

## 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 Anonymous Referee #1**

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

This paper by Bernard-Grand’Maison & Pollard aims (1) to develop and test semi-automated GIS methods for mapping ice-wedge polygons by the delineation of ice-wedge polygon troughs and (2) to estimate the ice-wedge ice volume for the Fosheim Peninsula on Ellesmere Island in the Canadian High Arctic by using high resolution satellite imagery and 3D subsurface models. Therefore they build upon a formerly published methodological GIS approach for ice-wedge volume calculations. The authors found that, in comparison to manual polygon trough delineation, their self-developed semi-automated polygon delineation approach based on a watershed segmentation algorithm provides generally better results in polygon geometry and thus allows more accurate ice-wedge volume calculations than mapping by Thiessen polygons at their study sites. Finally, they provide amounts of ice-wedge coverage and volume for the Fosheim peninsula, which are pretty close to previous estimated amounts of ice-wedge ice volume. Even if this methodology-focused paper based in large parts on a formerly published methodological approach it provides new insights and data of ground-ice conditions in the Canadian High Arctic and new possibilities in semi-automated methods by simple GIS analyses for mapping polygonal networks on large scale. This is in particular an important issue in thinking about time-efficient automatic methods for a Circum-Arctic mapping of ice-wedge polygonal networks in relation to the better understanding of ground-ice conditions, permafrost landscape sensitivity to thaw and not at least the large-scale calculations of permafrost carbon stocks. Overall, I think that’s a simple, short but nice methodological article. The simple and easily comprehensible GIS method is well presented in an easily accessible style. I have only a few suggestions and comments and can therefore imagine that this article will finally be published in The Cryosphere.

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

P2/L1: Please include “soil type” or an adequate term in the list of main permafrost and active layer controlling factors.

*The sentence now reads: “The main factors controlling permafrost occurrence and depth of the active layer are: air temperature, vegetation cover, soil type, snow cover and topography (French, 2007).”*

P2/L28-29: Do you talk about the High Arctic here? Please be more specific here. There are large areas in Siberia, for instance, in which syngenetic and epigenetic ice wedges equally widespread or even syngenetic ice wedges are representing the much larger proportion of ground ice.

*Specific reference to the Canadian High Arctic polar desert has been added in this sentence.*

P6/L12-16: What time period is used to refer to the information given here?

*The sentence has been changed to explicitly write the time period considered from the work in Couture and Pollard (2007): “They outlined two scenarios of +4.9 °C and +6.6 °C mean annual air temperature increase in the 2040-2060 period compared to mean annual air temperature from the 1948-1997 period.”*

P6/L22: Here and/or even somewhere in the results, I miss information about the polygon sizes at the different study sites. I realized there are mean polygon areas in the supplement information but something related to polygon diameter and its variance at the individual sites would be good to have within the main text.

Following comments from Referee #2, the supplementary tables have been deleted and the mean perimeter and area of the polygons for each method and sites have been added in the main text to Table 2. The authors agree that it would be best to have a measure of the variance. However, all the GIS files have been lost due to a hard drive malfunction and the polygons would have to be re-digitized and would not be the same as when the volume was originally calculated. A general idea of variance can be visualized in Figure 4, where the satellite images of the sites are shown. The authors then judge that the information provided in the manuscript is sufficient.

P12/L6-12: I wonder what will be the effect on the watershed segmentation method of more complex contrast differences in satellite data, for instance, with regard to existence of water bodies in the center and the troughs typical for low-centered tundra ice-wedge polygons. I think more discussion about the applicability of the methods (especially the novel water segmentation method) beyond the Canadian High Arctic would be useful.

The authors acknowledge that the Watershed Segmentation methodology might not be applicable as it was described for terrain with prominent vegetation and low centered polygons with water bodies inside the polygon center and in the troughs. The methodology could be applied using high-resolution Digital elevation models (DEMs) on non High-Arctic terrain instead of the pixel brightness values. The paragraph has been extended to clarify this point and merged to the second to next paragraph about potential use of DEMs, see P12/L9-14.

General on section 5.2: If possible, I would like to see a little bit more discussion here about polygon size and geometry differences as well as ice-wedge volumes in relation to the site-specific differences of the four study sites and vulnerability to thaw as done more generally in section 5.3.

The authors feel that discussing geometry relative to ice wedge volume at this point in the paper would be premature. Site-specific geometry is not influencing the ice wedge volume calculation because the ice wedge sizes are assumed to be the same (mean width and depth found by Couture and Pollard (1998)). However, the density of polygons at each site influences the ice wedge volume as mentioned in the classification of “high density” and “low density” from Couture and Pollard (1998) but this is a qualitative description.

P13/L28-30: Please could you provide references here?

The authors argue that a reference is not needed because this sentence refers to field observations from W. Pollard. To reduce confusion the text has been changed to: *“Based on nearly 20 years of fieldwork on the Fosheim Peninsula, we have found syngenetic IWs relatively uncommon, and limited to areas of active sedimentation like glacial forelands (floodplains), alluvial fans and deltas. Assuming that all IWs in the Fosheim Peninsula were epigenetic should therefore not affect largely our IW volume calculation.”*

P14/L3-4: And vice versa! There is not always a correspondingly large ice wedge below every crack and trough.

The authors agree and have now added this statement: *“The opposite is also true as there might not be an IW below every crack and trough, but our estimates can only be based on what is visible in the satellite imagery.”*

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

P10/L2: Please change "Manual" to "manual"

Changed as suggested.

P10/L10: "Watershed Segmentation" is sometimes capitalized, sometimes small or one big and the other small. Please be consistent in spelling. See also the figures and captions.

A consistent spelling of "*Watershed Segmentation*" has been applied to the whole manuscript.

Figure 4 top: Please change "Thiesen" to "Thiessen"

Changed as suggested.

Figure 5a: Please change "Bassin" to "Basin"

Changed as suggested.