AUTHOR RESPONSES TO REVIEWER 2 COMMENTARY ON TUTTON AND WAY MANUSCRIPT

Snow depth is a poorly observed variable: measurement networks are sparse (especially across the subarctic and Arctic) the measurements are prone to uncertainty, and often fail to capture the prevailing landscape-scale variability. Limited tools are available to address these weaknesses, so advancements in low-cost, easy to deploy instrumentation is of high interest to the snow community. This manuscript provides an overview of a new low-cost approach to acquiring snow depth estimates using vertical profiles of light and temperature measurements. The paper is clearly written and provides a careful inter-comparison of different processing techniques to derive snow depth from the profile measurements. While I have only a small number of suggestions on the manuscript, my main concern, as outlined in my first comment below, is with the experiment design.

1. Because no independent time series of snow depth measurements were acquired coincident to the profile measurements, it is not possible to know what the 'true' snow depth was. This means the various approaches to estimating snow depth can be compared, but not assessed. Only in the case of Goose Bay can the SCLT-derived snow depth time series be shown alongside an independent measurement, and as rightly pointed out in the text, this comparison serves to highlight the differences between snow measurements made in open environments (airports) versus in the forest. Ideally, an SR-50 or another independent sensor would have operated alongside the SCLT profile, although I understand the cost of such a deployment could be prohibitive. To mitigate the lack of assessment and highlight the inter-comparison, it would be useful to see the multiple snow depth time series produced at each location from the various techniques on a single plot (individual panel for each site). This would illustrate the range/agreement in snow depth through time based on the analysis methodology, which can now only be inferred by flipping between figures and looking at the correlation results in Figure S3. I suggest adding this new figure, and a brief discussion of it, to Section 5.

[T&W response] We agree and acknowledge that the lack of validation poses some challenges in evaluating the method. We have added an additional figure which compares the snow depth results for each of our SCLT sites to the CMC snow dataset (Brown et al., 2003; Brown and Brasnett, 2010) produced by Ross Brown (Section 5.1). Although these data are based on a combination of first-guess modelling and regional weather stations this product is widely-used and does represent variability in snow characteristics for an overlapping period with our stations. The new comparison we have added (Fig. 12) shows relatively strong agreement between snow estimates for our sites and the regional CMC product. The comparison also demonstrates a clear difference at BaseSnow but we believe our representation is as likely to be accurate as the CMC product which lacks any observed snow depth inputs in this region.

2. Line 262-268: consider moving Figure S3 out of the supplemental material to include it in this paragraph. I think these are worthwhile results to include in the manuscript.

[T&W response] We agree with this comment and have moved Figure S3 to the manuscript.

3. This issue is acknowledged in the Discussion, but why not use consistent vertical spacing of 10 cm? In a relative sense, greater uncertainty with deeper snow is ok, but the current setup dictates that uncertainty will be greater when snow is deeper.

[T&W response] There are two reasons for this. The first is that we had deployed ibuttons for many years at these sites using a similar vertical arrangement so for consistency sake we did not want to introduce an even greater inconsistency. The second reason is more practical in that we are often more concerned about the shallower components of the snowpack when considering thermal impacts of snow cover of permafrost in the region therefore this configuration saves costs and serves the original purpose of the stakes. We have amended the text to clarify this point.

4. Line 122: can you provide a simple description of the PELT method? Not clear what is meant by 'asymptomatic penalty of 10%'.

[T&W response] This penalty coefficient is part of the PELT method cost minimization function (Killick et al, 2011) and optimizes the number of changepoints in a segmentation. An asymptotic penalty is testing for significance, therefore β=0.1 (10%) is indicative of 90% confidence. We agree with the recommendation to provide a simple description of the PELT method in the manuscript with reference to the original derivation of the cost function.

5. Line 170: "all SCLT sites except for BaseSnow had a snowpack taller than the 170 uppermost data logger". Murphy's Law at work that there was an unusually deep snowpack during the season that you were evaluating this approach! Do you have a sense of how tall the profile needs to be? Is there any technical limitation to say, a 2 m profile with sensors every 10 cm?

[T&W response] This year was a high snow year relative to the 66-year average (Fig. S5). Prior work (e.g. Way and Lewkowicz, 2018) typically did not have snow depths exceeding 160 cm at these sites but as you say, this is an unusual year. There is no reason to limit the stake height to a particular limit other than we had cost, logistical and continuity considerations in the field that led us to choose the heights we did. We have amended the text in one location to reflect this point.

6. Cost effectiveness is a major driver of this work, but (unless I missed it) nowhere in the paper is the cost of the SCLT profiles stated. This information would be helpful! What is the cost sensitivity to the vertical resolution of the profile?

[T&W response] Cost of SCLT profiles is presented to Table S1 in the supplemental materials and compared to common iButton stakes. The cost is linear relative to the number of loggers installed, where total cost = installation cost + cost per logger * number of loggers. We have amended the text to make more clear reference to this.

7. Sections 4.5 and 4.6 provide more detailed analysis of the light intensity methods. To improve the logical structure of the paper, I suggest shifting these up to follow Section 4.3, and shift down the temperature measurement approach reported in Section 4.4.

[T&W response] We agree with moving Section 4.5 up to follow Section 4.3; however, we have kept Section 4.6 in place as it follows the presentation of the temperature method.

8. The reference list needs to be cleaned up. Some citations are missing (e.g. Archer, 1998) and details are missing from some references (journal titles, etc.). Review the sequence of figure numbers: figures jump from 10 to 13 to 15.

[T&W response] We agree and have addressed the errors in the reference list. Figure order was revised after editor comments and with the reviewer's comments above. It should be noted that referencing issues were introduced by the .csl file for The Cryosphere in the public repository therefore it may warrant further investigation.

9. While the code and data availability are provided, what about schematics to the design of the probe? Will these be shared in some form so that others can follow your design if interested?

[T&W response] We have provided a diagram of the stake set up in Figure 2 and Figure 3. If readers need more instruction on installation we can provide a detailed technical drawing upon request.

Editorial
Line 30: change to 'snowfall is hard to catch, melts differentially once on the ground..."
[T&W response] We agree and have made this change.

Line 55: not clear what is meant by 'relatively unambitious method'. . .uncomplicated?
[T&W response] We have changed relatively unambitious method to straight-forward for clarity.

Line 60: suggest changing to 'broader snow science community'
[T&W response] We agree and have made this change.

Line 118: change to "We determined SCLT-derived snow surface heights using. . ."
[T&W response] We agree and have made this change.

Figure 4: minor point, but the y-axis range for BaseSnow is slightly different from the other sites
[T&W response] We agree and have changed the axis to be consistent.

SUMMARY OF CHANGES:
[1] Included further validation to CMC snow depth analysis dataset and comparison between methods in manuscript.
[2] Added brief background for using stake height and logger distribution.
[3] Included additional description of the statistical methods used in the changepoint analysis.
[4] Included further reference to the cost differences described in Table S1.
[5] Made revisions to missing references and errors in citations.
[6] Made revisions to language to improve clarity of manuscript.