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# *Interactive comment on* "Vulnerable top-of-permafrost ground ice indicated by remotely sensed late-season subsidence" *by* Simon Zwieback and Franz J. Meyer

**Anonymous Referee #1**

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Zwieback and Meyer studied the suitability of exploiting InSAR-based late-season subsidence measurements in northwestern Alaska as an indicator of excess ground ice at the top of permafrost. They present a great piece of work that shows the value of radar remote sensing to upscale ground ice mapping and identify potentially unstable terrain at large scale. This topic is relevant both for Arctic research and operational end-user exploitation. The paper is well suitable for a publication in The Cryosphere and likely to become a reference in remote sensing of permafrost. I have no major concern regarding the methods and main results, but I think the manuscript could benefit from some additional information, clarifications, and adjustments, especially in figures. These elements, so called 'main comments', are described thereafter. I also listed



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more technical suggestions, so called ‘complementary comments’, in the second part of the review.

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Main comments:

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Abstract:

- Some parts of the current version are not easy to understand without having read the whole paper (although this should be the point of an abstract). For ex l.6 ’For locations independently determined to be ice rich’: Maybe sth like ‘Compared to an independently-generated manual mapping of ice-rich and ice-poor areas, . . .’; l.7-8 ‘The distributions overlapped by 2%...’: the first time I read it, I understood ‘spatial distribution’ which obviously did not make sense. Maybe sth like: ‘the distributions of the late-season subsidence values in referenced ice-rich/ice-poor areas overlap by only 2%, which. . .’.

- Seems that it was a conscious decision all along the article not mentioning that the subsidence results are InSAR-based. I understand that the detection technique is not the main point here but still think it would be good to mention it a couple of times, at relevant locations. For instance, in the abstract (l.3) and conclusions (l.326).

- Somehow l.58-66 do a better job to clearly summarize the work. Could maybe be used to rework the abstract?

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Figures: I believe some figures could become easier to read with minor changes. Here are some suggestions:

- Figure 1 right: A bit confusing that subsidence is written on the left (y-axis) but subsidence-heave is indicated on the right (in addition with counter-intuitive directions).

Indicate that the black horizontal line in the center corresponds to 0 and maybe consider inversing the direction (heave upwards, subsidence downwards). Except if there is really a good reason not doing so, it would be much easier to read. Positive sign mentioned in the legend is anyway not shown in the figure and I guess (+/-) here is used to correspond to changes of sensor-to-ground distance, but it should not impact the visualization (if you imagine Sentinel-1 at the top of the page and the ground at the location of your horizontal straight line, an increase of distance goes downwards). Consider also having all text in black instead of light grey.

- Figure 2 left: What do the different colors of the spline lines actually indicate? Missing a legend.

- Figure 4: Maybe add an information about location. In legend: what does Catena mean here? Is it a place?

- Figure 5: As you refer to 5-8 cm/yr of typical values, I wonder if your subsidence maps would not benefit from a better contrast by reducing min/max values of your color scale (-10 to 10?). Or considering other colors or an asymmetric scale (I guess you do not have heave values up to -15). Add in legend somewhere that blue = no data in a) / b), i.e. under a certain coherence threshold I guess (which btw is not mentioned). Remind somewhere that late-season is 10 Aug. to 10 Sept. Here also the scale could be inversed without having to change the signs (heave as negative LOS values but upwards).

- Figures 6-7-8-10: Use cm instead of m, as you wrote and mapped everything using cm.

- Figure 6: Not clear what is the point having a) and b) as they are basically showing the same, as also described at l.200. Locations a-c) are actually pts 1-3, I guess? And they are shown in 7b, not 7d.

- Figure 7: to me, Figure 7a (with Figure 8) is the best part of your article. Especially





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the grey part with the indeterminate is great. It is a bit unfortunate that it is visually the part that we see the least (due to contrast).

- Figure 9: a) has also poor contrast: maybe because of the color choice it looks like a semi-transparent mask has been applied. b) topography would maybe be more useful as background. NIR-R-G does not bring much info here.

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Potential for additional maps:

- It is missing somewhere a map showing the total subsidence or the early-season subsidence only compared to late-season, as you are discussing it in the text. Maybe as supplement?

- As you show examples of time series in Figure 10 that are estimated as ice-poor/ice-rich pixels based on InSAR subsidence in intermediate mapped areas, why not do the exercise at full scale and finish the article showing a whole map ice-poor/ice-rich. Of course, it would be an indicator/proxy/estimate, with some uncertainties/limitations (well explained in section 5.2), especially in areas where the distribution overlaps (where it should probably remain 'undefined'). But that would beautifully close the loop, I think.

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Discussion:

This is a bit a mismatch between 5.2.2 and 5.3. With almost too negative statements in 5.2.2. (for ex l.280: 'late-season subsidence in a warm summer is not a perfect indicator of whether the top of the permafrost is ice rich or not') and almost too positive ones in 5.3 (for ex l. 310-312: 'late-season subsidence can be enhance the automated mapping of vulnerable permafrost ground ice' and 'the mapping can be automated, as no manual interpretation and no calibration using in-situ cores are required'). I understand the reason of both statements, but just think the text needs to be slightly more





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balanced. For example, in 5.2.2, it is obviously good to discuss the limitations but somehow the pretty negative first sentence is rather confusing at this stage. Consider rephrasing and starting by saying where/when it is likely to work (extremely warm summers when the thaw front penetrates substantially into the top permafrost or where the ice content at the very top is abundant) and then discuss the problems. In 5.3. as you say that incorporating geological constraints can counteract weaknesses (l.319), it is probably good to change a bit the sentences at l.312. It may be technically possible to automate but that does not fully replace the needs for manual interpretation and in-situ measurements. In general, I would say the main point is: ground ice maps based on remotely sensed subsidence are complementary to other techniques and can contribute to upscale the identification of potentially hazardous areas.

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Complementary comments:

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- L.35-36: could be rephrase to: . . . that the mapping identifies degradational features when it is already too late.

- In section 2: missing an information about active layer thickness. Documented in this region? What is the typical thickness and which variability? Would be a useful information when discussing the limitations in section 5.2.2.: one way to identify false negatives may be to estimate which subsidence is expected from the 'normal' thawing of the active and transient layers.

- L.74: missing reference.

- L.82: heavy sentence. Not clear what is 'with respect to the previous decade'

- In 3.1.1. Could mention somewhere that this is based on images acquired with an ascending geometry. LOS arrow could be shown on maps.





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- L.94: ref to Copernicus Sentinel should come in 3.1.1

- L.98: which DEM resolution?

- L.99: which multi-looking factors, and so which ground resolution?

- L.108: maybe mention here already that t1 actually is 10 August.

- L.123: this is the first (maybe only?) mention of the time period used to define with is late-season. Could be useful to remind it e.g. in figure legends, in conclusion. Maybe in the abstract as well.

- In 3.1.3: ref to figure where we see the points.

- In 3.1.4: What about the inter-annual variations in timing? Here the start-end of the late-season are fixed to 10 August / 10 September. May it become a problem when thinking about automation and processing of many years? It could perhaps be mentioned here or in the discussion.

- L.134: 'the sensitivity and specificity of the late-season subsidence indicator. . .' or 'the sensitivity and specificity of the late-season subsidence for ground ice mapping. . .'

- L.139: which percent is not mapped due to lack of unambiguous indicators? Could be mentioned as for the discarded areas, cause what we actually care is to know the percent that is documented (vs what is not in total).

- L.167: $ -> &

- L.169: I believe that Tatchim Isua has not been introduced yet so more info about where it is and a reference to a map would be welcome.

- L.187: weird to say 'peaks' when referring to a map. What about: 'The distribution of the observed late-season subsidence in our study area shows two distinct ranges of values'?

- L.188: later you mention 5-8 cm (e.g. l.196). Good to be consistent.

- L.216: ref to Supplement table?

- Figure 10: legend: '. . . from 2019 for point from Figure 7b).'

- L.225-231: this is especially where I thought: where is the final full-scale map showing the estimated distribution of ice rich / ice poor areas based on subsidence observations?

- L.243: 5-8 cm? A number 10 probably misplaced after 'late-season subsidence'

- L.254-259: this part if already focusing on limitations. Could be moved to 5.2.

- L.328: here comes another version of the range of values: 4-8 cm...

- L.338: could mention here again the likely difficulties in other kinds of environments (discontinuous permafrost, more vegetated).

- L.345: this point starts with 'Its'. Do not know what it is referring to. Points 4 and 5 could be merged.

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