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Interactive comment on "Terrestrial laser scanning for quantifying small-scale vertical movements of the ground surface in Artic permafrost regions" *by* Sabrina Marx et al.

Anonymous Referee #2

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The manuscript makes an attempt to illustrate and evaluate a methodology for permafrost surface morphological change assessment using repeat TLS surface mapping. This is a worthwhile and long-awaited theme of research and is relevant for readers of Earth Surface Dynamics. As such, publication is warranted as this will undoubtedly be an area of highly active research moving forwards. However, I believe this paper, in current form, is not worthy of publication as I believe it does not meet basic criteria either for a proven innovation in methodology or new information about rates of change over a permafrost landscape. Indeed, upon starting to read the ms, the reader has high expectations for a demonstrated effective new methodology or about spatial variation in permafrost land surface change patterns but those expectations were not met. TLS

surface change using the methods advocated is not in-itself new and - more importantly - the conclusions present recommendations which experts in the field of TLS change detection might consider trivial and a priori self-evident. Regarding the highly anticipated permafrost dynamic component, we are told merely that the observed changes are "plausible" but the changes are weakly validated, despite the descriptions of control point arrays and field measurements. Given the 'observed' changes are not related with confidence to driving mechanisms and that they are close to noise levels in the analysis, we don't learn much that is new and it does not follow that a "plausible" rate of change is proof that change has been observed. Of note, one area of uncertainty that is not adequately addressed is that of changing moisture condition influences in apparent seasonal or inter-annual surface heave or subsidence.

I have not rejected this paper, however, as I believe documentation of the experiment is worthwhile given so few TLS data collections have been performed with a view to quantifying change in these landscapes. The application obviously has merit and should be further refined / evaluated. However, I would prefer to see a more thorough analysis with the current conclusions downplayed to match the evidence presented by the data. i.e. avoid implying that "plausible" rates of change are evidence that change has been observed. This is not proof. Also, I was a little confused as to why there are so many figures in appendices. In some cases, the cited figures would be logically embedded, while in others it is clear the figures are not needed. I find this aspect of presentation cluttered and confused. Finally, while referencing is adequate, I find there are instances I am aware of where the paper does not cite original studies but rather focusses on recent studies that have recreated the innovations of others. Alas, this seems to be a trend in published material these days so is not a 'rejectable' offence but it does suggest the authors could have done more homework on their topic of study.

My specific comments are not exhaustive, as I think the manuscript needs to be reframed as a technical evaluation as opposed to demonstration, and the recommendations need to go beyond the obvious and a priori self-evident into actionable criteria on sampling and analytical design (I make some suggestions below). I also believe the criteria for success in the experiment need to be defined. If this is done, however, I believe the experiment will fail to achieve its goal. That, however, does not preclude publication rather it does provide an opportunity for the authors to make recommendations over what is needed to achieve success? For example, it may be that collection or processing improvements could be made or that there are hard limits on the magnitude of change that can be observed with confidence under certain circumstances. Detailing these technical and circumstantial constraints would be informative for future studies. Stating results are plausible does little to advance the science or our understanding.

Specific comments:

P1 L19-21 - here and elsewhere I find that the text is suggesting two mutually exclusive concepts (i.e. multiple scan positions vs raster DEM differences) when in fact one concept could be a subset of the other. This needs to be tightened up throughout, as the implication is that these are distinct methods (which I assume they are in the way executed here) but in fact raster approaches could be applied either to single or multiple scans so the distinction the author is aiming for is not obvious.

P2 L11-17 – In this section, it is implied (though not stated) that ALS assessment of permafrost-related processes and associated surface and vegetation dynamics has not yet been evaluated / published. However, I'd like to draw the authors' attention to a recent paper by Chasmer et al (2017) in Global Change Biology ("Threshold loss. . ..."). Here permafrost loss and associated vegetation dynamics were explicitly evaluated and rates of change quantified using time series ALS.

P9 L29-31 – Two points. i) I'm not sure the evidence presented can be taken as proof that any spatial change has been observed with confidence at all. I think this is a general conclusion that requires further proof; ii) it is a priori self-evident that multiple samples or measurements of the same object will tend to increase confidence in the observation being made. As such, I'm not sure why much is being made of this

'finding'. Many TLS papers going back over a decade that deal with vegetation and other surface attribute sampling and modeling have either implicitly made this assumption without needing to prove it or have directly observed that multiple scan sampling improves results vs single scan sampling. On the one hand, I think this result is not worthy of recording because it is trivial but on the other, if the authors' feel it is worth commenting upon, then I suggest placing this observation into the context of existing TLS plot sampling literature.

P10 L5 – "This can indicate. . ..." I'm not sure what to make of this statement. Does it mean that surface deformation is influenced by vegetation attributes? If so, this would be a worthwhile topic to further explore and characterise. Stating that something might be happening is not instructive but if it is (or even might be) happening then providing further evidence and discussion would be worthwhile given the stated topic of the paper and journal.

P10 L7 – i) minor point but is a negative subsidence heave? Here we see a subsidence rate of -0.4cm. I think this needs to be clarified to avoid confusion. ii) more importantly, however, could not 4mm of change simply be due to differences in moisture content in the sphagnum between data collections? I am not familiar with the depth or volume of moss overlying the TVC study site but in areas to the south I do know peat moss surface scan expand and contract vertically by up to 5cm in a single season. As such, given we are here seeing changes that are generally less than 2cm and even discussing differences at mm-level, how much of this change could be due to moisture differences in the surface organics? This may not have been studied directly but it at least must be acknowledged as a source of uncertainty or discounted using an evidence-based argument.

P11 L11 – it is ok to confirm that TLS subsidence monitoring will be influenced by vegetation. However, this is an odd 'finding' as – again – it is a priori expected to be true without the need for verification. What would be more instructive, in this regard, would be to quantify to what degree certain types or heights or densities of vegetation influence rates of observed subsidence. The text and Figure 9 hint that this might be possible but the analysis does not get far enough into such stratification. As such, the stated finding is trivial and does not enhance our understanding.

P11 L17 – as already mentioned, noting that the observed surface differences match plausible rates of subsidence is not an informative result; especially when those rates are close to noise-level in the analysis anyway. . ..and when other influences like changes in surface moisture and sphagnum expansion / contraction have not been considered.

P11/12 L27 + Recommendations: (i) I don't see co-registration of scans as an appropriate recommendation of this study. Indeed, it is a priori required to co-register scans for this type of analysis. The specific aspect of requiring permanent fixed reference points is appropriate, though also to be expected given the existing literature on change detection over dynamic surfaces. If the recommendation were to provide specifics on design elements for TLS change assessment over an actively subsiding permafrost surface that are based on findings of this study, then that would be instructive given the authors' somewhat unique experience in this specific experimental context. As such, more in-depth analysis of the rigidity of the ctrl marker array through time - and where outliers may have occurred - would be informative. (ii) It seems obvious that spatial sampling, surface undulation and vegetation cover will influence the accuracy of deformation analysis as a result of specific location-based occlusions. This should be expected, so to state this in the conclusion as part of a recommendation for spatial averaging seems redundant. A more quantified statement concerning under exactly which conditions spatial averaging is needed and to what degree would be more actionable for future experimental design. (iii) Not clear why it is worth recommending multiple scan positions. This is a priori known from previous TLS plot-sampling studies and it follows logically / intuitively that increased independent sampling will increase accuracy. However, it is instructive, for example to document errors associated with a single scan vs 2 scans vs multiple scans and from different geometric sampling

locations. Following such analysis, a specific recommendation regarding number of scans and the geometric configuration necessary to meet certain accuracy requirements could be made and would be very useful. (iv) Not immediately clear how this is a recommendation or how it functionally differs from the previous recommendation.

P12 L10 – I don't agree that the results – as presented – demonstrate that TLS provides highly accurate ground truth data for subsidence monitoring over the time or space scales evaluated at this particular study site. I do agree it has the potential to do so, however, but the statement needs to be adjusted so it is based on the actual provable findings of this study.

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