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Interactive comment on "Decadal changes of surface elevation over permafrost area estimated using reflected GPS signals" *by* Lin Liu and Kristine M. Larson

Anonymous Referee #2

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This paper analysed a dense and multiyear time series of reflected GPS signal to measure the elevation changes over a permafrost region in Alaska due to melt of ground ice. The application of method is novel and this study certainly proves the method's feasibility, thus, potentially opening new perspectives in permafrost dynamics investigations. One of the positive aspects of this study is a direct comparison of the results with measurements from another study over the same regions which used a different method for the quantification of the thaw subsidence.

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

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Discussion paper

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.

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?

"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.

"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.

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?

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? 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.

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.

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.

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.

I also recommend the language proofreading to smooth the style and improve the clarity.

Specific comments:

p.2 line 3 and further on: Please check if it should be DGPS instead of GPS?

p.2 line 6-7: I think providing numbers here just for two years is too specific.

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.

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.

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.

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.

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?

p.4 line 14: reference.

p.4 line 17: reference.

p.4 line 18: When exactly the ALT measurements were made?

p.5 line 5: What is the resolution of the relief map?

p.5 line 8: When the main photograph (with snow) was made?

p.6 line 3: 53 cm thick in 2016.

p.6 line 13: might need more detailed explanation or at least reference.

p.7 line 14: reformulate please the first sentence. What means “issue” in this case?

p.7 line 27: why “reiterating”? I don’t think it was mentioned before.

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?

p.8 formula 2: should the H be actually the change of H?

p.8 line 22: what means forward manner?

p.8 line 26: what about the outflow of the water? Worth to mention.

p.9 line 5: what are the massive cryogenic structures within the active layer? I think terms are confused again.

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.

p.10 line 12: how the unreliable estimates were defined?

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.

p.11 Figure 4: Is it right to use standard deviation having 4 observations? I think the range would be a better characteristic.

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.

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.

p. 12 line 3: Figure 4a, not 3a

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?

p. 12 line 11: "...show strong heave relative to the previous August..."

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.

p.12 line 20-21: Again, I would add the word "generally", because not all years showed a good fit.

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?

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.

p.12 line 32: Please briefly explain the method with reference to Liu et al. 2012 for more details.

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.

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.

p.15 line 7: What is meant by 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?

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.

p.15 line 15-16: need a reference.

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?

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?

p.17: Soil moisture data description should be added to the Data section (which should be created).

p.17 line 12: Did you check rain events with precipitation data? Is it available?

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

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

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.

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”?

p.19 line 7-8: “Because the seasonal changes are more significant than the inter-annual and long-term changes. . .” Why so? Please reformulate.

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.

p.20 line 7-8: Is it possible to provide some numbers such as how many of these stations are available circum-Arctic?

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.

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