

Reply to Nicholas Kinar comments on "Comparison of manual snow water equivalent measurements: questioning the reference for the true SWE value" by Maxime Beaudoin-Galaise and Sylvain Jutras, The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-354-CC3>, 03 Feb 2021

Comments in blue

Answer in black

1. This paper is an up-to-date and intelligent commentary on differences between manual SWE samplers in a regional context. Many papers on comparisons between manual SWE samples and pit methods have been published in journals associated with snow conferences and most of the comparisons have been made at high mountain locations. In this paper, the authors provide an analysis of comparisons that support other studies, and the data is collected at the Foret Montmorency site, an experimental forest in Eastern Canada. Most papers written in science are not completely novel since scientific research is based on the work of other researchers and there is always a need for validation and verification studies. This paper therefore helps to provide ancillary data to support other studies and provides an important test of different manual samplers at a forest site that is not situated in the mountains. The paper is important in a regional context.

I strongly recommend eventual publication after the authors have addressed review comments and added additional information. The paper compares novel snow samplers such as the Hydro-Québec sampler and the Université Laval sampler and is therefore important for characterizing these new devices, particularly at a forested continental site in North America. The title of the paper can be modified to better communicate this idea.

Thank you for your interest and comments on the submitted manuscript. You rightly underline an important point about the importance of our study in a regional context. The snow samplers used in our study were designed principally for snow conditions typical of the boreal forest of eastern Canada, i.e. generally snow depth < 150 cm and with presence of several ice layers through the snowpack. As mentioned by anonymous referees #1 and #2, we have added in a revised version of the manuscript more precision on the description of the typical snow cover conditions of our study site, as well as clarifications in the discussion and the conclusion on results that have a more regional scope. The title has also been changed to clarify the regional context of the study. Your comment highlights the importance of these additions.

There can be some additional information added to the paper:

2. Wavelet or fractal scaling mathematics can be added to the paper to quantify differences between devices. This would nicely complement the statistics presented by the authors.

We find that this suggestion is relevant, but not necessary to achieve the objectives of our manuscript. We wish to restrict the results and discussion to the statistical comparison of different manual methods for SWE estimation. We believe that it is sufficient to base the interpretation of the results on field observations and the scientific literature to explain the differences between the SWE measurement methods studied.

3. Snow pit sampling is always a subjective process since it is performed by human beings. The authors should indicate in a revision how this subjectivity influenced the study and how the height of snow measurements have uncertainty. Some additional information can also be provided on the regional characteristics of the snowpack. The paper therefore provides a test quantifying this subjectivity.

The snow pits were mainly done by a trained observer (66%) and the first author (22%). When the snow pits were made by another person, it was qualified people from our laboratory who had already accompanied the main observers in the field (for help or training). In response to your comment, a statistical comparison (i.e. ANOVA) was performed to check if there was an observer influence in the measurement error (MBE) for the snow pit. By comparing individually the MBE results of the three methods of snow pits and two snow sampler (SFS and HQS) according to the two main observers, there is no significant difference (p -value between 0.256 and 0.716). For the four other observers who made snow pits, it was not possible to perform a significant statistical test since the number of snow pits they made is between 1-4 depending on the observer. A sentence has been added in the discussion (section 4.1, line 365) to support our assumption that the influence of the observer in our results is not significant.

For the uncertainty of the snow depth, it was not evaluated and presented in our manuscript since it is not necessary for the SWE calculation with snow samplers. The snow depth is only used to evaluate during the measurement if the ratio between the snow core length and the snow depth is sufficient to consider the SWE measurement valid. For the uncertainty due to instruments, the snow depth was not considered because it was not used for SWE calculation. According to Equation 1 (line 150) and Equation 2 (line 154) used for the SWE calculation, snow depth is divided by itself when combining the two equations. Although the assessment of the snow depth uncertainty by different snow sampler is relevant, its assessment deviates from the main objective of our study.

Regarding the typical snow conditions at our study site, a new subsection (3.1 Snow Measurements Distribution) has been added reply to this comment, also identified by the anonymous referees #1 and #2. In addition to describing typical snow conditions, new figures have been added showing the distribution of snow depth, density and SWE values encountered during the 4 years of measurement.

4. Also, it is not possible to individually sample snow layers that have a width < 5 cm, and this should be clearly indicated in the paper, along with a discussion on the sampling practice. I believe that the snowpit was sampled in a continuous fashion, but the current draft of the paper does not clearly communicate this sampling practice.

As mentioned in more detail in the response to Charles Fierz's comment (DOI: 10.5194/tc-2021-354-AC4), the strategy applied was density per snow layer sampling. We could not actually take measurements in layers with thickness < 5 cm. The method for estimating the density for these snow layers was done in two ways. A first by applying the average density of all the layers sampled (snow pit 1-a). The second by applying the average density of the closest layer, and with the same type of snow grains if possible (snow pit 1-b). For these two methods, ice layers density has been calculated by applying a density of $0.7175 \text{ g}\cdot\text{cm}^{-3}$. This density was obtained by field measurements in 2019. These details are explained at lines 183-190 of the submitted manuscript.

5. Additional information should be added to the paper on how the results are important in a regional context.

Through your comment and those made by the anonymous referees #1 and #2, additional information has been added to the discussion and conclusion in the revised version of the manuscript to clarify the regional scope of our results.

6. Some additional clarification should be added related to sampling procedures and the use of formulae.

This is a good suggestion. Indeed, the equations and measurements made in the field are described in the method section of the manuscript. Additional information has been added by your comment to relate how the measurements are then used in the equations presented for SWE calculations.

7. I believe that the study is novel in a regional context and therefore contributes to the literature. The paper is an excellent fit for The Cryosphere journal and without hesitation, I would cite a revised version of the paper in a future review paper written on snow and snowpack processes. I ask the editors to consider the eventual publication of this paper after the authors have addressed some comments. I think that the authors should submit a major revision, but the paper is a valuable contribution to the literature and provides novel data in a regional context.

We thank you for your comments. As responded to reviewers and community comments, we were able to identify modifications to be made to our manuscript to better clarify the regional context related to the methods used and the scope of our results. Relevant information has also been added to the revised version of the manuscript to avoid confusion regarding the measurements and analyzes performed in our study.