Answer to the Referee #1 – Manuscript tc-2024-1708

I was unable to open the online resources for review. I tried twice and am not sure where the error is occurring so this is not currently part of my review.

Currently, these resources are accessible upon request, which must be submitted via Google Colab. We apologize you were not granted access but for some reasons we did not receive any requests. However, to ensure wider accessibility, we plan to make these resources publicly available following the paper acceptance.

The manuscript by Barella et al. describes a detailed analysis of errors in melt calorimetry for snow liquid water content (LWC) calculations. They compare this to that of freezing calorimetry and additionally conduct experiments to determine some of the random user uncertainties associated with the method. The authors present some melt calorimetry field protocol and conduct field investigations to show the utility of the methods.

Overall, I really like the concept of this study as I stated in my previous review of an earlier version of this manuscript. I think that this study adds a great contribution to the use and application of melt calorimetry. However, many of my primary concerns in the previous review have been addressed.

We thank the review for his positive comment on the manuscript.

I suggested that this may be more appropriate as a technical note somewhere since it is an advancement on existing methods and equations.

Checking on the available manuscript type in TC there is no Technical Note (https://www.the-cryosphere.net/about/manuscript_types.html). Among all the manuscript types and according to the editorial rule we think that our work could fit to be published as a Research Article.

Making the code available helps bring this towards a research article and the addition of the calorimeter constant and experiments are an excellent addition. This will greatly improve my own work and the manuscript rightfully points out the high errors resulting from methods published in my own work. However, in my opinion it needs to be re-organized to be a research article. I would like to emphasize that I really like the project and that these comments are meant to be constructive and helpful in producing a more readable and impactful final paper.

We thank the reviewer for the positive comments. We will implement all the constructive comments in order to make the manuscript more readable and impactful.

I think more background as to why LWC in snow is important could be included, as stated in my previous review. Here are some of those papers. This is a minor point that I wish the authors to re-consider. Examples of possible background papers for consideration:

Valence et al., 2022: https://tc.copernicus.org/articles/16/3843/2022/

This paper is about the spatiotemporal monitoring of the snowpack due to ROS events.

Donahue et al., 2022: https://tc.copernicus.org/articles/16/43/2022/

This paper is about the LWC mapping at millimeter scale using optical measurements.

Eiriksson et al., 2013: https://onlinelibrary.wiley.com/doi/10.1002/hyp.9666

Paper about the importance of downslope flows during snowmelt and ROS events.
Leroux et al., 2020: https://doi.org/10.1029/2020WR027466

Paper about lateral flows modeling.

Schlumpf et al., 2024: https://www.sciencedirect.com/science/article/pii/S0165232X23002872

Mechanical properties of snow.

Even if all these papers are a great contribution to the field, they are beyond the scope of our paper. However, we understand the reviewer's suggestion to include additional papers demonstrating the broader implications of LWC knowledge, rather than classifying them as state-of-the-art or related work. While we already cite eight background studies, we will incorporate the suggested five papers as well.

Major comments:

- *The frequent mention of how significant of an improvement is made by this work makes it appear more like an argument/perspective manuscript. I think the tone of the writing could be improved. The math is solid and I think the work stands on it's own quite well without the need to argue for how much of an improvement on previous work. With how frequent this is done it lengthens some of the paragraphs and I think those familiar with the literature will see the impact of this improvement.*

We will revise the article to improve conciseness and readability. Unnecessary repetitions, which were added from the previous version to highlight the novelties, will be removed and the already consolidated concepts will be streamlined for clarity.

- *There is a lot going on and the writing jumps around quite a bit. I think that the manuscript needs to be re-organized for readability. In the current form, the writing is difficult to interpret what is methods, results, or discussion/interpretation. The manuscript would benefit from having distinct Methods, Results, and Discussion sections. As it is currently written, the manuscript bounces around quite a bit and is hard to follow.*

We appreciate the reviewer's feedback (this and the next minor ones) identifying the conflation of methods and results especially for Section 3.4. To address this, we will relocate section 3.4 (including dry snow and ice cube experiments) to the results portion of the paper. In this way, the revised structure will clearly delineate methods from results, enhancing overall readability. Specifically, Sections 3.1, 3.2, and 3.3, focusing on uncertainty derivation

and uncertainty analysis, will precede the protocol definition in Section 4. Subsequently, in Section 5 we will describe how the melting calorimeter is applied in the dry snow and ice cube experiment to random uncertainty identification (formerly Section 3.4), and LWC profile estimation for WFJ and Schnalstal (current Section 5, which will be significantly shortened according to your next comments). We believe that this rearrangement of the paper structure will improve the readability.

More specific Comments (by line number):

326-328: This is one example of the type of statement that is repeated multiple times in the paper. Once the results are shown, the work stands on its own and this argument can be made once in a discussion or conclusion.

We have deleted this repetition that was introduced to better grasp the novelty of the paper.

457-465: This seems like the methods for the experiments, but not enough details are given to re-produce this work. Some details are given later, but still not quite enough.

As stated before, we moved this section on the results section of the paper and this will address this concern.

489: Were these conditions in the field or in a cold lab?

It has been performed in a cold lab. We added this information.

530: The current flow of the manuscript is odd. The protocol comes after discussion of the user experiments, even though much of the protocol is based on the uncertainty points made prior to the experiment sections. This is where a more clear methods, results, discussion could clarify much of this. The discussion of the uncertainty results would lead into the protocol discussion quite well.

The new structure change described before will address this point.

571: How did you dry the calorimeter? I assume using a rag or something, but perhaps you learned something that could be useful here to reduce uncertainty with any moisture left in the container.

Any residual moisture will contribute to the measured mass and temperature of the hot water, parameters accounted for in the calculations. However, drying the external surface of the calorimeter and its lid is essential. Moisture on these surfaces is not factored into the heat exchange equation and can affect mass measurements. That said, our practical experience suggests that residual water droplets on the calorimeter exterior have negligible impact on mass measurements.

581: The SSA and IR seem to add little to this paper that is so focused on calorimetry.

We will use the same presenting scheme used for Schnalstal also for WFJ eliminating the IR and SSA information.

591-598: This is a lot of details about the dates and max snow depth when only a single day was used. If only one of the 36 days were used, then only details from that one day are necessary.

We shortened that section and focused on the detail of that single day's measurements. This helped to shorten the paper.

638-641: I do not see the justification for not including the two depths that had different values. Yes, the LWC can vary significantly but then so could the values that you did compare. If the same volume of snow is not being tested and compared using the different methods, then all results should be used for comparison. Also, with 36 days you could have many more than 16 or 18 measurements for comparison that would be better for comparison statistically.

We thank the reviewer for his comment, which provides an opportunity to clarify this point. As Boyne and Fisk (1987) reported, "the work was conducted in a laboratory cold room over a range of 0-14 mkg per 100 mkg of snow. The range is typical of a well-drained snowpack free of stratigraphic boundaries. Attempts to compare measurement methods in stratified snowpack have not been successful because of the spatial variability of liquid water". To ensure comparability with Boyne and Fisk (1987) results, we focused on similar experimental conditions, excluding the two measurements influenced by impermeable boundaries, which produced inconsistent results between the two methods in the WFJ profile. We agree that a more comprehensive statistical analysis would provide deeper insights into the comparison of the two methods. However, due to the paper's length constraints and the removal of detailed WFJ campaign data, as suggested by your previous comment, we believe such an analysis is beyond the current scope. Nevertheless, all data will be publicly accessible upon the relative publication, enabling future researchers to conduct in-depth statistical comparisons.

672-673: "very similar" and "good stability" are not defined. At the previous site % values are given but not here. Please be consistent in quantitative comparisons.

Thanks for pointing out this issue. We modified as follows:

"Finally, to verify the consistency of our measurements, we repeated a subset of measurements at very close distances on a uniform layer. The results of these repeated measurements yielded the following θ_w values: 7.27, 7.10, 7.19, and 7.20, with corresponding standard deviations of 0.50, 0.46, 0.56, and 0.52 respectively. These values were found to be very similar and fell within the uncertainty range, demonstrating the good stability of the melting calorimetry technique. Additionally, a z-test for null hypothesis verification (Rouder et al., 2009), applied at the difference between these values confirmed the evidence."

967: Please use the appropriate reference as given in my previous review: Webb et al., 2021: https://www.mdpi.com/2072-4292/13/22/4617

We updated the reference