Reply to Interactive 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

Replies to Referee #2: M. Kanevskiy

Overall quality of the discussion paper ("general comments"):

This manuscript describes new methods of wedge-ice volume estimation based on GIS approach. It evaluates wedge-ice contents for the Fosheim Peninsula, Ellesmere Island, and compares various methods of such estimations. The paper will be very helpful for permafrost researchers who can use the suggested methods for estimations of wedge-ice volume in different permafrost regions, and I strongly support its publication. However, the manuscript needs some revision. My major comments and suggestions are listed below.

Individual scientific questions/issues ("specific comments"):

Comments (questions and wording changes that we did not apply) from the supplement to the comments (https://www.the-cryosphere-discuss.net/tc-2018-29/tc-2018-29-RC2-supplement.pdf) have been added to this list and addressed.

Page 1, Line 21: I recommend to cite a new edition of this book (French, 2018), where the numbers were updated (24%, see Table 5.1).

The citation and the number have been updated according to French (2018). This edition was not available at the time of preparation of the manuscript.

Page 2, Lines 21-30: Syngenetic IW are much more common than anti-syngenetic, and in some regions (e.g. yedoma regions in Siberia and North America) they occupy very large areas.

We added reference to the Canadian Arctic for this sentence, where epigenetic IW are the most common.

Page 2, Line 25: Accumulation of slope deposits

Accumulation of slope deposit is the result of mass wasting. No changes were made.

Page 2, bottom. Probably you should also mention that in this study you analyze mainly high-centered polygons?

This information was added clearly as it was not mentioned in the description of the sites. Based on our observations high-centered polygons are the most common expression and typical of polar desert environments. To add clarity the sentence was changed to: *"In this study, our analysis is concerned with epigenetic IWs, most commonly expressed as high-centered polygons in polar desert environments."*

Page 3. Section 1.2. Comment about decreased/increased snow cover.

The decrease in snow cover is a generalisation for the high latitudes, see the globe. An increase in snow accumulation is projected for the High Arctic due to warmer winter temperatures and availability of moisture. The text was modified to improve clarity:

"Decreasing albedo due to diminishing snow cover, glacier ice and sea ice extent affects atmospheric and oceanic circulation amplifying the increase in temperatures in the northern polar region (IPCC, 2013). In the Canadian High Arctic, increase in air temperature has largely resulted in winter and autumn warming, and sites with little snow cover exposed to winds are therefore more responsive to changes in air temperatures (Smith et al., 2012). An increase in annual snow accumulation is also projected for high latitudes due to warmer temperatures (AMAP, 2017)."

Page 4, Lines 17-22: It's better to start describing various methods of IW studies with exposures and drilling because geophysical methods are not very precise.

The order in which the methods are described has been changed in the paragraph.

Lines 21-22: Not all of the cited studies used this combination of techniques you're talking about.

The text was changed to make it clearer which studies are referred too for exposed IW and drilling and which use geophysical techniques:

"Point measurement data from exposed IWs, excavation and/or boreholes helps to constrain IW type, shape and mean IW width and depth for a specific site (e.g. Pollard and French, 1980; Morse and Burn, 2013; Jorgenson et al., 2015). Geophysical techniques such as ground penetrating radar (GPR) and electrical resistivity tomography (ERT) have also been used to investigate IW morphology (e.g. Munroe et al., 2007; Bode et al., 2008; De Pascale et al., 2008; Léger et al., 2017)."

Page 5, Line 14. What about pingos?

The text now reads: *"IWs and in some cases pingos are the only massive ground-ice types that can be mapped using high resolution satellite imagery."*

Page 5, Lines 19-21: I recommend to cite previous studies because Ulrich et al (2014) already applied semi-automated technique in their study.

The authors agree with this comment. The reference to Ulrich et al. (2014) was removed and the sentence was changed with no added references:

"Semi-automated techniques to delineate IW polygons on satellite images would greatly improve time efficiency and coverage area of wedge ice volume estimates compared to manual delineation."

Both techniques are already mentioned and described and referenced in the previous paragraph, so we see no need to repeat the references in this closing paragraph describing the objective of the study.

Page 5, Line 29: ESL region is not shown in Fig. 3.

The ESL region has never been clearly defined on a map. We modified the text to reference the Fosheim Peninsula which is shown in Fig. 3.

"Our study focusses on the Fosheim Peninsula on Ellesmere Island (Fig. 3) which lies within the ESL region."

Page 6, Lines 7-8: Please check these numbers. In the cited paper, it was stated that wedge ice accounts for 3.3% according to Table 2, and numbers 1.8 and 3.5 are from Table 1. Anyway, these numbers look confusing and I recommend to explain them better (probably you should compare them with volumes of other types of ground ice reported by Couture and Pollard 1998).

The authors agree that this sentence was confusing. After checking the numbers in the original Couture and Pollard (1998) paper the text was modified to: *"It was estimated that in this region wedge ice accounted for 1.8–3.5 % in volume of the upper 5.9 m of permafrost and that all types of ground ice combined accounted for 30.8 % (Couture and Pollard, 1998)."*

Page 7, Line 9. Use of Thiessen polygons in Ulrich et al. 2014: Not only!

The text has been modified to: "This approach was used in Ulrich et al. (2014) to estimate volume of a relict IW network in baydzherakhs landforms, where IWs had melted and only raised polygon centres remained, and at other sites to compare with manual delineation."

Page 9, Line 32. I'm not sure this is a good approach to attribute mean IW parameters from that study to all four sample areas that you use in this study because definitely these areas are very different: as you already mentioned, they have "different polygon size, morphology, density and width of troughs."

We acknowledge the reviewers concerns but are confident that our method adequately accounts for these differences. The three delineation techniques presented in the paper gives the centerlines of the IW through and not the width. Therefore, we don't see it justifiable to choose different width and depth qualitatively based on polygon size. We consider that applying our methodology to a large area is the best estimation we can provide at this stage.

Page 9, Lines 10-14: I recommend to clarify your approach. It would be good if you add a simple equation or a figure.

The authors acknowledge the reviewers comment but think that adding a figure or an equation is not necessary as the numbers provided are specific to our application and depend on the elevation reference used for the "Surface Volume" tool. All the given elevations in this paragraph and the previous have been changed to match the reference elevation of 0 m at the base of the active layer which corresponds to what was already indicated in Fig. 7 (now Fig. 6). Reference to this figure (Fig. 6, previously Fig. 7) was added to help the reader visualize the TIN and imagine the invisible planes used to calculate volumes. Clarifications in text have also been added concerning the provenance of the negative elevation values from mean IW depth and from depth of frozen soil considered.

Page 9, Lines 18-20: Actually, polygons in bedrocks are rather common in your study area. I recommend to explain your assumption in a different way: polygons may exist in bedrocks but wedge-ice volume is negligible.

We are of opinion that the reviewer is not entirely correct. Ice wedges in "solid" bedrock are not common in this area and as such bedrock areas can be ignored. By intact bedrock we mean areas of exposed rock occurring as outcrops in major ridges as defined in the surficial geology map by Bell (1992). The only ice wedges that might be considered as occurring in bedrock are where the bedrock is largely unconsolidated (Tertiary deposits) in which case their pattern is basically the same as areas that are discussed in this paper and are therefore included in our volume estimate. The sentence now reads: *"The potential area occupied by IWs was determined by subtracting the area of the large lakes and intact bedrock areas."*

Page 10, Lines 24-30: I recommend to report mean values (perimeter and/or area of IW polygons in different sample areas) either in the text or in Table 2.

Values of mean perimeter and area that are plotted as per cents in Figure 7 (previously Figure 6) have been added to Table 2.

Page 11, Line 10: I recommend to add these numbers to Table 2.

In this statement we were referring to the % IW volume of the manual delineation method that was found in Table 2. We think it would be confusing to add these in Table 2 as the averaging is only for the Manual method output. The text has been changed to: *"The total IW ice volume is 6.7x10^8 m^3, when assuming an IW volume of 3.81 % by averaging the results from the manual delineation at the four sample areas in Table 2."*

Page 12, Line 21. Add references to "various other image segmentation and classification techniques"

This sentence was deleted from the text as the authors do not have sufficient knowledge of references in these fields and it was originally put as a suggestion for further studies.

Page 13, Line 1-2: The goals of your study are mentioned in the introduction and there is no need to mention them again but I recommend you to explain somewhere that unlike Ulrich et al. you have implemented one more method - watershed segmentation.

This is a valid point and the sentence was removed from the text here. It is now mentioned in the conclusion that the Watershed Segmentation is the new methodology presented in this study.

Page 13, Line 8: I recommend to mention here that your values from EL2 are very similar to "high density" values in the cited paper.

Changes has been made according to the suggestion: *"Even if our values from EL2 are very similar to the "high density" values in their study, EL1 and EL3 have a much higher IW ice volume percentage, redefining "high density" polygonal terrain on the Fosheim Peninsula."*

Page 13, Line 16 – Page 14, Line 4: There are many publications on volumes of gas and solids in wedge ice (and their contents are rather small), so it is much more important for your purposes to obtain the field data on size and morphology of ice wedges specific for your sample areas – these numbers are very variable and may affect your IW volume estimations much stronger.

The authors agree with this comment. Our ice wedge volume estimates are first approximations and that the variability in gas and sediment inclusion volumes as a factor in the estimate of ice wedge volumes is not realistic in a study of this nature. The text has been changed to emphasize this point:

"IWs may contain gas inclusions, small amounts of sediment as disseminated grains and discontinuous veins of silt and fine sand (French, 2007). Inclusion of this factor in our volume calculation is not realistic for this first approximation study so it was assumed that IW were all composed of pure ice. This has also been assumed by Ulrich et al. (2014) and most previous studies (e.g. Pollard and French, 1980; Couture and Pollard, 1998; Bode et al., 2008)."

Page 14, Lines 11-13: I recommend to cite Jorgenson et al., 2006, 2015 here (they describe ice-wedge dynamics and related positive and negative ecological feedbacks).

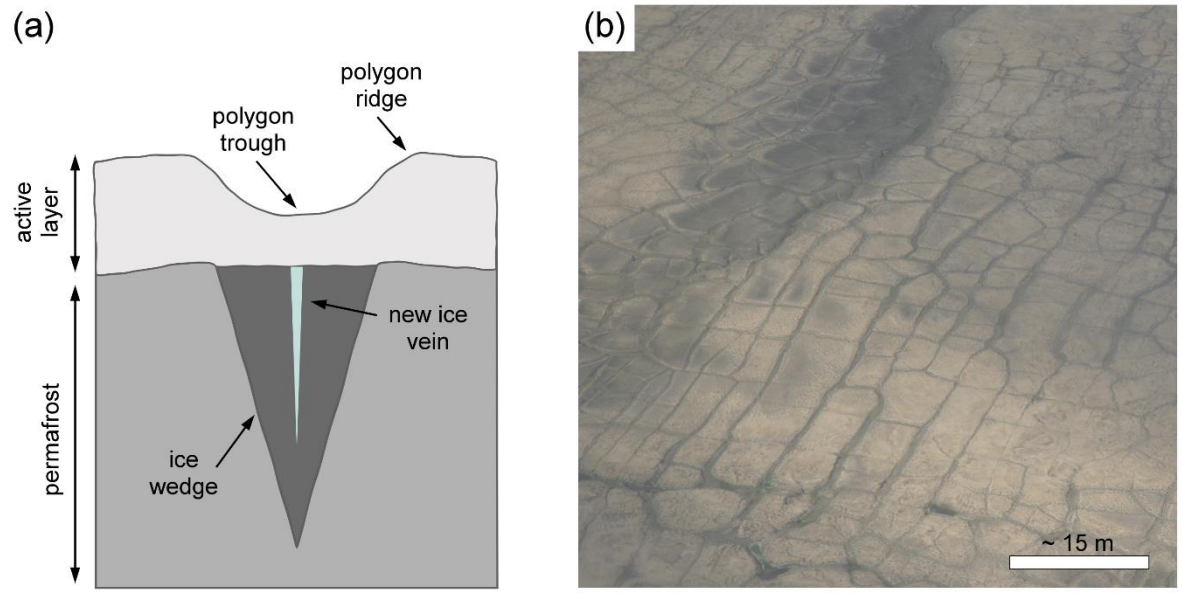
These references have been added to the specific sentence. However, since our research focuses on high latitude cold polar deserts the processes occurring in lower latitude warmer tussock tundra, (e.g. Alaska) are not entirely similar but nonetheless relevant.

Figure 1: What do you mean by length?

Length was changed to depth. It was meant ice wedge depth.

Figure 2: Most of ice-wedge polygons in your study area are high centered (see Figs. 2b and 4) but in Fig. 2a you show well-developed ridges typical of low-centered polygons.

The authors understand that from a certain perspective (there is no scale) this diagram could be confusing because of the surface of the active layer going down after the ridges, which is typical of low centered polygons. A small ridge can be seen in high-centered polygon in our area of interest but with no ponding in the middle of the trough, which would also be typical of low-centered polygons. The diagram and the caption have been modified as follows:



"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 4. Please change colors: it should be yellow for manual and blue for semiautomated methods.

Changes made as suggested.

Figure 7. This figure strongly resembles Figure 7 in Ulrich et al., 2014. I understand that you have already mentioned in the text that you follow their method (and this figure depicts your own sample area) but anyway I recommend to mention in the caption that this model was developed based on their approach.

(Figure 7 is now Figure 6). Mention of Ulrich et al. (2014) was added in the figure caption.

Technical corrections at the very end ("technical corrections": typing errors, etc.):

In the supplement to the comments:

https://www.the-cryosphere-discuss.net/tc-2018-29/tc-2018-29-RC2-supplement.pdf

We appreciate the thorough comments of the reviewer on sentence structure and typing errors. Most of those changes have been applied to the manuscript and improve its clarity.