This manuscript shows an original interpretation of InSAR data by isolating the lateseason subsidence of an extremely warm summer. Authors then link the high lateseason subsidence, significantly different from the low late-season subsidence of icepoor permafrost, to the degradation of the ice-rich top of permafrost. Although, there are limitations to this approach (well addressed by the authors), there is also a great value to spread the concept so the same idea can be tested in different permafrost environments, and perhaps in more complex settings. I support having this paper published in The Cryosphere. In general, the paper reads well, however, some suggestions are made for clarifications and additional information that can help improve the understanding and support the conclusion.

We are grateful to the reviewer for their insight and constructive suggestions which we are confident will help us improve the clarity and balance of the manuscript.

Below, I summarize my main comments, followed by specific comments and comments on Figures.

Main comments:

1. Transient layer versus intermediate layer
From the start (abstract), it would be important to bring the distinction between the transient and the intermediate layers (ice-rich top of permafrost) and to explicitly say to what the late-season subsidence is related to (ice-rich top of permafrost). There is some confusion in the introduction about this (lines 38-45 and line 55) and while you mainly associate the late-season subsidence to melting excess ice at the top of permafrost, you can't exclude the thawing of the transient layer as a contributing factor (as mentioned in your discussion). If possible, you should try to provide a description of the ice content within the transient layer as observed in the cores. In doing so, you can perhaps estimate the subsidence associated to the thawing of the transient layer and compare this estimate with the magnitude of late-season subsidence derived from Sentinel-1. This will give you arguments to support your conclusion.

We agree and plan to extend the discussion of the transient and the intermediate layer. Having said that, we also note that a process-based interpretation of the satellite observations is not a major goal of ours. We emphasize this point in the discussion, where we talk about the need for contemporaneous and dense ground-ice samples in order to reliably interpret the observations and to assess the potential to quantify ice contents (i.e., to move beyond the two-class taxonomy of ice-rich and ice-poor terrain).

We have added a separate paragraph in the introduction.

> The stratigraphy of permafrost-affected soils adds complexity to the link between upper-permafrost ice content and remotely sensed late-season subsidence. To describe the cryostratigraphy in ice-rich terrain, Shur et al. divide the long-term permafrost into three layers. The uppermost layer, the transient layer, generally has a low to moderate excess ice content, as a result of occasional deep thaw. Disappearance of the transient layer is frequently triggered by sustained warming or disturbance. The subjacent ice-rich intermediate layer is then exposed, increasing the susceptibility to enhanced subsidence. The risk of sustained thaw consolidation is amplified where the intermediate layer overlies massive ice such as ice wedges. Once the protection afforded by the transient and intermediate layer has been lost, further thaw will lead to ice wedge degradation. Ice wedge polygons also illustrate the large lateral variability in ground ice conditions, which need to be considered when interpreting late-season subsidence as an indicator of ice-rich upper permafrost.

We do not intend to change the figure because we feel these details would distract from the bigger picture.

We also plan changes to the discussion section. Most importantly, we intend to strengthen the discussion of how we suspect the transient layer contributed to the difference between 2018 and 2019:

> Inter-annual variability in late-season subsidence of ice-rich areas poses challenges for ground ice mapping. Potential sources of inter-annual variability and trends include surface changes (e.g., soil moisture, disturbance, snow) and variable meteorological conditions such as precipitation. Memory effects could also be relevant. Taking the ice-rich area in Figure 8a) as an example, we speculate that thaw of materials with moderate excess ice contents (transient layer) at the end of the warm summer of 2018 (limited late-season subsidence of ~ 2 cm) could have promoted larger subsidence in 2019 by weakening the protection given to the subjacent materials richer in excess ice (intermediate layer, massive ice). Equally, the summer of 2018 may not have been warm (and wet) enough to allow for reliable identification of the vulnerable ground ice at this location. That the identification strategy presupposes an initial degradation of ground ice constitutes its biggest limitation.

2. Selection of the beginning of the late-season subsidence The beginning of the late-season subsidence is mentioned quickly in the text (and too late, in line 123) and it is embedded in the caption of Figure 1. The definition of this lateseason (especially the beginning) is central to your results and you should discuss your choice of selecting the second week of August for your study area. Your late-season is quite large (1 month) and likely encompass thawing of the active layer, transient layer and ice-rich top of permafrost. Furthermore, for automated ground ice mapping, selection of the late-season period will likely vary across the North.

We agree with these points. We plan to add a figure to the supplement where we show Fig. 8 for shorter periods. For this study area and the year 2019, the difference between the temporal trajectories of ice-rich and ice-poor terrain is, for the most part, sufficiently large so that we will not expect a substantial impact of the specification of the period on the separability. But this may not always be the case, and we will strengthen the analysis in the discussion and the methods.

3. Resolution of 60 m and detection of subsidence associated to the degradation of ice wedge polygons

At several places in the manuscript, you make the association of ice-rich permafrost to the presence of ice wedge polygons; more precisely, you classify ice-rich permafrost based on visible ice wedges, but what about the ground ice content (excess ice) in the center of the polygons? My concern is about the resolution of 60 m used to derive the subsidence. Your manual classification of the ice-rich class seems to be based on visible ice wedges, but the InSAR result returns a 60 m pixel value where the subsidence is a mixture of various surfaces.

We agree that this is a major complicating factor that we failed to address in sufficient depth in the manuscript. To remedy this failure, our planned major changes are:

- Include a separate bullet point on this issue in the description of the ground ice mapping:

> A major caveat is that the presence of ice wedges provides no direct information on the ice content in the polygon interiors. However, previously taken cores (by Shannon and Wilson; to be discussed later}) from centres in various terrain units were ice rich, and the presence and ongoing expansion of thermokarst ponds and lakes also provides support for this assumption.

- Extend the discussion of the spatial variability

> A big challenge, the sub-resolution spatial variability of ground ice, is exemplified by ice-wedge polygons. To what extent do the satellite observations reflect the wedges and the polygon interior, and how does it vary with factors such as ice content of the centres and the thickness distribution of the protective layer above the wedges? Quantitative answers will require densely sampled ground ice cores

- Further minor qualifications in the discussion, for instance

> Site knowledge is indispensable for interpreting the stratigraphic complexity and sub-resolution variability of ground ice conditions

4. Result over the Kivalina study area

I understand that you want to present a simple case (focus area) to illustrate your approach. However, although not available for review, you seem to have, for your study area (Kivalina), the ground ice map in term of ice-rich, ice-poor, and indeterminate classes. Why then you did not provide the assessment for your entire study area?

We first apologize for uploading the data to the repository without granting access. This has been fixed.

The independent map was only derived for the focus area because of the laborious nature of the manual mapping (> 3 days for > 60 km^2). In the methods section:

The mapped focus area, 8 km by 8 km in size, was chosen because of the wide range of ecotypes and the availability of field observations.

Specific comments:

We have made numerous smaller changes. We have grouped several of these minor comments to streamline our response.

Line 3 : Could you define the late-season period?
Line 5 : Make the distinction between the transient layer and the intermediate layer (ice-rich top of permafrost).
Line 6: Please consider rewording "For locations independently determined to be icerich" which is a little hard to understand at this stage.
Line 7: Is it also the 5th–95th percentile for the range of subsidence of ice-poor areas? Please add if so.

We have clarified these points in the abstract. We prefer not to introduce the transient/intermediate layer model at this stage because we want to focus on the big picture.

Lines 25-27: "Current approaches for mapping ground ice have significant shortcomings. One approach, palaeogeographic modelling of ground ice aggradation and degradation, is currently limited to coarse-scale assessments (Jorgenson et al., 2008; O'Neill et al., 2019)". True, but in fact, it is not the approach that is limited, but the input data that limits the result of such approach (in particular, the scale of the surficial geology used. In O'Neill et al. 2019, the surficial geology is at 1:5000000. As stated by O'Neill et al. 2019, the model output could be improved by including updated surficial geology mapping). Please consider rewording.

Agreed.

Maps obtained from palaeogeographic modelling of ground ice aggradation and degradation, are currently limited to coarse scales

Lines 38-45: It is somehow understood here that you relate the late-season subsidence to thawing of ice-rich top of permafrost (intermediate layer) and not to the thawing of the transient layer. However, you can be clearer by adding the transient layer and intermediate layer in your schematic of Figure 1 and by explicitly say to what the lateseason subsidence is related to.
Line 55: In contrast to previous statement (in lines 38-45), you seem here to relate late-season subsidence to both "excess ice at the base of the active layer and top of the permafrost". Please clarify.

Please see our response to point 1.

> Line 71: Please define the rubble-covered surfaces in term of material or terrain unit type.

Specified as well-drained uplands.

> Line 71: Please give the mean annual ground temperature of this warm permafrost and the range of active layer thicknesses with the approximate date of the maximum thaw front (see comment of line 108).

Added range of active layer thicknesses and permafrost temperature.

> Lines 78-79: "Ice-rich layers of segregated ice are also ubiquitous in fine-grained sediments (Shannon & Wilson, Inc., 2006)" Do you mean in marine sediments? Please clarify.

Clarified that they occur throughout the study area, including uplands. We do not want to engage in excessive speculation about the quaternary history of the study area because the geological reports available to us focus on geotechnical properties. They do include interpretations as to the history of several locations, but the various reports are often in disagreement and, in our personal opinion, not always compelling.

> Line 82 and Fig 2b: Please add the climatic normal (TDD) in Fig. 2b, so that the decade and summer 2019 will be put into a longer perspective.
> Lines 80-85: Could you also define the climate of the three years (or summers) in term of precipitations? That may help understand the soil moisture content at the end of the summers and support some discussion points (e.g. soil moisture used to aggrade ice at the base of the active layer, line 298). In addition, Douglas et al. (2020) recently shown the relationships between anomalously wet summers and thaw depth in discontinuous permafrost (Alaska). Therefore, not only extremely warm summers can lead to degradation of vulnerable excess ice in the upper permafrost, but extremely wet summers as well.

We will add the precipitation observations that are available. It appears that 2019 was indeed a wet summer, but it is difficult to make comparisons because of the gaps the observations contain. We will also add the 30-year TDD average. We have further strengthened the discussion section by expanding the discussion of precipitation and soil moisture, and by adding references to the Douglas et al. paper and one by Shiklomanov et al. (continuous permafrost).

> Lines 89-92: Please add the resolution of Sentinel-1 interferometric wide mode. The resolution of 60 m is obtained by multi-looking (lines 98-99). This might be obvious to remote sensing experts, but not necessarily to permafrost experts. Please explain your choice for the resolution of 60 m.
> Lines 89-92: Could you add the looking direction of the satellite?
> Lines 98-99: Perhaps, briefly explain the purpose of multi-looking or link the multilooking to the 60-m resolution.

Done.

> Line 106: Can you show that there is no aspect-dependent trends that are associated with downslope movements in the supplementary file?

We believe the existing figures are sufficient to show that there are no strong aspect-dependent trends.

> Line 108: Could you define and discuss your choice for the beginning of the lateseason subsidence in the text. In Fig 1, you mention second week of August. When is the maximum

thaw front generally reached? Please add this information in the study area section (see comment of line 71).

Lines 113-115: Please refer to Fig.3b

Line 123: If not done previously, please explain why you choose August 10 as the starting point of your late-season. Your late-season is quite large (1 month) and likely encompass thawing of the active layer, transient layer and ice-rich top of permafrost.

Agreed. We have expanded this section, drawing attention to our new sensitivity analyses (see above) and spelling out the key considerations relevant to selecting the dates.

Line 132: Please add a reference to your ground ice map. However, and unfortunately, the independently derived ground ice map (Zwieback, 2020a) is not available for review(see comment of lines 515-516).

Done. We apologize that the map was previously inaccessible. This error has been fixed.

Line 155: Alluvial deposit seems to be classify as ice-rich based on visible ice wedge polygons, what about the ground ice content in the center of the polygons, can you add this information?

Agreed. See point 4.

Line 168: Can you specify the surficial geology of all sites in Shannon & Wilson, Inc., 2006 since the readers don't have access to this reference? Perhaps add the information in Table S1.

We have extended the information provided in Table S1. We have added descriptions of the surficial geology, ice wedge polygons, topographic position and, where it could be classified with reasonable confidence, surficial geology.

Lines 168-170: Again, does ice-rich permafrost associated to ice wedge polygons only?

Added clarification about where the cores were taken in relation to ice wedges.

Line 175: Even if you acknowledge that the cores were taken in 2005 compared with Sentinel-1 over summers 2017-2019, you should try to provide a description about the ice content of the transient layer as observed in 2005.

Unfortunately, we are unable to do so because the report does not provide sufficiently granular information. For instance, for core 05-5, the soil below the thaw front is described in intervals of > 0.5 m (the top one reads: Organic SILT and ice-rich SILT; frozen.).

We plan to take our own cores next summer for more detailed studies.

Line 182: Please correct Table S1, it should be Cores from 2005, not 2015.

Fixed

Lines 179-181: Could you specify if ground ice contents represent the first meter below top of permafrost? Perhaps add the depth interval corresponding to the ground ice description in Table S1.

Unfortunately, the report does not contain measurements of excess or total gravimetric ground ice. Estimates of the visible ice content are not provided for all sections of all cores, even those that are ice rich (e.g., Organic SILT and ice-rich SILT; frozen; ICE with gray silt inclusions.).
We have extended the description of the cores and their location in table S1 (see above).

> Line 187: Not sure if the use of the word "peak" to describe spatial variability is appropriate since it can be confused with a temporal peak. Please consider rewording.

Fixed.

> Line 189: For negative late-season subsidence, do you mean in 2017 or 2019 or both?

Clarified.

> Line 189: Do you mean uplift displacement and/or displacement toward the satellite? Please clarify. Did you add this sentence to support lines 105-106 about downslope movement? Please clarify.

Simplified to "There were no notable instances of pronounced negative estimates in any of the years. " to avoid invoking the slight ambiguity of the term subsidence.

> Line 200: Again, it looks like the ice-rich deposits are only associated to the presence of ice wedge, and therefore, associating the subsidence to ice wedge degradation, however, at a resolution of 60 m, subsidence will rather reflect the one in the polygon center or at least be an average of polygon center and trough.

We have removed the reference to the ice wedges, instead specifying that these locations were identified as ice rich in our independent map. We pick up the interpretation of the results in the discussion section.

> Line 207: In your manuscript, you chose to shown only the results over the limited focus area. However, your manual classification into ice-rich and ice-poor terrain (and indeterminate category) was done for the study area of Kivalina (Zwieback, 2020a – unfortunately unavailable). It would have been interesting (perhaps at the end of your result section) to provide the distribution of the late-season subsidence over the Kivalina study area, such that someone can appreciate the performance of the approach compared to a small, almost ideal case study.

We first apologize for uploading the data to the repository without granting access. This has been fixed.

The independent map was only derived for the focus area because of the laborious nature of the manual mapping (> 3 days for > 60 km^2). In the methods section:

> The mapped focus area, 8 km by 8 km in size, was chosen because of the wide range of ecotypes and the availability of field observations.

> Line 207: Even if the difference in 2017 between ice-rich and ice-poor areas is smaller than 2019, is it statistically different?

We are not entirely sure whether the referee is referring to the difference between years or between ice-rich and ice-poor areas. We avoid questions of statistical significance because the appropriate underlying population (to which the repetition intrinsic to significance testing refers) is not self-evident.

> Line 209: Is it also the 5th–95th percentile for the range of subsidence of ice-poor areas? Please add if so.

Fixed.

Lines 211-213: Should you also discuss 2017 and 2018? You can perhaps quickly say that the suitability of cooler years will be address in section 4.2.2

We will tweak this passage.

Line 234: What the distribution overlap in 2017? Even with large overlap, is it statistically different?

See line 207

Line 248: It is true that the transient layer contains less ice than the underlying intermediate layer (Shur et al., 2005) commonly known as the ice-rich top of permafrost.However, the transient layer could contain excess ice that will lead to late-season subsidence (you refer to this in lines 297-298). From the coring done in 2005, is there any indication of the thickness of the transient layer and its ice content? If yes, you can perhaps estimate the subsidence of the transient layer and compare with the magnitude of late-season subsidence derived from Sentinel-1.

Unfortunately, the granularity of the reported results is insufficient for such purposes. Please see our response to the first point on how we improved the discussion of the transition zone.

Line 274: Please specify the magnitude of this spurious heave signal?

Done.

Lines 282-283: Do you infer than the warm summer of 2018 was enough to thaw the transient layer? Please clarify.
Lines 297-298: See comment of line 248 about the ice content of the transient layer from the 2005 cores.

Please see our response to the first point on how we improved the discussion of the transition zone.

Line 310: This automated ground ice mapping presuppose the selection of a starting date for the late-season subsidence; this starting date (and the end) will vary across the North. As mentioned previously, you should consider adding a discussion on the choice, for your study area, of the starting date for the late-season subsidence.

We have added

> To automate the specification of the late-season period, globally available reanalysis data could be considered.

Line 312: However, frozen cores will be needed for the interpretation of ground ice content (e.g., in the transient layer, not only to estimate excess ice at depth – line 321) as well as other ground truthing to reduce observational uncertainties. Please consider nuancing your sentence and moving lines 319-322 after the first paragraph.

We agree that the suggested structure works better. We further plan to add the following qualifying statement

> Incorporating geological constraints and expertise will be essential to counteract weaknesses of remotely sensed late-season subsidence. Site knowledge is indispensable for interpreting the stratigraphic complexity and sub-resolution variability of ground ice conditions. Most importantly, observational constraints will be needed to estimate total excess ice contents.

Line 316: How, with the resolution of Sentinel-1, the results can show subsidence of ice wedges?

We have made several changes, albeit prior to this sentence. Please see point 4.

Lines 515-516: These two products are unavailable to review:
Zwieback, S.: Kivalina ground ice map (Version 1.0), https://doi.org/10.5281/zenodo.4072407, 2020a. Zwieback, S.: Kivalina subsidence observations (Version 1.0), https://doi.org/10.5281/zenodo.4072257, 2020b.
Is it possible to consider adding them in the supplementary file?

We apologize for not having made the data accessible. We have now made them publicly available.

Figures

General: I suggest changing the color of your late-season subsidence scale; the blueish color is not the best to see the contrast, everything looks the same. You can maybe try the brown to blue, you used at Figure 5a-b, but only for subsidence (positive values). This scale will also be consistent with the colors chosen at Figure 7c, and choose another scale of colors for any heave displacement (negative values in Figure 5 a-b).

We will adjust the color scales and the ranges to improve the contrast.

Figure 1: Please consider adding the transient layer and intermediate layer in your schematic of Figure 1.

We will experiment with it. Our concerns are that it may clutter the figure and exacerbate already existing issues to do with neglecting the spatial variability.

Figure 2: Please add a horizontal line corresponding to the climatic normal; 30-year average in term of TDD.

We will add it.

Figure 3: Not sure if Fig. 3a and 3b go together. The figure title does not really represent Fig. 3b? Could they be separate figures?

We agree that the juxtaposition is not ideal, but we contend that both pertain to how (accurately) late-season subsidence can be extracted from the data.

Figure 4: Please locate these two sub-regions within Figure 4 or Figure 2 and add an arrow for the North. Also, please define Catena.

We will remove the word catena and mark the location of sub-regions.

Figure 5: Please add the reference of Figure 2 for the study area shown in Figure 5 "Regional variability of remotely sensed late-season subsidence dl within the study area (see Figure 2 for location)"

Agreed.

Figure 6: Move this Figure after its first mention in the text, after line 199.

Agreed.

Figure 7: Please add the reference of Figure 5 for the focus area shown in Figure 7 ": : :determined by manual mapping in the focus area (see Figure 5 for location)"
Figure 7: Please consider rewording "a) Versus independent ground ice map and cores" (it took me some time to understand the meaning of that sentence). Suggestion: For example, "a) Distributions of late-season subsidence in 2019 according to the three ground ice classes manually/independently mapped"
Figure 7: I like Fig. 7a, but I don't understand why the distribution has to be in both direction, can it just be a "positive" distribution? I was trying to understand the "lense" effect.

Figure 7: "The diamonds indicate points mentioned in the text" this is rather vague, could you say "for points mentioned in Figure 10"?

Figure 8: I like this figure!

We have amended the caption. In regard to the "lense" effect: we show the negative values in Fig. 7a, and also in Fig. 8, because they will need to be considered in any ground ice mapping effort. Here, the magnitudes of the negative values are generally within the observational uncertainty.

Figure 9: Please refer to Figure 5 for site location (after the mention of Tatchim Isua site).

Agreed.

Figure 10: Why points 4 to 19 while in Fig 7b-d it is points 1 to 17 with points 11, 18, and 19 missing? Please explain.

Time series for points 1-3 are shown in Fig. 6 (for all three years). Points 11, 18, 19 are outside the focus area and have now been added to Fig. 5.

Figure 10: point 4, independently determined as indeterminate not ice rich, please explain or correct.

Thank you for pointing out this error. We mixed up two different points in our database.

Figure 10: point 17, looks more ice poor than indeterminate according to manual mapping, please explain or correct

We agree that the fact that we assigned it to the indeterminate class is not readily apparent at this scale (not aided by the fact that the large marker obscures an area of substantial extent). The location is in the middle of a narrow strip of vegetated and soil-covered upland terrain that protrudes into the bedrock outcrop/rubble-covered ridge. We will consider choosing a different point to avoid this issue.