

Tc-2021-354 - Author's response

Dear Editor Guillaume Chambon,

On behalf of Sylvain Jutras, co-author, and myself, I thank the two anonymous referees for their comments on the revised version of the manuscript. Their review allowed us to make further changes to the manuscript to improve its quality. The track-changes version allows you to see all the changes made compared to the latest version. The most important modifications were made mostly in the section Conclusions. Additions have been made to the manuscript in order to clarify even more the scope and novelty of our study.

In the following, we have compiled all comments made by the two anonymous referees on the revised manuscript with detailed responses in a point-by-point form.

Best regards,

Maxime Beaudoin-Galaise

(on behalf of Sylvain Jutras, co-author)

1. Reply to Anonymous Referee #1 comments on revised manuscript " Comparison of manual snow water equivalent measurements: seeking the reference for a true SWE value in boreal biome" by Maxime Beaudoin-Galaise and Sylvain Jutras, The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-354-RC1>, 30 Apr 2022

On behalf of Sylvain Jutras, co-author, and myself, I thank the anonymous referee #1 for his comments on the revised version of our manuscript. Following the reading of his report, responses to each comment have been formulated point-by-point. It is possible to see the changes made to the manuscript in a new author's track-changes file.

Comments from the reviewer in blue

Answer in black

Modification to the manuscript in red (line number related to track-changes file)

Thanks to authors for the clear and concise reply. The paper definitely improved a lot, especially the title is now more adequate. However, I still do not agree on the following points, which I believe needs to be adjusted or at least discussed, before acceptance.

- 1.1. L77 & L191: Only one reference explicitly mentions that a layer is defined as a “snow strata with distinctive properties” (Pomeroy and Gray 1995). The other three publications just use generic sentences like “by integrating the individual layer densities measured in the pit”, without in advance explaining how an individual layer is defined. Experiences demonstrate that the exact interpretation of such sentences heavily depend on the background of the individual reader (e.g. avalanche service observer vs scientist). The two references of Senese et al. (2018) and CAA (2016) possibly really estimated density based on layers with distinctive differences. However, this is not surprising since both references refer to instruction of avalanche services, where the density differences of distinctive different layers are the main interest and not the SWE of the entire snow pack. Moreover, the statement of the authors that “there is no scientific literature stating that the density measurement per snow layer is incorrect” demonstrates that there is misunderstanding. If you are interested in the density of stratigraphic snow layer than you actually ought to do this. However, it is definitely not the ideal measurement procedure, if we (like authors) are interested in the SWE of the entire snowpack. Finally, one important “official, global” reference, which explicitly mentions continuous sampling, is missing (WMO Guide to Instruments and Methods of Observation., Volume II – Measurement of Cryospheric Variables, 2018)**

Thank you for your detailed comment. First, we understand that the continuous sampling method is more appropriate when someone is interested to measure the SWE of the entire snowpack, like you commented. Preferably, it would have been beneficial to have carried out snow pits by continuous sampling. It would have been even better to have done both density sampling methods (stratigraphic and continuous) for a more efficient comparison. Unfortunately, this is not the protocol that was used at the start of the measurement in 2016. The snow pit protocol with

stratigraphic density sampling was chosen in order to satisfy the needs of the users of the NEIGE site and potential studies, and not only the present study. Despite this, we believe there is an important difference between a method that is incorrect and a method that is not ideal. In addition, with generally large numbers of ice layers in the snowpack encountered during the study, continuous sampling will have brought other biases related to the difficulty of taking density measurements through ice layer. We believe that the changes already made in our manuscript allow the reader to fully understand the scope of our results in relation to the type of snow pit performed.

According to your recommendation, this is an oversight on our part and the reference to the WMO has been added to the lines 82 and 215 (in addition to the full reference in the section References at line 753).

1.2. L221: Despite the fact the manuscript it full of uncertainty considerations my original comment about the uncertainty due to a missing reference snow depth is still not mentioned. It is still not clear if the snow tube SWE was acquired close enough to the snow pit SWE to use the same snow depth. Especially, it is still not clear how much many mm or cm are lost by using a plate in the slot. In the answer to my comment the authors correctly write sentences like: “If there is a difference between the length of the snow core and the snow depth of the snow cover, we assume that it is due to snow compaction in the tube during sampling”. Please add some corresponding sentences, especially because the ground surface of common measurement locations is not as perfect as at NEIGE site.

First, the measurements with the snow samplers were taken as close as possible to the snow pit, possibly at a maximum distance of 1 m. Although the measurements between the samplers and the snow pit were close together, the SWE depth was measured at each snow sampler measurement. The average snow depth variability for each sampler is described in section 3.1 (lines 327-328). The sampler's snow depth was only used to validate that the ratio between the snow core length and the snow depth was above 60% in order to ensure a valid measurement. Even if it's contrary to intuition, the SWE for the samplers was calculated without considering the snow depth, unlike the snow pit. The calculation was done only by considering the weight of the full sampler and the empty sampler, since the snow depth was withdrawn when Eq. 1 and 2 (lines 163 and 167 in the manuscript) are combined. It is also for this reason that the uncertainty of the snow depth measurement was not considered for snow samplers for the uncertainty due to instruments.

For large diameter samplers, which use a metal plate at its base, the sampler was insert a few extra cm into the ground before digging to its base for insert the plate. This was done to ensure that the plate cuts into the soil layer and not into the snowpack. This step is similar to what is done with the standard federal sampler, where the sampler is pushed in a few extra cm to create a soil plug at its opening in order to get the sampler out of the snowpack without snow loss. Under our site conditions, with a sandy soil, it was possible to insert the sampler into the ground without difficulty. In boreal forest conditions, it is also possible to do so, since the ground is regularly covered with a layer of humus. In the first revision of the manuscript, we added details on the presence of soil particles to validate the snow core (lines 158-159), but we forgot to include

precisions on these last points. Per your comment, a sentence has been added to line 155 to clarify this step for HQS and ULS samplers. So, it is certain that there was no loss of snow by the use of a plate, because if it was not certain that it was inserted in the soil layer, the measure was not considered valid. I understand your comment and in other contexts, some users may misuse this type of sampler due to lack of experience and this leads to under-sampling, but our objective was to compare and evaluate the performance of SWE estimation methods under optimal conditions and with meticulous observers.

1.3. L636: Since the non-continuous sampling strategy is a major source of uncertainty it is important to mention it also in the Conclusions.

This source of uncertainty was only clarified in the Discussion section, but indeed, a sentence was added in the Conclusions at line 641 to clarify this point.

1.4. L660: The recommended large diameter tube samplers make it necessary to dig to the ground surface to insert a plate in a slot at its base to prevent snow loss during its extraction. This procedure makes sense, but is unusual for long tube samplers and should therefore be mentioned also in the Conclusions.

This comment is closely related to comment 2.14 from anonymous referee #2, therefore a sentence was added to the conclusion to clarify that large diameter samplers are not suitable for all environments and were therefore designed for shallower snow cover. This addition was made at lines 655-658.

Minor point:

1.5. L159: amount

The correction was made at line 159.

2. Reply to Anonymous Referee #2 comments on revised manuscript " Comparison of manual snow water equivalent measurements: seeking the reference for a true SWE value in boreal biome" by Maxime Beaudoin-Galaise and Sylvain Jutras, The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-354-RC1>, 25 Apr 2022

On behalf of Sylvain Jutras, co-author, and myself, I thank the anonymous referee #2 for his comments on the revised version of our manuscript. Following the reading of his report, responses to each comment have been formulated point-by-point. It is possible to see the changes made to the manuscript in a new author's track-changes file.

Comments from the reviewer in blue

Answer in black

Modification to the manuscript in red (line number related to track-changes file)

2.1. Authors gave justified responses and made related text modifications for the comments from me as well as from referee #1 and community. Title is improved and describes now better the study. The improved text also clarified aim and novelty of the study. However, it could be clarified even more, in the conclusions, for example. Chapter 3.1 was suitable addition to describe the typical snow conditions and Chapter 4.2 was improved and easier to read with the modifications. I recommend to consider following minor edits and publishing in the journal

Thank you for your evaluation of the revised manuscript. As advised by your comment and those of anonymous referee #1, additional clarifications have been added in conclusion.

Minor comments:

You could consider following minor comments to improve the text.

2.2. Lines 119-122: Consider adding that distributions spatial variability analyses are available in section 3.2.1.

This addition was made at line 120.

2.3. Line 194: "snowpack"

The change has been made at line 193. A verification of all the text has been made to ensure the uniformity of the term "snowpack" in one word.

2.4. Line 195: "was inserted horizontally and upright"

This modification was made at line 194.

2.5. Line 207: consider "because of its height"

This modification was made at line 205.

2.6. Line 211: “similar snow layers”

The correction was made at line 208.

2.7. Line 320: “3.1 Distribution of snow measurements”

The section title was changed like you suggest at line 318.

2.8. Line 321: “During five winters”

Thank you for this observation, the correction was made at line 319.

2.9. Lines 359-360: Chapter between figures 2 and 3 is only one sentence. I would recommend to combine with following chapter.

The sentence has been moved to line 351 to be at the end of the previous paragraph before Table 2.

2.10. Line 460: “snow tube is too warm”

The modification was made for the term "warm" instead of "hot" at line 452.

2.11. Line 643: “based on upon small snow samples taken with density cutters,”

This addition was made at line 633.

2.12. Lines 645-647: “Although the snow pits measured in this study are based on regional protocols, the conclusions obtained remain relevant and can be applied also to other snow pit protocols.”

These corrections were made to lines 637-638.

2.13. Line 648: “other methods and areas”

The modification was made at line 639.

2.14. Lines 657-659: “it must be considered that using large diameter samplers is environment related. Large diameter samplers will not be as well suitable for all environments, like in deep snow conditions, because they are designed for shallower snow cover.” Also consider to move the added text to the end of the chapter.

The sentence has been modified and moved as suggested at lines 655-658.

2.15. Line 637: Authors stated well goal and novelty of the study in the response at first chapter of 2.6. Consider to add the text with small edits to the conclusions, for example as “In the context of the boreal biome, which is different from an arctic or alpine environment, the snow pit made with a small size density cutter is frequently used to estimate the true SWE of the snowpack. The objective of the study is to compare this method with snow samplers already used in the field (SFS and HQS) and a larger sampler developed for research purposes (ULS). Novelty of the study originates in analysing the snow pit data at the same level as the samplers instead of considering it as a reference. The snow pit -based method has been used to measure the SWE of the snowpack, but in the literature is no evaluation of its uncertainty and its measurement error at this level. This study made it possible to compare different snow samplers with ...”

Good suggestion that represents very well the conclusions of our study, the suggested sentence has been added to line 622 at the beginning of the conclusion.