Responses to Reviewer #2

AUTHORS: We thank the reviewer for his/her insightful and constructive comments. We have addressed all of them and made the suggested changes in the new version of our manuscript. Please refer to the attached pdf for our point-by-point responses (in black) to the critical comments (in blue). Please note that the page/line numbers in our responses refer to the new line numbers.

The main critics of this manuscript is partly not correct and clear interpretation of permafrost related processes. Thus, the chapter 2 (Key processes for surface vertical movement. . .) is intended to explain processes in permafrost but instead confusing the reader due to lack of structure and logical flow. I suggest to either include a permafrost expert as a co-author or at least to consult one regarding the explanations of the processes in permafrost and results of this study. You might include a concise version of this chapter in the introduction, especially considering that you start the paper with the short mentioning of the processes leading to surface lowering and uplift.
AUTHORS: We rewrote Section 2 after the reviewer's suggestions (see more specific responses below). The very first two sentences in Section 1 give a brief introduction to the key processes, quoted below:
"Over permafrost terrains the ground surface undergoes seasonal vertical deformation due to the water/ice phase changes occurring in annual freeze/thaw cycles. Superimposed on the seasonal cycle, inter-annual and long-term changes of ground surface elevation may occur due to permafrost degradation/aggradation and subsurface water migration" (Page 1, Lines 21-23).

Some of the statements in the chapter 2 I found controversial:
"However, such an uplift due to pore ice formation is not 'frost heave' as referred to by permafrost scientists." Do you disagree with "permafrost scientists" on the term or do you want to distinguish two processes?
AUTHORS: To avoid potential confusion, we completely rewrote this sentence as:
"Ice segregation near the base of the active layer results in total frost heave that exceeds the potential 9% volume expansion of all the water in the active layer" (Page 3, Lines 9-10).

"In cold winters or cold summers, migrational water can form massive ice bodies within the transition layer, which becomes thicker and causes surface uplift". How can any ice form in summer? But also the coldness of the winter should not make a difference for the ice formation – any winter in your study area is cold enough to form ice in the active layer. Having permafrost directly beneath active layer, amount of available water to form segregation ice is defined by the thickness of active layer. Also, what is meant by massive ice bodies? Massive ice wedges take thousands of years to form.
AUTHORS: We agree that some terms were confusing or incorrect. We completely rewrote this sentence as "Reversely, in the following thaw season, pore and segregated ice within the active layer melts, volume decreases and thaw consolidation causes the ground to settle" (Page 3, Lines 10-12).

"Thaw subsidence due to permafrost degradation is gradual and homogeneous at regional scales". Also confusing, because all the rapid and irregular "subsidence", i.e. thermokarst, slumps etc, occur due to permafrost degradation as well. Likely, you mean the term subsidence only in a sense of gradual and homogeneous lowering of the surface, but confused in terms.
AUTHORS: We rewrote this paragraph by first introducing thermokarst-related subsidence and then gradual and homogenous subsidence. The revised sentences now read as: "In areas where the near-surface permafrost is ice-rich, thermokarst processes would initiate at local scales upon thawing, causing abrupt and deep thaw as well as strong and irregular surface subsidence (Jorgenson, 2013). Recent observations from campaign GPS and InSAR reveal that thaw subsidence due to permafrost degradation can also occur gradually (a few millimeters per year) and relatively homogenously at regional scales (Liu et al, 2010; Shiklomanov et al., 2013; Streletskiy et al., 2016)" (Page 3, Lines 24-28).

Another critical point in this study is the modelling of subsidence due to pore ice melt without knowing the active layer depth and soil properties (i.e. porosity) for the study site. Were ALT measurements at the study site conducted at the same time as CALM measurements in 2016? Generally, do I understand correctly that these calculations are made only to show that the overall subsidence is larger than subsidence only due to ice-water phase transition in soil pores?

AUTHORS: The ALT measurements were conducted on August 16 2016 at this site and August 19 2016 (three days later) at the CALM grid. We added this information (Page 4, Line 20).

Yes, our calculations are to show that the observed seasonal subsidence is larger than the subsidence only due to melt of pore ice and attribute the residual as the contribution from segregated ice. This idea is first introduced in Section 2, then more explicitly in Section 4.4. The quantitative results are presented in Section 5.4.

Critical for me is the extrapolation of the best fit to the month of June for each year. I wonder if the station is equipped with time lapse camera to track the snow conditions on the ground directly? In case it is, why not to use the real GPS data for the entire thaw season? Did you check GPS signal for June anyway? How does it look? Is it possible to see if the signal is affected by snow or not?

AUTHORS: We did check the snow cover at SG27 in two ways. First, PBO H2O published the snow depth at SG27 retrieved by GPS-IR (http://xenon.colorado.edu/portal/index.php?station=sg27). Figure R1 below shows the retrieved snow depth between May and August in each year from 2004 to 2015. This set of records indicates that snow-free days started in mid to late June. We don't include this plot in the manuscript.

Also, a recent paper by Cox et al. (BAMS, 2017) showed the 1987–2016 climatological mean and indicated that the snow-free days typically last from July to mid-August. We added this information and the citation in Section 3 "GPS station SG27 and permafrost conditions" (Page 4, Line 14).

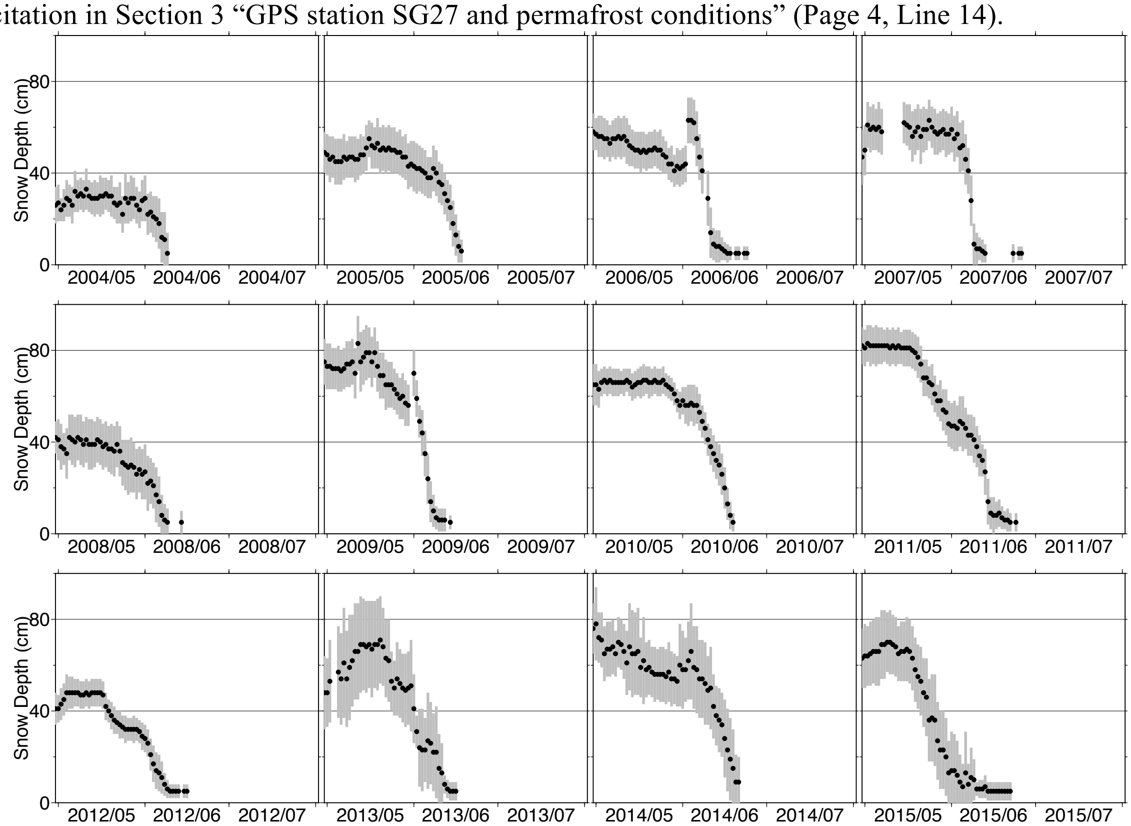

Figure R1: Snow depth retrieved using GPS-IR at SG27. Gray bars denote the uncertainties.

Maybe besides soil moisture there is also a ground temperature sensor installed within the study site? This kind of data would also be helpful to track the beginning and the end of the thaw season.

AUTHORS: The Barrow CALM soil-climate site "U1-1" also measured ground temperature. But the temperature data provided through the CALM webpage only span 1998 to 2011 (https://www2.gwu.edu/~calm/data/webforms/u1_f.htm).

Figure R2 below shows the ground temperature at 5 cm from 2004 to 2011 (the first eight years of our study period), measured at U1-1. The thawing period at 5 cm in each year lasted from early June to early September. Surface (0 cm) freezes earlier than 5 cm depth. And according to Shiklomanov et al. (2010), the thaw season is from early June to late August (Page 4, Line 13). We don't include this ground temperature plot in the manuscript.

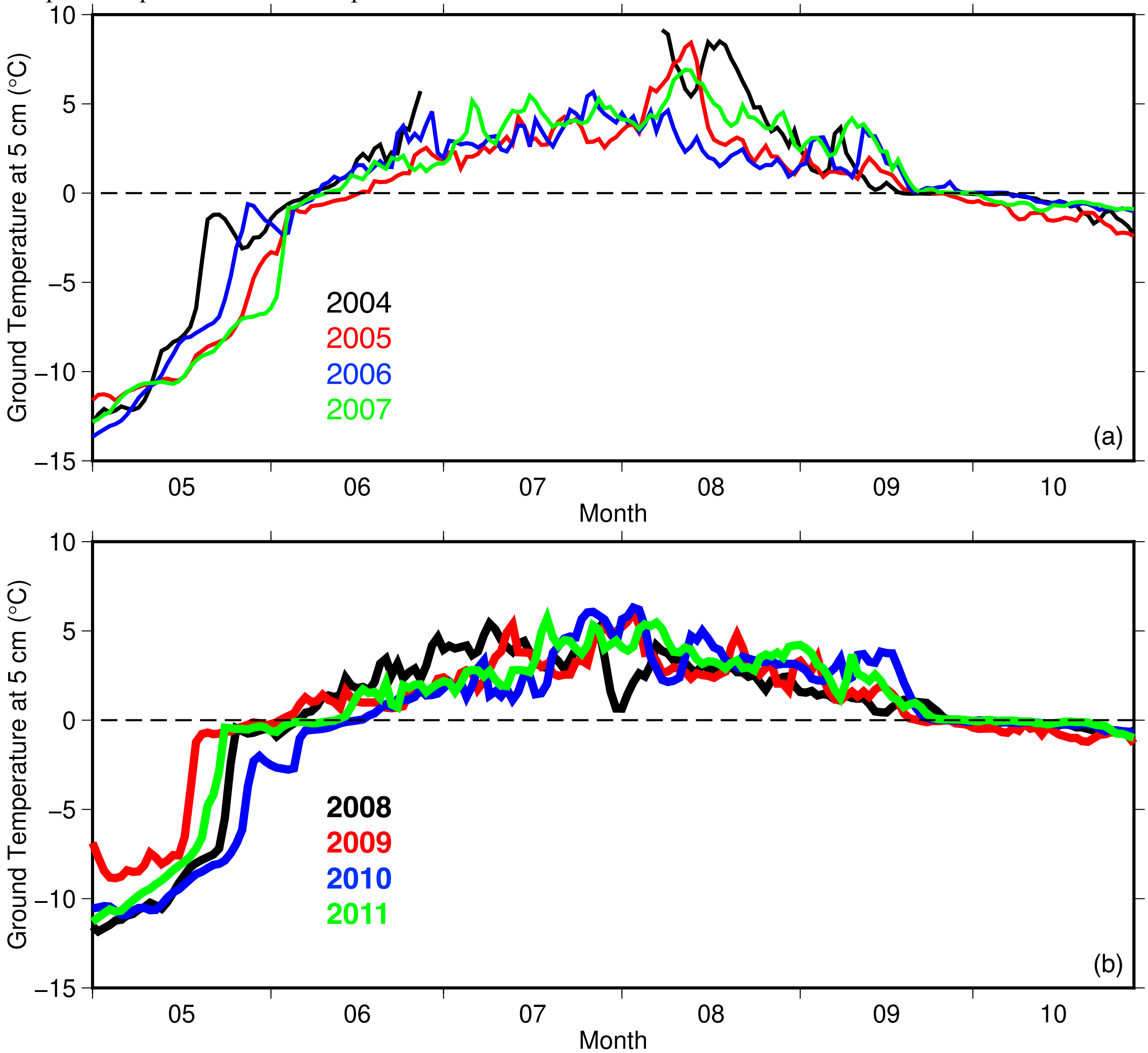

Figure R2: Ground temperature at 5 cm at the Barrow CALM soil-climate site "U1-1".

Soil moisture data is used to check moisture influence on the GPS signal but also could be used in an attempt to explain the difference in subsidence (heave) magnitude instead of a suggestion that the winters

“were not particularly cold” (p.12, lines 10-15). As mentioned before, the coldness of the winter should not influence the amount of heave in this case.
AUTHORS: Due to lack of soil moisture data throughout the complete study period, we use the cumulative precipitation in August as a proxy for excess soil water before freezing to test the correlation between seasonal subsidence and soil wetness.

As suggested, we now compare the subsidence with the precipitation in the previous August (new Figure 6). We observe two distinct groups: for seasonal subsidence that are larger than 5 cm, they increase nearly linearly with the precipitation, which confirms our hypothesis; yet for small subsidence (around 2 cm), they are independent of the precipitation (Page 15, Line 13 to Page 16, Line 3).

I see some potential for the structure improvement: Study area and datasets could be outlined by separate chapters; datasets can be described more rigorously using subchapters, e.g. (i) H from GPS, (ii) meteo data (DDT, DDF) and ALT data, (iii) DGPS data from Streletsky et al. 2016, (iv) soil moisture data, etc. Large part of the chapter 5.2 can be moved to Methods.
AUTHORS: As suggested, we added a subsection 4.1 ‘Datasets’ to list the data used in this study (Page 6), including:
4.1.1 GPS data from SG27
4.1.2 Surface elevation changes from the GPS campaigns of Streletskiy et al. (2016)
4.1.3 Soil and meteorological data
We moved relevant parts to this new subsection.

The results of Streletsky et al., 2016 are used very extensively in this study. Please state more clearly in the very beginning the intention to compare the results (i.e. in the end of Introduction) and introduce the data from their study in a separate subchapter as mentioned in the previous point.
AUTHORS: As suggested, we now explicitly state in the end of introduction that “We will show that our observed inter-annual and decadal elevation changes match well with the GPS campaign observations from Streletskiy et al. (2016) at a nearby site” (Page 2, Lines 27-28). We also added a subsection 4.1.2 “Surface elevation changes from the GPS campaigns of Streletskiy et al. (2016)” to introduce the work of Streletskiy et al., 2016 (Page 6, Lines 14-19).

p.2 line 3 and further on: Please check if it should be DGPS instead of GPS?
AUTHORS: Throughout the manuscript, we refer to the works of Little et al. (2003), Shiklomanov et al. (2013) and Streletskiy et al. (2016) as GPS campaigns, to distinguish from using continuously-operating GPS systems. We only explicitly use the term “differential GPS” when we mention the first of these series of work, i.e., Little et al. (2003) (Page 2, Line 3).

p.2 line 6-7: I think providing numbers here just for two years is too specific.
AUTHORS: As suggested, we deleted the sentence “They reported a surface uplift of up to 6.7 cm between July 2001 and June 2002, and a subsequent subsidence of up to 2 cm between June and August 2002.”

p.2 line 10: do you mean here that the measurements do not allow to monitor seasonal subsidence but only interannual? I would specify here.
AUTHORS: As suggested, we specify these the measurements “do not allow one to measure seasonal changes” (Page 2, Line 9).

p.2 line 10: p.2 line 14: “However, it still suffers from relatively long sampling intervals (about once per month) and loss of interferometric coherence for longer time series analysis.” What about TerraSAR-X/TanDEM-X and Sentinel-1 with 11 and 6 day intervals? Loss of coherence is crucial for the longer

time span between observations while time series can be long and nevertheless consist of short revisit time observations. Please reformulate. I would also add the problem of atmospheric phase delay.
AUTHORS: We rewrote the sentence as "However, InSAR suffers from relatively long repeat intervals (6 to 46 days, depending on the satellite platforms) and loss of interferometric coherence for mapping multiple-year changes over permafrost areas" (Page 2, Lines 12-14).

p.2 line 19: "However, these measurements typically only have annual or multi-year intervals, and the accuracy of elevation changes are on the order of sub-meters." I think the measurements can be repeated more frequently, especially in case of satellite acquisitions, but the problem is exactly in the accuracy which allows to detect changes on the multiyear scale only. Another problem is the expensiveness of LiDAR campaigns.
AUTHORS: As part of a brief literature review on remote sensing methods, we only briefly summarize the intervals and accuracy of the LiDAR and photogrammetric measurements, mainly for comparing with other methods and later introducing the motivation of using GPS-IR. We chose not to give suggestions what should be done or mention the cost of LiDAR campaigns.

p.2 line 23-24: ". . .and are campaign studies that only spanned a few days up to a few years". Not clear what is meant, please reformulate. Please also add some thoughts about soil moisture and vegetation which interfere with most of the remote sensing observations of elevation change.
AUTHORS: We rewrote the sentence as "However, most of these field campaigns have been focusing on slope movements, for instance, rock glacier flow and retrogressive thaw slumps" (Page 2, Line 22).

As explained above in the response, the brief review about the remote sensing methods is to set up the stage for GPS-IR. We chose not to discuss the effects of soil moisture and vegetation on remote sensing observations. We do quantify the soil moisture effects on GPS-IR though (see Sections 4.6 and 5.5).

p.4 line 1: Is it possible to add the information on how dense is the network of such GPS receivers in the region? E.g., over Alaska?
AUTHORS: We added a map to show the PBO GPS stations in the permafrost area of Alaska (Figure 1b, Page 5). Based on the circum-polar permafrost map of Brown et al. (1997), 58 Alaskan PBO stations are located in permafrost areas. Among these, 14 and 19 sites are underlain by continuous and discontinuous permafrost, respectively (Page 4, Lines 5-7).

p.4 line 14: reference.
p.4 line 17: reference.
AUTHORS: We added Shiklomanov et al. (2010) as a reference for the typical duration of thaw seasons (Page 4, Line 13) and for the active layer soil composition and saturation status (Page 4, Line 15).

p.4 line 18: When exactly the ALT measurements were made?
AUTHORS: August 16 2016 (Page 4, Line 20).

p.5 line 5: What is the resolution of the relief map?
AUTHORS: 0.5 m (Page 25, Line 1). Since this is not pertinent, we don't specify the resolution in the figure caption.

p.5 line 8: When the main photograph (with snow) was made?
AUTHORS: The date is unknown. The original photo is available from https://www.esrl.noaa.gov/gmd/obop/brw/gallery/old_pictures/index.html

p.6 line 3: 53 cm thick in 2016.
AUTHORS: We added "in August 2016" as suggested (Page 7, Line 9).

p.6 line 13: might need more detailed explanation or at least reference.
AUTHORS: We added Press et al. (1996) as a reference to the Lomb-Scargle spectral analysis (Page 8, Line 3).

p.7 line 14: reformulate please the first sentence.
AUTHORS: We deleted this sentence.

What means "issue" in this case? p.7 line 27: why "reiterating"? I don't think it was mentioned before.
AUTHORS: We changed this sentence to "It is worth pointing out that …" (Page 8, Line 21).

p.8 line 2: what is the reason for the steady subsidence trend? Also there should be a reference for the surface mass loading contributions. What is the seasonal subsidence in the case of solid earth? What about isostatic rebound? Also, why reporting results in this chapter?
AUTHORS: We added two references, van Dam et al. (1994) and van Dam et al. (2001), for the surface mass loading on solid earth (Page 11, Line 12).

According to ICE-6G, a global glacial isostatic adjustment (GIA) model, the predicted GIA vertical displacement rate at SG27 is 0.78 mm/year (positive means subsidence). (http://www.atmosp.physics.utoronto.ca/~peltier/datasets/Ice6G_C_VM5a_O512/GS_Hor_Vert_vel.ICE6G_C_VM5a_O512.txt)
We do not need to introduce GIA to the TC readers. As explained in the manuscript, the use of reflector height from GPS-IR conveniently excludes the contribution from solid-earth movements (Page 8, Lines 30-31).

We moved the results of solid earth movements, including Figure 3, to a new result section "5.1 Changes of receiver position due to solid earth dynamics" (Page 11).

p.8 formula 2: should the H be actually the change of H?
AUTHORS: Following our derivation that leads to equation (2), it is correct to use expression $H(t)$. And $H(t)$ explicitly indicates that H is changing with time.

p.8 line 22: what means forward manner?
AUTHORS: We mean forward modeling. We deleted 'forward manner' as it is redundant and potentially confusing.

p.8 line 26: what about the outflow of the water? Worth to mention.
AUTHORS: We added that "in this flat area, surface runoff is negligible and can be ignored" (Page 9, Lines 15-16).

p.9 line 5: what are the massive cryogenic structures within the active layer? I think terms are confused again.
AUTHORS: We changed "massive cryogenic structures" to "segregated ice" (Page 9, Line 14).

p.10 line 6-7: "Because surface subsidence is fast in early thaw season and gradually slows down toward the end of the thaw. . ." I would say it's not that straight forward. It also kind of contradicts with your own suggestion about ice rich transition layer at the bottom of the active layer, which may thaw in the end of the season leading to increase of subsidence. Anyway, it is true that it is important to consider the subsidence occurred in in the beginning of the thaw season.

AUTHORS: We rephrased the sentence as “Because surface subsidence can be rapid in early thaw season, this extrapolation is important if one needs to consider the net change during the entire thaw season” (Page 10, Lines 18-20).

p.10 line 12: how the unreliable estimates were defined?
AUTHORS: Only two reflector height estimates (DOY 205 and DOY 231, both in 2004) were excluded in our continuous daily time series (a total of 731 values). The DOY 205 SNR data were missing from the source data file. The DOY 231 results didn’t meet quality-control requirements. We believe that technical details at this level are not of interest to the TC readers, therefore not include them in the manuscript.

p.10 line 15-16: there is no general rule actually if we look at every year. For example, year 2012 features the highest thaw index but seasonal subsidence is small. Years 2010 and 2011 have approximately similar thaw indices but subsidence magnitudes are very different. Thus, I would also add “mismatched” years into the description to avoid bias.
AUTHORS: We added that “the subsidence was comparatively small during a warm summer in 2012, deviating from the general correlation” (Page 12, Lines 7-8).

p.11 Figure 4: Is it right to use standard deviation having 4 observations? I think the range would be a better characteristic.
AUTHORS: As suggested, we now use the range instead of standard deviation (Figure 4a, Figure 5, and Figure 4 caption Page 13, Line 5).

p.12 line 1: “Our estimated surface elevation changes agree well with the in situ measurements made by Streletskiy et al. (2016). . .” I would add “generally” since some years featured mismatch.
AUTHORS: We added “generally” as suggested (Page 12, Line 13).

p.12 line 2: “. . .in an area dominated by ice wedges (~2 km southeast of SG27)”. If you describe the data from Streletskiy et al. (2016) in a separate chapter you don’t need to add information here in the Results.
AUTHORS: As suggested, we moved this information to section 4.1.2 “Surface elevation changes from the GPS campaigns of Streletskiy et al. (2016)” (Page 6, Line 16).

p. 12 line 3: Figure 4a, not 3a
AUTHORS: We fixed this referencing mistake.

p. 12 line 5-7: Is it justified to report the trend for the entire period considering very cyclic subsidence behaviour of in situ data?
AUTHORS: Streletskiy et al. (2016) reported the trend in their in situ data. It is justified to compare the trends from ours and theirs.

p. 12 line 11: “. . .show strong heave relative to the previous August. . .”
AUTHORS: We changed to “relative to the previous August” as suggested (Page 12, Line 21).

p. 12 line 13: “We cannot explain them by strong ground uplift during winters as none of these three winters were particularly cold (their freeze indices were at the mean level, Figure 4b) or during cool summers (the thaw indices of 2009 and 2011 were higher than the mean level, Figure 4b)” As mentioned before, cold winters cannot explain the magnitude of the heave. Better to look at the amount of the available moisture in the preceding summer. Why heave should happen in the cool summer is even less clear. Unless you mean that during a cool summer subsidence is small and therefore uplift during the next winter can be more pronounced. But this is not the case as far as I can see from the Figure. Also check please the figures numbering. In general, some of the discussions here could be moved to the Discussion.

AUTHORS: We completely rewrote these sentences because we now compare the subsidence with the precipitation in the previous August (new Figure 6, and our response above). We observe that for seasonal subsidence larger than 5 cm, they increase nearly linearly with the precipitation, which confirms our hypothesis; yet for small subsidence (around 2 cm), they are independent of the precipitation (Page 15, Lines 13 to Page 16, Line 5).

We fixed the figure referencing problems.

p.12 line 20-21: Again, I would add the word "generally", because not all years showed a good fit.
AUTHORS: We added "generally" as suggested (Page 14, Line 10).

p. 12 line 21-22: Again, I don't see a rule – not always small range and bad fit coincide. Is it justified to use the fit results for all the years including very poor fits? And especially to extrapolate June values with poor fit?
AUTHORS: Poor fit results in larger uncertainties of the best-fit subsidence and the extrapolation (Table 3, Page 17). Moreover, we did not use $d_{\mathrm{seg}}^{\max}$ in years with R^2 smaller than 0.5 in our analysis or interpretation.

p.12 line 24 and further on: I don't think it is proper to refer to the table columns in the main text. It can be described in a neater way. I think the color of the line in figure 5 is magenta, not cyan.
AUTHORS: We completely rewrote this section (Page 14, Line 9 to Page 15, Line 19).
We changed cyan to magenta (Page 14, Lines 6 and 7; Page 15, Line 1).

p.12 line 32: Please briefly explain the method with reference to Liu et al. 2012 for more details.
AUTHORS: In the methodology section 4.4, we added the following description (Page 9, Lines 26-28): "We also estimate the uncertainties of $d_{\mathrm{pore}}^{\max}$ by propagating the standard deviation of ALT measured within the footprint (i.e., 6 cm) and the uncertainties in the assumed model parameters for calculating water content (see equation 16 of Liu et al., 2012)".

p.13 line 9: As mentioned before several times the cold winter could not lead to more segregated ice. I think this part of the Results including Figure 6 is not plausible. Instead you could try to compare the soil moisture or precipitation in the previous summer to the subsidence in the next summer.
AUTHORS: As suggested, we compare the subsidence with the precipitation in the previous August (see our response above)

p.14 Table 2: Please add the R^2 of the fit for each year. In the bottom of each column I would add mean and standard deviation. Please also add ALT measurements in the table.
AUTHORS: As suggested, we added the R^2 values as well as the mean and standard deviation to Table 3 (Page 17). We chose not to add ALT to this table as it is all about subsidence. Instead, we added a new plot to show the ALT time series (Figure 4c, Page 13).

p.15 line 7: What is meant by excess seasonal subsidence?
AUTHORS: We have completely rewritten this sentence and no longer use "excess seasonal subsidence".

p. 15 line 9: Is the thaw index increase gradually?
p.15 line 10: Can you check if the trend is linear and what is the linear fit then?
AUTHORS: The linear increasing trend was 20.3 °C days/year. We rewrote the sentence to "The thaw indices also increased from 2005 to 2013 with a trend of 20.3 (°C days)/year …" (Page 20, Lines 8-9).

p.15 line 11-13: as before, does not sound reasonable. Please add here some discussion on the modelled pore ice subsidence VS segregated ice subsidence. Because we observe the difference between the subsidence due to pore ice melt (assuming the modelling is correct given unknown ALT and porosity) and the overall subsidence, it is reasonable to suggest the thawing of the transitional layer.
AUTHORS: As we now compare subsidence with August precipitation, we completely rewrote this sentence as "In years when excess water remains in the active layer before freezing, significant accretions of segregated ice can develop within the transition layer and cause surface heave during winter (Figure 6)" (Page 20, Lines 10-11).

p.15 line 15-16: need a reference.
AUTHORS: We added two references: Hinkel and Nelson (2003); Shur et al. (2015) (Page 20, Line 14).

p.15 line 18: Please emphasize the generally high match between your results and results of Streletsky et al. 2016. I think it is very important and positive. How these results correspond to the results of Liu et al., 2010? Are there other relevant studies?
AUTHORS: As suggested, we highlighted the match as "The two independent estimates of linear trends at Barrow agree very well, i.e., 0.26 ± 0.02 cm/year from this work and 0.19 ± 0.14 cm/year from Shiklomanov et al. (2013)'s GPS campaigns" (Page 20, Lines 18-19).

We also added a sentence to describe the results of Liu et al. 2010: "The InSAR measurements of Liu et al. (2010) revealed linear subsidence trends of 0.1 to 0.4 cm/year between 1992 and 2002 over Prudhoe Bay, consistent with the two Barrow studies within the same order of magnitude" (Page 20, Lines 20-21).

To the best of our knowledge, there are no other relevant studies on the North Slope of Alaska.

p.16 Chapter 5.2: as mentioned before I would move it to the Methods. p.16 line 11: section 3.2 instead of 2.2?
AUTHORS: We agree and moved relevant sentences to the new method section 4.6 "Simulating soil moisture effects on the retrieved reflector height" (Page 10, Line 21 to Page 11, Table 2).

p.17: Soil moisture data description should be added to the Data section (which should be created).
AUTHORS: We agree and moved soil moisture data description to the new data section 4.1.3 "Soil and meteorological data" (Page 6, Lines 26-28).

p.17 line 12: Did you check rain events with precipitation data? Is it available?
AUTHORS: Yes, we checked the precipitation records measured at the Barrow Airport. Figure R3 on the next page shows precipitation events (gray peaks) caused sharp increases in soil moisture (black dots) in summer 2010. Because these facts are non-essential, we do not include this figure in the manuscript.

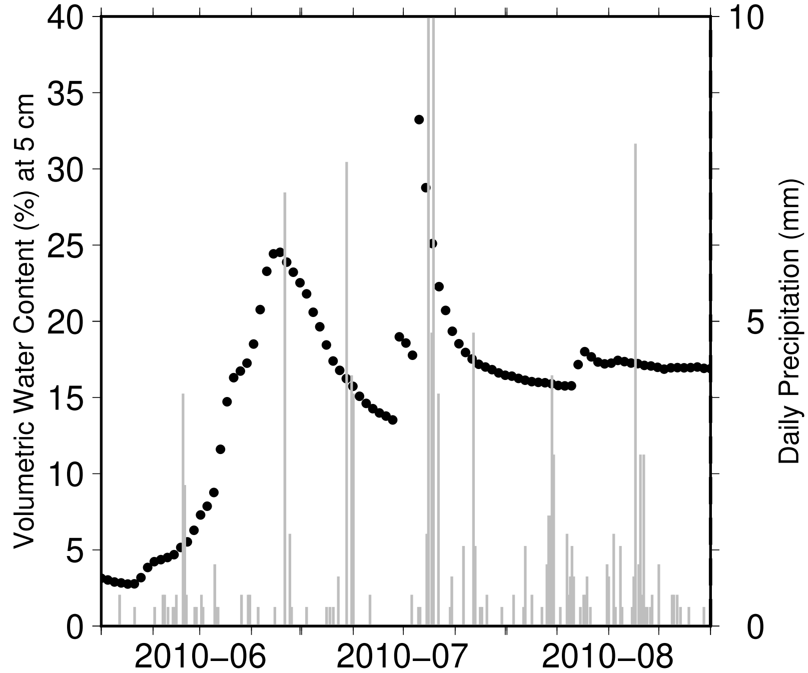

Figure R3: Time series of volumetric water content at 5 cm depth near SG27 (black dots) and daily precipitation measured at the Barrow airport (gray bars) in summer 2010.

p.17 Figure 8b: should the y-axis label be "compositional height changes"? What is the purpose of scale direction from top to bottom?

AUTHORS: As suggested, we changed the y-axis label of Figure 8b to 'Compositional Height Changes' (Page 19). Because an increase in compositional height can be potentially mistakenly interpreted as an apparent ground surface subsidence, the vertical axis of this figure is flipped to facilitate comparison with subsidence plot such as Figure 4a (Page 19, Lines 4-6).

p.17 line 10-11: I did not understand this. Consider reformulating.

AUTHORS: We rewrote these sentences as: "Between July and August in each year, the change range of VWC was up to 15%. The only exception was 2009 when the range was the largest, ~25%" (Page 18, Lines 11-12).

p.17 line 15-17: I see some matching between decreasing soil moisture and decreasing compositional height change between 2002 and 2007. I also see larger height change for the years 2009 and 2010 when there were rain events. Although I agree that all the changes are smaller than the observed subsidence, I think you should discuss this.

AUTHORS: Our simulations (Figure 7) illustrate that the compositional height increases monotonically (not linearly though) with soil moisture (Page 18, Lines 3-4).

p.19 line 6-7: "In situ ALT or GPS measurements have been conducted annually, but not always on the same day of the year due to logistical constraints." Do you mean "can be conducted" / "typically conducted"?

AUTHORS: We mean "typically conducted". We rewrote the sentence as "In situ ALT or GPS campaign measurements were typically conducted annually, but not always on the same day of the year due to logistical constraints" (Page 20, Lines 27-28).

p.19 line 7-8: “Because the seasonal changes are more significant than the inter-annual and long-term changes. . .” Why so? Please reformulate.
AUTHORS: We rephrased the relevant sentences as “Our GPS-IR results show that the seasonal changes are more significant than the inter-annual and long-term changes. It is possible that the inter-annual and long-term changes estimated from a poorly-sampled record of elevation changes (e.g. annual measurements) may be aliased by the seasonal changes (Liu et al., 2015). Our daily-sampled and long-lasting records from GPS-IR can avoid such aliasing problem and give robust estimates on the inter-annual and long-term variations” (Page 20, Lines 28-32).

p.19 line 11-12: “Since the GPS-IR-estimated reflector height directly reflects the frozen ground dynamics, it is convenient for permafrost scientists who do not need to process geodetic-level GPS positioning data or correcting for the solid earth movement.” It sounds a little bit offensive towards permafrost scientists, please reformulate. You can just say something like the data processing is relatively easy and does not require special skills or training.
AUTHORS: We revised the sentence to “Since the GPS-IR-estimated reflector height directly reflects the frozen ground dynamics, it is unnecessary to process geodetic-level GPS positioning data or correcting for the solid earth movement” (Page 21, Lines 1-2).

p.20 line 7-8: Is it possible to provide some numbers such as how many of these stations are available circum-Arctic?
AUTHORS: We added that “more than 200 GNSS stations are located in permafrost regions in the Northern Hemisphere” (Page 21, Line 31).

p.20 line 8-9: “Our study also highlights the importance of long-lasting measurements of active layer thickness, soil moisture, ground temperature, and surface elevation changes, ideally at the same location. . .”. You are not using ground temperature data and do not discuss them previously in the manuscript. Also, from your study one can draw a conclusion that ALT can be roughly estimated based on the measurements at the different side, meaning that in principle there is no need in the continuous measurements of ALT at the same location. Although this can be debated.
AUTHORS: We rewrote this sentence as “Our study also highlights the importance of long-lasting surface elevation changes and in situ soil measurements (such as active layer thickness and soil moisture), ideally at the same location, for a comprehensive and quantitative understanding of near-surface dynamics of the active layer and permafrost” (Page 21, Lines 31-33).