point raised by Reviewer 2 about your assumptions regarding the use of average ice wedge dimensions from Couture and Pollard (1998). Essentially, the paper argues that it is hard to justify different IW width and depth based on polygon size/trough length alone (agreed! There are so many reasons why there is no direct relation), so it is assumed that IW width does not vary significantly between polygonal terrain types. However, the problem is that Section 3.1, Figure 4, and Table 2 highlight 4 sample locations with different polygon sizes, morphologies, densities and, critically, widths. This makes it hard for a reader to agree with the assumption declared in Section 5.2. This section rightly points out that assumptions regarding IW width and depth affect volume estimates, but then the discussion focuses on implications regarding depth and does not treat width. The noted variability in width has implications on IW volume estimate and likely relates to sub-regional variation of geological setting/history (e.g., marine vs. fluvial vs. glacial).

In a general sense, it would be very informative to explore the implications stemming from assumptions regarding IW width variation, in addition to your exploration on trough length, as width and length are often both estimated for from remotely sensed images, whether the polygons are on Earth, Mars, or wherever. At minimum, it would be an important contribution simply to demonstrate the magnitude of variation that a range of assumed IW widths can have on regional volume estimates. It would give readers a sense of the margin of error, and can probably also be tied in to your discussion regarding the general assumption that all IW have a surface expression.

However, if your sample locations do suitably represent different geological contexts/settings, and you have some general surficial geology map data, it would be really great to tease out some aspects on sub-regional IW volume variation, and possibly refine the overall estimate of IW volume for the FP.

I think you can investigate this component of variation using the data already presented in the paper, and with out any additional digitizing. This is important as you indicated that the original polygon data are gone due to a hard drive failure.

This may take a bit of time to address, but when addressed should clear up and strengthen some of your arguments, and will demonstrate to readers the degree of sensitivity that IW volume estimates have to assumptions regarding IW dimensions.

My final comment is to see if you can improve the "hook". Your paper further develops the method for estimating IW length and I think this innovation can be better highlighted at the beginning to really grab the reader's attention. In the current version, your innovation isn't highlighted until close to the end of the paper.

I think that there will be a fair amount of interest in this paper, and I look forward to reading a revised version of the manuscript.

Detailed comments follow below.

Best regards,

Peter

Dear M. Morse,

Thank you for considering our manuscript for publication in *The Cryosphere* and providing detailed comments and edits to further improve the paper. We believe our revised version better highlights the new GIS methodology presented in the paper to “hook” the reader and makes the case for the value of our first order estimation of ice wedge volume for the Fosheim Peninsula. We have addressed the general comments by replying to the specific comments below (in red).

Regards,

Claire Bernard-Grand’Maison and Wayne Pollard

Specific comments:

P5/L14: Frost blisters and lithalsas are other massive ice types that can be mapped. You can go this route and cite a few more papers, or perhaps just say that IW often have a distinct surface expression that can be mapped using high resolution satellite imagery (cite appropriate references).

The authors agree that using the word “only” is misleading and ignoring the cases of frost blisters and lithalsas. The sentence was changed to: *“IWs often have a distinct surface expression that can be mapped using high resolution satellite imagery (Gilbert et al., 2016).”*

P5/L16: This statement is reason enough to explore (if possible) your volume estimates if you assume that your sites are representative of different surficial geological settings, rather than pooling them together.

The statement is: *“subsurface geometry [of ice wedges] is relatively consistent and closely related to terrain conditions and surficial geology (Couture and Pollard, 2007).”*

Looking at the original map by Bell (1992) from which the surficial geology data was taken to classify two regions (no ice wedges and potential ice wedge coverage) in Figure 8, all EL sites are from marine origin (gravel, sand, silt and clay deposited during higher sea level). The map only gives information for the Fosheim Peninsula, hence the surficial geology of site AH1 is not classified. This site is in a valley next to a large braided river and the origin of its surficial geology could be marine, glaciofluvial or fluvial. Our sample locations are then not representative of multiple surficial geological settings in terms of their origin (marine, fluvial, glacial or glaciofluvial sediments). Therefore, we cannot present a plausible estimate for each class where we could have differentiated between density of polygons, width and depth, which are probably related to their geological history. However, we think it is acceptable to pool all the classes together because the mean ice wedge width and depth that we use to estimate volume is representative of the glacial, fluvial and marine deposits as they are present within the surveyed area of Couture and Pollard (1998) centered on the Slidre Fjord. Any estimate of IW volume based on remote sensing techniques carried out over a large area with limited ground validation, which is what is presented in this paper for the Fosheim Peninsula, is as best a first approximation.

As the map by Bell (1992) is not accessible online for the readers to look up as complementary information, we decided to add more details concerning the surficial geology classes in the manuscript. In section 3.4, the classes are now properly listed in the first sentence and details is given on what classes have been included in the calculation of the area where ice wedges are present:

“We estimated the cumulative coverage area of IW polygons for the Fosheim Peninsula based on the surficial geology map from Bell (1992), which differentiates between surficial sediments of marine, fluvial, glaciofluvial and glacial origin as well as indicate weathered bedrock and residuum areas. The map was digitized with reference to the shoreline and contour datasets of CanVec series dataset from Natural Resource Canada (2016). As it is rare for IW

polygons to occur in bedrock (French, 2007), it was assumed that they can be located in all the unconsolidated surficial sediment classes (marine, fluvial, glaciofluvial and glacial). The potential area occupied by IWs was determined by subtracting the area of the large lakes and areas identified as bedrock to the total area of the peninsula. The 150 m CanVec contour was isolated as this provides a proxy for the Holocene marine limit on the Fosheim Peninsula because IWs are ubiquitous below this elevation (Bell, 1996; Couture and Pollard, 1998). We assumed that the mean of the IW percent volume of our sample locations was representative of the geomorphological settings where IWs are present on Fosheim Peninsula and used it to calculate the equivalent IW ice volume over the entire peninsula.”

The definitions of the surficial geology classes have also been added to the caption of the new Figure 3, a combination of Figure 3 and 8. The legend of past Figure 8 will also be changed to reference the glaciofluvial sediments class which has been omitted due to its very small proportion. The new caption is:

“Figure 3. Sample locations of this study and potential coverage area of ice wedges on the Fosheim Peninsula in the Canadian High Arctic. Surficial geology data is from a map produced by Bell (1992). The marine sediment class is defined as gravel, sand, silt and clay deposited during higher sea level. The fluvial sediments class is defined as gravel and sand deposited on floodplains and fans. The glacial sediment class is defined as non-sorted diamicton interpreted as till. The glaciofluvial sediment class is defined as gravel and sand deposited in the marginal zone of a former glacier. The 150 m contours (CanVec data, Natural Resources Canada, 2016) are a proxy for the Holocene sea level on the Peninsula (Bell, 1996). Coordinate System: NAD 1983 UTM 16N. Projection: Transverse Mercator.”

P5/L19-20: Cite Ulrich et al., 2014? They used Thiessen polygons for this reason.

The reference to Ulrich et al. (2014) was added.

P5/L23-24: Need a hook here too. Why do this? To develop a first order assessment of the response to climate change? Link to your conclusions.

The two last sentences of the paragraph were modified to include a “hook” to highlight the innovation of the paper (as mentioned in the general comments of the editor) and to justify why estimating ice wedge ice volume for the Fosheim Peninsula in a rough manner is relevant by making a link to section 5.3 Impacts of Melting Ice Wedges. The text now reads:

“In this study, we build upon the methodology introduced by Ulrich et al. (2014) by testing GIS-based methods to delineate IW troughs and present a new semi-automated method based on watershed segmentation principles. We then provide a first approximation of IW ice volume in a High Arctic polar desert environment, the Fosheim Peninsula, to assess its sensitivity to thermokarst processes as a response to climate change.”

The presentation of a new delineation technique was also highlighted in the abstract, where a hook is most needed: *“We demonstrate the potential of two semi-automated IW trough delineation methods, one newly developed and one marginally used in previous studies, to increase time-efficiency of this process compared to manual delineation.”*

P5/L27-28: If you can't map ESL is it because it is not well defined?

Suggest to change the text to: Our study focuses on the Fosheim Peninsula of Ellesmere Island (Fig. 3), which lies within the Eureka Sound Lowlands (ESL). The ESL include the central parts of Ellesmere and Axel Heiberg Islands in the northernmost part of the Canadian Arctic Archipelago.

The ESL has never been defined on a map but only qualitatively in a previous study by Pollard et al. (2015) by the geographic features bordering the regions (fjords and mountains). The text was changed to the suggested sentence and details on the area and bordering geographic features were added:

“Our study focusses on the Fosheim Peninsula of Ellesmere Island (Fig. 3), which lies within the Eureka Sound Lowlands (ESL). The ESL region roughly covers 750 km² on central Ellesmere and Axel Heiberg Islands in the northernmost part of the Canadian Arctic Archipelago and is bordered by the Sawtooth Mountains to the east and the Mueller Ice cap to the west (Pollard et al., 2015).

P5/L29: Before this text, please include a line or two about the general geology and unconsolidated surficial materials that you later use to make your regional IW volume estimation (Fig. 8 indicates marine, fluvial, and glacial sediments are present).

Details on the surficial geology from Bell (1996) have been added:

“Surficial geology of the area is mainly composed of unconsolidated sediments of glacial, fluvial and marine origins. The area is mostly flat to gently rolling with elevations below 300 m a.s.l. except for several ridges which rises to a maximum of 840 m a.s.l. where outcrops of intact bedrock occur (Bell, 1996). Ice-rich silty-clay marine sediments dominate below the ~150 m a.s.l. Holocene marine limit (Bell, 1996) and are underlain by continuous permafrost ~500 m deep (Pollard et al., 2015).”

P6/L18: Search and replace "Sample Areas" with "Sample locations". Also check for this in figures and tables. The areas are all the same, the locations are different.

Changes made as suggested.

P7/L1: Reviewer 1 asked for some indication of the variance of polygon dimensions. Recognizing that your GIS files are gone, can you please make a note that somehow indicates that while the importance of assessing variance is recognized, it couldn't be treated quantitatively, but can assessed quantitatively by examining Figure 4.

The authors agree that recognizing that variance would have been an interesting parameter to compute is a good idea. A sentence was added at the end of the first paragraph in the results section 4.1 after the mention of Fig. 7a and Table 2 where variance would have been added. Added sentence:

“While we recognize the importance of assessing variance in polygon size at the sample locations, it couldn't be treated quantitatively but can be assessed qualitatively by examining Figure 4.”

P11/L16: 2 other methods were employed. What are the other volumes according to the semi-automated techniques? If the differences are small, it would add strength to the argument that semi-automated techniques are an improvement. This may also allow you to point out in your discussion that more error may be introduced by width assumptions than by the technique used to determine IW length.

The volumes for the two other methods have been added to this paragraph. The differences are indeed small and both volumes are lower than for the manual method. The text now reads: *“The total IW ice volume is 6.7×10^8 m³, when assuming an IW volume of 3.81 % by averaging the results from the manual delineation method at the four sample locations in Table 2. Slightly lower estimates are obtained when averaging the IW volume of the two other semi-automated methods: 6.4×10^8 m³ with 3.61% for the Thiessen method and 6.6×10^8 m³ with 3.74% for the Watershed Segmentation method.”*

P12/L31-32: Please clarify this statement. Some of the ideas in this paragraph are a bit jumbled together. On line 22 you say that the method should be applied to a DEM to gain more confidence, but later in the same paragraph you indicate that high-res. DEM generation requires field work. Then here you say that you don't need field work.

The authors agree that this sentence might be confusing. The methodology we present is based on satellite images only and not on DEMs. Creating these DEMs with the techniques mentioned in the previous lines would require fieldwork. The only fieldwork required in our methodology is to get an average width and depth if this cannot be

found in the published literature. To clarify, the sentence was changed to: *“This highlights the strength of our relatively simple methodology, relying on high resolution satellite images and minimal field data, which can be applied on remote locations without the need of extensive fieldwork to create DEMs.”*

P13/L26: Implications for gross error regarding assumed IW width should also be discussed. Differences in width are obvious in the images.

The average width estimate from Couture and Pollard (1998) is based on a small sample of measurements and the authors argue that it would be misleading to offer any amount of error analysis. When attempting estimates on the regional scale considered in this study, it is necessary to make assumptions that reduce the absolute accuracy of the calculation. We are confident that our total ice wedge volume calculation for the entire peninsula is a reasonable “low” estimate. However, given the limitation of 3 variables already discussed in Section 5.2 (assumptions of mean width, mean depth and representative surficial expression), we modified the manuscript to refer to our estimate as a “first approximation”. It is also not possible to provide a range of IW estimates for the entire Peninsula using different width and depth without additional digitizing and without the original files which have been lost in a hard drive failure. We used a TIN to calculate the total frozen sediment volume and the ice wedge volume at each site and this is constrained in three dimensions by the delineated ice wedge trough lines which we do not have anymore. Therefore, we cannot simply apply a new width and depth to the total ice wedge length in each site for the manual method because we do not have the volume of total frozen sediments to calculate a new percentage of IW. For these reasons, we argue that a new calculation would not be possible, nor would it increase the accuracy of the estimate. We think that the assumptions made are valid and have modified section 5.2 to emphasise these points. We are confident that there is value in presenting this first approximation to inform on the magnitude of landscape change that can be expected at a regional scale in a polar desert environment where ice wedges are omnipresent. Here is the main text modified in Section 5.2 concerning ice wedge width:

“Multiple necessary assumptions are made when calculating IW volume with TINs and here we consider their potential effect in estimating IW volume at large scales. The most critical is probably the assumption that IW width and depth does not vary significantly between polygonal terrains, and lack of subsurface data meant that using mean IW width and depth was the best approximation we could use for our calculations. The small variability in estimations of IW volume for the entire peninsula from the three delineation methods suggests that more error might be introduced in our estimate from the assumption of a fixed IW geometry than by the technique used to derive IW length in a specific area. Differences in IW width at our sample locations are obvious in Figure 4 where multiple troughs are greater than the 1.46 m average used (e.g. EL2) and likely relates to sub-regional variation in geological history. To use these visible differences in surface expression as information of IW width would require another assumption that cannot be validated with the limited field data available: that trough width approximate IW width. Multiple sample locations in each surficial geology class presented by Bell (1992) would have permitted the calculation of IW volume on a sub-regional basis and the definition of IW parameters such as apparent width and polygon density for each surficial geology class. A specific example where assuming a fixed IW geometry is not valid on the Fosheim Peninsula is in the surficial geology unit of thin veneer of glacial sediments identified by Bell (1992). The thickness of this geological unit over bedrock is defined as 2 m, which is less than the 3.23 m mean IW depth used here. It is important to mention that the IW width and depth used in our calculations are minimal estimates because only exposed IWs were measured by Couture and Pollard (1998). Like was done in their study, we used the depth of 5.9 m below the active layer to calculate the IW volume because no IWs were observed below this depth.”

The justification of our estimate “as a first order approximation” has been added to conclude section 5.2: *“Given the potential errors discussed above associated with assumptions of width, depth and surface expression of IWs on the Fosheim Peninsula, we refer to our estimate of total IW volume as a “first approximation” and are confident it is a reasonable minimum estimate for this regional scale.”*

Our abstract and second point in the conclusion was also modified to position our estimate as first approximation:

“Secondly, IWs potentially cover an area of ~3,000 km² on the Fosheim Peninsula where a minimum of 3.81% of the upper 5.9 m of permafrost is comprised of IW ice. This first approximation is based on limited field validation data and sample locations which constrains it to the Fosheim Peninsula; however we are confident that our results are applicable to the entire ESL.”

P14/L12: Indicate why. Inactive? Incipient? obscured by mass wasting (Anti-syngenetic? Syngenetic?)?

Information from the cited source was added to the sentence to indicate why some IW do not have a visible surface expression in the ESL: *“Field observations in the ESL show that this is not always the case because many of the factors leading to trough development (e.g. vegetation coverage and surface hydrology) do not always apply in very cold and relatively dry polar desert environments. Usually, no trough structure is visible when the top of an IW is in equilibrium with the thin active layer depth (Fig. 1a) (Pollard et al., 2015). This assumption would lead to an underestimation of IW volumes on the Fosheim Peninsula.”*

P14/L14: You use 3 methods to delineate the polygons, only one is visual inspection. Suggest to delete the word “visible”.

Changed as suggested.

P16/15: Please use a consistent style. For example, some titles are abbreviated and others are not.

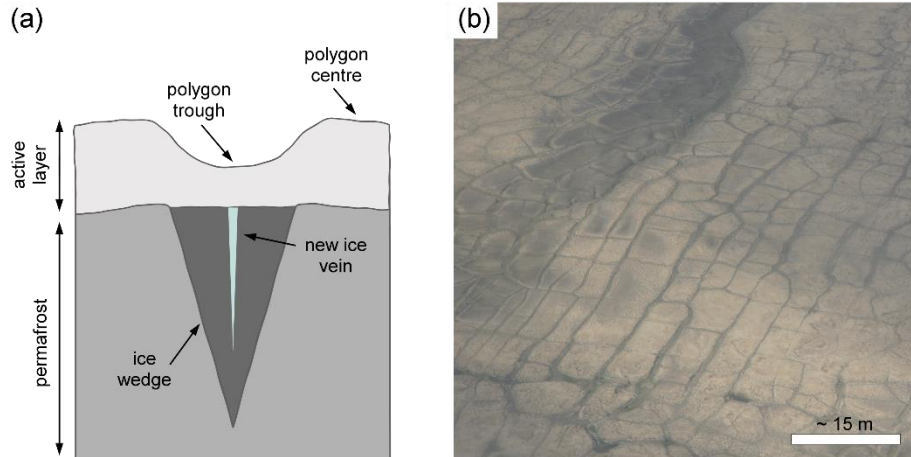
A consistent style was applied to the references.

Figure 1: Possibly an anti-syngenetic ice wedge [in a)]? Interesting if an epigenetic wedge of that great size did not have a trough/surface expression.

The ice wedge shown in a) was found in a stable plain terrain and is believed to be epigenetic in nature. This detail was added in the caption: *“Figure 1. Thermokarst processes in the Eureka Sound Lowlands. (a) Retrogressive thaw slump headwall with an exposed epigenetic ice wedge (~6 m depth) with no surface expression, Axel Heiberg Island, July 2016. Helicopter and person for scale. (b) Aerial view of an active melt out along ice wedge troughs and the resultant dissected landscape, Fosheim Peninsula, July 2015. (c) Example of back wasting of ice wedges melting out, Fosheim Peninsula. July 2013. (d) Rapid melt out of ice wedges where massive ice is present, Fosheim Peninsula July 2017.”*

Figure 2: By definition a high centred polygon does not ridges because they have collapsed (the centre is now high).

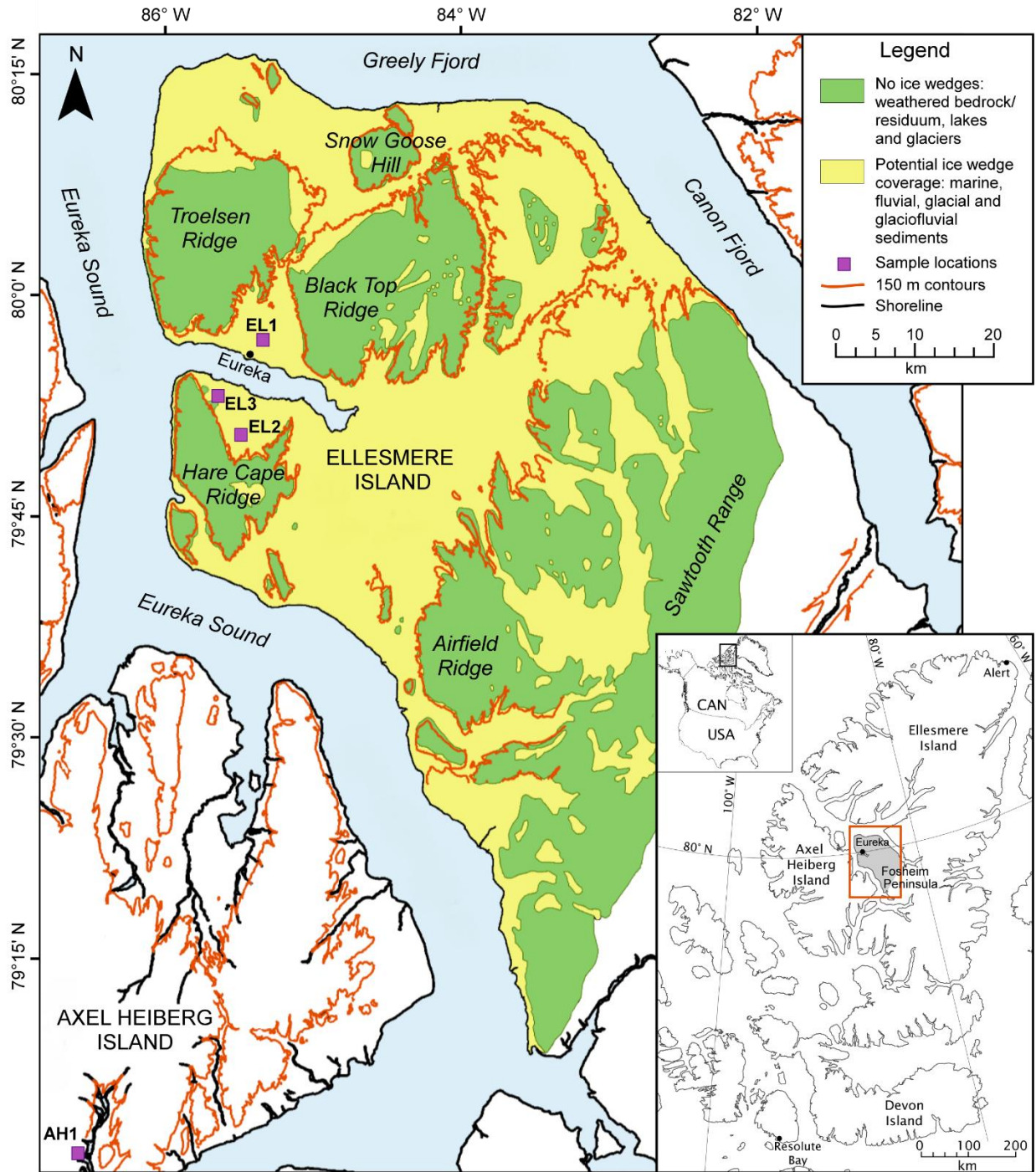
The authors agree with this comment. The center of the polygon on the figure was raised due to comments of Reviewer 2 that the majority of the ice-wedge polygons in our study area are high centered and diagram represented a typical low-centered polygon with ridges. The description “polygon ridge” has been changed to “polygon centre”, which is an important term in the manuscript. Updated figure and caption below:



“Figure 2. Ice wedges surface expression. (a) Representation of an epigenetic ice wedge in a high-centered polygon environment. (b) Aerial view of ice wedge polygons on the Fosheim Peninsula, Ellesmere Island.”

Figure 3: Please show sample locations in Figure 3. From a geographic perspective, the most useful figure is Figure 8, while these two maps provide context. Move Figure 8 here and use these two as insets. This will lower the figure count and you can then reference Fig. 3 as a part of your new background info on geology.

Figure 8 was merged with Figure 3 as suggested. The new Figure 3 is referenced as part of the geologic background info in section 2 Study Area: Fosheim Peninsula. New figure and caption below:



“Figure 3. Sample locations of this study and potential coverage area of ice wedges on the Fosheim Peninsula in the Canadian High Arctic, shown in inset. Surficial geology data is from a map produced by Bell (1992). The marine sediment class is defined as gravel, sand, silt and clay deposited during higher sea level. The fluvial sediments class is defined as gravel and sand deposited on floodplains and fans. The glacial sediment class is defined as non-sorted diamicton interpreted as till. The glaciofluvial sediment class is defined as gravel and sand deposited in the marginal zone of a former glacier. The 150 m contours (CanVec data, Natural Resources Canada, 2016) are a proxy for the Holocene sea level on the Peninsula (Bell, 1996). Coordinate System: NAD 1983 UTM 16N. Projection: Transverse Mercator.”

Figure 4: Image compression artifacts are visible in these vector diagrams. Please use little to no compression or vector format.

The image was changed for a higher resolution version.

Figure 7: Y-axis of (a): change to “Difference from the manual method”. Properly reference the tables (Table S1 and S2 are gone)

Changes made as suggested. Reference to Table S1 had been changed to Table 2. Reference to Table S2 was eliminated.