Dear authors and reviewers of "Comparing thaw probing, electrical resistivity tomography, and airborne lidar to quantify lateral and vertical thaw in rapidly degrading boreal permafrost".

The Editor's comments after the review are in green text.

I have gone through the comments and responses, and I would like to ask Reviewer Nr 2 to respond on the following three points, to see if they are satisfied. These points are as follows:

1) As a major point, Reviewer 2 asked for dates of the frost probing and even the field work. Knowing just the year is not sufficient.

I see that Reviewer 2 writes: "P4 L100, there is no mention of the exact timing or dates of the site visits that were completed each year from 2012 to 2020. This information is very important, as biases can be introduced if site visits occurred later in the thaw season as the study period progressed. Please provide a table of the dates of the site visits and additional contextual information, such as the sum of thawing degree days leading up to the site visit date for each year. This can be included as supplementary information if needed."

The authors' reply was : "As was stated earlier we have added the following text: "Repeated thaw probing using a metal rod in late summer (September) quantified changes in the top of near-surface permafrost. Maximum probing depths varied from 2.5 to 4.0 m depending on the number of extensions used, occurrence of gravel, and stickiness of unfrozen silts."

I agree that the dates are very important pieces of information. To the authors: Can a table of field work dates not be provided as a Supplemental table? Could the reviewer please comment on whether reference to simply "late September" is sufficient (note that above states only "September"), if the authors cannot provide the precise dates.

2) Could the reviewer please look at the new table about the class definitions and comment on whether this is acceptable? In addition, could the authors consider whether the information is complete and consistent here, eg, Shouldn't you include in S1 Table 1, the thaw depths that define your classes?

3) Is reviewer 2 satisfied with this reply from the authors?

P17 L352-354, many years of personal experience with frost probing in various materials would lead me to disagree with these statements. I would say that frost probing does not provide precise measurements, as the depth to the frost table may vary according to the person performing the probing, to the material, and to the timing of the measurement from year to year. I also do not think that it is possible to detect precise changes in soil texture with depth, and if this is the method that is used to determine stratigraphy, then soil pits need to be dug to verify these interpretations. The discussions of the limitations of probing on P18 L370-373 also contradict this description of probing as a "precise" method that can "detect marked changes in soil texture".

This is a great point and we have addressed it by clarifying WHERE the frost probe measurements are most useful and HOW they have been confirmed with soil pits and cores: “Thaw probing, accompanied by surveying of ground and water-surface elevations, yielded the most precise measurements of subsurface changes in the permafrost table than ERT or repeat LiDAR and this helped identify degradation stages (Figs. 2 and 3). Probing can detect changes in soil texture (peat, silt, sand, gravel) with depth, however, soil stratigraphy was confirmed with soil pits and boreholes (Brown et al., 2015).”

If the Reviewers are unsatisfied with any other changes, please let the editor know.

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Now there are some Editor's comments (which are in green color) regarding the authors’ replies.

1) *In your changes to the manuscript based on Reviewer comments, there may be some language to correct (see my suggestion below in red). In fact, in a few of the additions or changes to text in the Reply to Reviewers. Please do some grammatical checking before submitting the next version.*

“A total of 183 Ground Control Points (GCPs) were measured using real time kinematic and post processed kinematic techniques to validate the accuracy of the LiDAR DEM products. The average error was 0.093 m in the DEM (95th percentile) across different ecotypes (Zhang et al., 2023), leading to the propagated error of 0.13 m for the differenced LiDAR DEM. This error was considered in our thaw stage analysis.”

2) In your reply to reviewer 2, I did not see an answer to this. Is this already addressed?

Given that the first ERT survey and the first LiDAR acquisition were conducted in different years, it would be very helpful if the authors could provide a description of the climatic context for the 2012 versus 2014 years. The authors should also include a limitations section in the Discussion that addresses overall limitations to their study, including issues in comparing between methods in different years.

3) The authors have added “Based on decadal mean annual temperatures at the Fairbanks International Airport the area warmed ~2.3 °C between the 1930s-1940s and 2010-2020. Over that same timeframe mean summer temperatures (May 1 to October 10) warmed ~1.7 °C while mean winter temperatures (October 11-April 30) warmed 2-4 °C (Douglas et al., 2024). ”

This is an addition of text, so I will comment on this. All of the temperatures are given as a mean value change, but for winter temperatures a range is given. Why? Please clarify what the range refers to.

4) Grammatical issue here*:* “We estimate the accuracy to be mostly with 10 cm in open areas and 30 cm in forested areas.”

5) Please give the full reference, if this is a book: Blaschke, T., S. Lang, E. Lorup, J. Strobl, and P. Zeil. 2000. Object-Oriented Image Processing in an Integrated GIS/Remote Sensing Environment and Perspectives for Environmental Applications. Environmental Information for Planning, Politics and the Public 2: 555–570.

6) In response to the reviewer comments on the repetition on P7, L173-187, the authors add

"We differentiated trends across each transect into seven degradation/aggradation stages (Supplemental Information Table 1). "

I think you have six stages, and not seven now?

7) I agree with reviewer 2 here. You cannot know what happened between 2012 and 2014 if you do not back it up with data. Either point to the data that supports the statement or remove it.
Review 2 writes: P15 L299-300, this statement that the resistivity values decreased between 2012 and 2014 should be removed if the authors do not present the tomograms from 2014 in this paper.

8) Some grammar to fix here. P17 L333, "These are the most common landforms above boreal discontinuous permafrost" – what does this mean? That the permafrost plateaus mentioned in the previous sentence are the most common landforms? Please clarify.

My suggestion (feel free to modify) for a grammatical change is: "Plateaus are the most common landforms occurring in boreal discontinuous permafrost, and are a signature indicator of the presence of permafrost in the region."

9) This addition could be written better for flow: "This is likely due to the lower ice content of lenticular ice from 1.2 to 2.8 m depth at TF88 compared to braided (reticulate-platy) ice from 1.5 to 2 m at T1 (Brown et al., 2015)."

10) I cannot find a real answer to the reviewer's question in the following:

Reviewer 2 writes: CONCLUSION P22 L494, what data was provided from 2004? The methods section describes the survey transects as being established in 2011/2012, not 2004? Please revise and correct.

You need to revise the sentence in the conclusion, and it seems you replace this:

"Between 2004 and 2020 top-down thaw of near surface permafrost doubled from 18% to 36%."
With this:
"For example, at site T1, our control transect, top-down thaw of near surface permafrost occurred over 44% of our study sites between 2004 and 2020."

Which does not change the problem as I see it. What the authors say they have added to the methods for clarity (below) has not added any information about 2004, as the reviewer asked.

We have updated the Methods to clarify when each transect was initially surveyed or modified (expanded): "The Tanana Flats lowland has experienced numerous wildfires and we established transects to represent high severity fires in the summers of 1988 (TF88, 64.734 °N, 147.826 °W), 2001 (TF01, 64.644 °N, 148.295 °W), and 2010 (TF10, 64.716 °N, 148.010 °W). Transects were initially studied to assess the effects of fire on permafrost (Nossov et al., 2013): TF88 (200 m; first surveyed in 2012) is in an area burned in ~1950 that reburned in 1988; TF01 (established as a 320 m transect in 2012) is in an area burned in 2001, and TF10 (initially 100 m in 2011, extended to 200 m in 2012) is in an area burned in 2010. Transects were positioned to cross a range of permafrost and non-permafrost ecotypes and TF01 and TF10 were extended to more adequately represent different cover types. We also included an unburned site (T1, 64.722 °N, 147.959 °W) that has not burned in recent years (~1950s-present) for comparison. Supplemental Information Figs. 3 and 4 provide photographs of the field sites. T1 (initially 100 m in 1995, extended to 200 m in 2012) was established during ecological land surveys (Jorgenson et al. 1999). In Douglas et al. (2016) T1 is referred to as "1930". Two of the sites (T1 and TF88) were relatively ice-rich with thick peat and silts extending down 34 m whereas the other two study sites (TF01 and TF10) had sand and gravel at relatively shallow depths and lower ice contents."

11) Editor comment:

Line 146: Airborne lidar surveys were conducted by Quantum Spatial Incorporated (Anchorage, Alaska) in May 2014 and 2020 in a period of 95% snowmelt with leaf-off conditions using a Lecia ALS laser system**, leading to an average pulse density larger than 25 points/m2** over the targeted area (~40 km2, Figure 1) where our field transects were located.

A lot of numbers are larger than 25 … could you be more specific?

12) Editor comment: Do you need to change Figure 4 to show six classes rather than 8 now?