Response from the authors for review of 'Proposed improvement of the detection and measurements of light precipitation in the Canadian Arctic'.

We thank the reviewer for the careful evaluation of our manuscript and for the constructive comments and suggestions. The comments have helped us to improve both the clarity and scientific content of the manuscript. We have carefully considered each point and provide our responses below. Where appropriate, additional analyses have been undertaken and the manuscript has been revised accordingly.

Reviewer 2

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The authors propose a novel method to quantify the amount of snowfall that is missed by traditional precipitation gauges. Measuring snowfall accurately is a long standing, important topic particular in polar regions, the subject of the manuscript is therefore highly relevant and well suited for HESS. In general, the paper is well written, the figures are clear. I do have, however, a couple of comments that I recommend being addressed.

Major Comments

RC2.1. Light precipitation should eventually accumulate inside the Geonor and eventually trigger the sensitivity threshold. Can the authors comment on why they think this does not allow the Geonor to measure also light precipitation (even though delayed)?

Response: Thank you for this comment. We agree that light precipitation collected in the Geonor bucket should eventually contribute to an increase in bucket weight, even if the amount is initially below the reporting threshold. Therefore, gauge sensitivity primarily affects the timing and temporal distribution of interval precipitation, rather than implying that all sub-threshold collected mass is permanently lost.

We have revised the manuscript to clarify this distinction. In the revised text, we now distinguish between: (1) precipitation that is collected by the gauge but reported later once sufficient mass has accumulated, and (2) precipitation that is not collected by the gauge because of wind undercatch, which affects the total accumulation. We also note that the filtering and processing of hourly bucket-weight measurements can redistribute precipitation among adjacent intervals, introducing additional uncertainty when comparing the Geonor with higher-frequency MRR observations. This is particularly relevant for light precipitation events, where timing and event detection are central to the analysis.

RC2.2. The authors mention they heated the MRR dish. However, we found in https://doi.org/10/gb8r75 that the MRR heating is not always sufficient and half-melted snow can lead to problems due to signal attenuation. In subsequent studies (e.g. https://doi.org/10/w4b), we therefore deactivated the MRR heating at colder temperatures because dry snow does not attenuate the signal significantly (in contrast to wet snow) and is usually blown away by the wind. For the light snowfall shown in this study, half melted snow is certainly not a problem. But I wonder whether this caused

problems when deriving the fit coefficients for the Z-S relation. Did the authors observe any problems with partly melted snow during the IOP when they were at the site?

Response: Thank you for this comment and for highlighting the potential impact of antenna dish heating on MRR observations. Following this suggestion, we revisited both the instrument configuration and our field observations during the Intensive Observation Period (IOP). We subsequently confirmed that, to conserve power at the remote TVC field site, the MRR antenna heater was configured to operate for only one hour each day (00:00–01:00 UTC), rather than continuously. Consequently, the antenna was effectively unheated for the majority of the winter season.

During the IOP, we did not observe any accumulation of slush, wet snow, or ice on the antenna dish, nor did we observe any evidence of signal attenuation or reduced radar sensitivity attributable to snow or ice accumulation. Snowfall during the IOP consisted predominantly of cold, dry snow, which readily blew off the dish under the prevailing wind conditions. Although we cannot completely exclude the possibility of attenuation outside the observation period, we found no evidence that the intermittent operation of the antenna heater adversely affected the derivation of the Z–S relationship or the MRR observations used in this study.

The manuscript has been revised to clarify the antenna heating configuration at the TVC site and to discuss its potential implications for radar-based snowfall measurements.

RC2.3. In particular, light snowfall can occur during sublimation of snowfall in the atmosphere before reaching the ground. Fig. 7 shows a nice example and Fig. 6a shows that humidity was typically not 100% during snowfall. Therefore, I am concerned that this could bias the Z-S relation estimates but also overestimate the light snowfall even though the lowest altitude was only 75 m. To correct for this, I would recommend looking at the Ze gradient close to the surface, which is quite often surprisingly linear. Then, Ze can be extrapolated to the surface using the identified gradient form a linear fit. Of course, quality checks need to be employed to make sure the linear fit worked.

Response: Thank you for this comment. We agree that sublimation below the lowest usable MRR range gate could affect the relationship between radar reflectivity aloft and snowfall reaching the surface.

Following this suggestion, we investigated whether near-surface reflectivity could be extrapolated to the surface using the reflectivity gradient between approximately 300 m and 75 m during the Intensive Observation Period (IOP). The analysis focused on periods when light precipitation was observed: 19, 28, and 30 March, and 1–2 April 2024. For each period, we examined 5-minute averaged reflectivity profiles and assessed whether a linear near-surface gradient could be robustly identified.

Overall, the near-surface reflectivity profiles did not show a consistent linear pattern. Some periods showed decreasing reflectivity toward the surface, while others showed increasing reflectivity, and the corresponding R^2 values were highly variable. The clearest period occurred between 15:00 and 18:00 UTC on 1 April 2024, when many 5-minute profiles had R^2 values approaching 0.9. Even during this period, however, 64% of the profiles indicated a 2–23% decrease in extrapolated surface reflectivity relative to the 105–175 m average used in the study, while 36% indicated a 2–21% increase.

Based on these inconsistent results, we did not apply a linear extrapolation correction to the MRR retrievals. Instead, the manuscript has been revised to acknowledge sublimation below the lowest radar gate and near-surface humidity structure as sources of uncertainty in the *Z–S* relationship and MRR-derived light snowfall estimates. As additional observations become available from the TVC site, we intend to investigate the near-surface reflectivity gradient in greater detail to determine whether more robust retrieval methods can be developed for light snowfall under Arctic conditions.

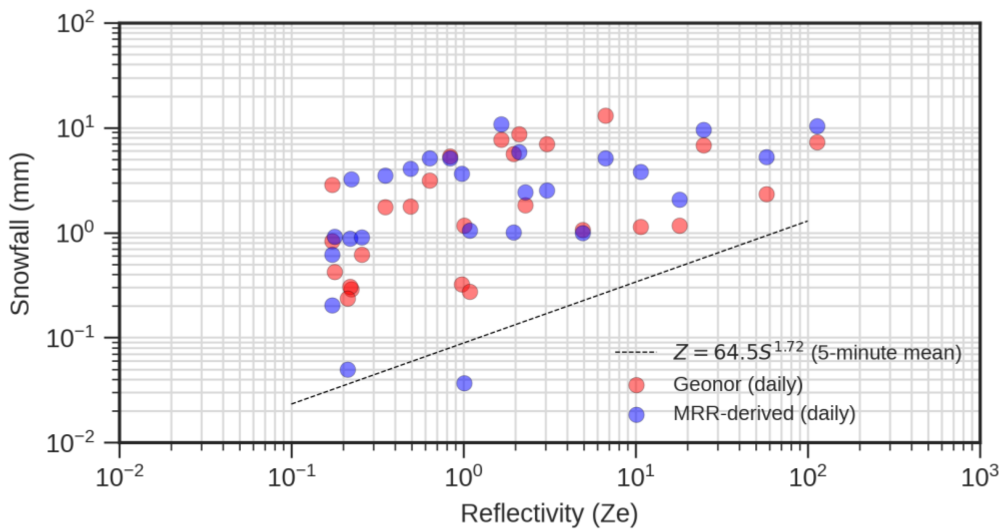
RC2.4. I would recommend adding a scatterplot showing the data of that fit. Surface wind speed could be added as a color to assess whether wind undercatch is properly corrected. It might be even an option to include only cases with low surface wind speed to limit the impact of wind undercatch?

Response: Thank you for this suggestion. We produced the requested scatterplot (shown below) and, in doing so, realized that our original description of the methodology was not sufficiently clear.

The coefficients a_{zs} and b_{ZS} were not obtained by fitting a conventional regression to paired reflectivity and snowfall observations. Instead, following Schoger et al. (2021), they were determined through an optimization procedure in which 10,000 candidate *Z–S* relationships were evaluated. For each candidate relationship, five-minute MRR reflectivity measurements were converted to snowfall and integrated to daily accumulations, which were then compared with the corresponding daily accumulations measured by the wind-adjusted Geonor gauge. The optimal coefficient pair was selected by maximizing R^2, while simultaneously minimizing the RMSE and the difference in total seasonal snowfall.

Because the optimization is based on reproducing daily accumulated snowfall rather than fitting paired reflectivity–snowfall observations, the resulting coefficients are not expected to pass through the centre of a scatterplot of daily mean reflectivity versus daily snowfall. We therefore believe that including this figure in the manuscript could unintentionally suggest that the coefficients were derived using a conventional regression-based approach, which was not the case.

Rather than adding the figure, we have substantially revised the Methods section to more clearly describe the optimization procedure used to derive the *Z–S* coefficients and to explicitly distinguish it from a conventional regression-based derivation of a *Z–S* relationship.



RC2.5. As the authors stated, the wind undercatch correction is rather uncertain. How does this uncertainty compare to the uncertainty of the Z-S relation? I wonder what the benefit is of combining gauge and radar data instead of relying only on the radar data after developing a site-specific Z-S relation? Shouldn't this be better because it avoids combining to instruments with different error characteristics?

Response: Thank you for this comment. We agree that both the wind-undercatch correction and the site-specific *Z–S* relationship introduce uncertainty into the final snowfall estimates. As the reviewer is aware, these uncertainties are fundamentally different in nature and cannot be directly compared. The wind correction represents uncertainty associated with the measurement itself, whereas the *Z–S* relationship represents uncertainty associated with the radar retrieval.

Our intention was not to demonstrate that either the Geonor or the MRR provides a superior estimate of snowfall, nor to propose that one instrument should replace the other. Rather, the objective was to demonstrate how two complementary observing systems, each with different strengths and limitations, can be combined to improve the characterization of low-intensity snowfall at remote Arctic sites.
An important motivation for this work is that many hydrologists, ecologists, and other environmental scientists rely on operational precipitation gauges, such as weighing gauges or tipping-bucket rain gauges, but may not be aware of the additional information that can be obtained from a portable, relatively inexpensive observing system. Conversely, many research groups already operate MRR systems but may not have developed a site-specific *Z-S* relationship or considered how the radar observations can complement operational precipitation measurements. We therefore hope that this study provides a practical framework for integrating these complementary observations while acknowledging the uncertainties associated with each instrument.

We have revised the Discussion and Conclusions to better emphasize that the MRR is intended to complement, rather than replace, conventional precipitation measurements, and that no single observing system can be considered a complete reference for snowfall measurements in remote Arctic environments.

Minor Comments

RC2.6 L31f: These two references are an odd choice for such a general statement. I would recommend to either replace them or to omit the reference for the first sentence.

Response: Thank you for this comment. We agree that these references are unnecessarily specific for this general statement. We have therefore removed the references from the sentence in the revised manuscript.

RC2.7 L47: Snow -> Snowfall

Response: Thank you for pointing this out. We agree that "snowfall" is the more appropriate term in this context and have revised the manuscript accordingly.

RC2.8 L84: The authors mention there was also an optical disdrometer and a hotplate deployed, what make and model was used? And at least the optical disdrometer should be able to pick up light precipitation. The authors mention technical problems, but can the optical disdrometer at least be used for a case study?

Response: Thank you for this comment. We have revised the manuscript to identify the instruments that were deployed at the site. The optical disdrometer was an OTT Parsivel2 and the hotplate precipitation gauge was a Pond Engineering K63.

The primary issue affecting the Parsivel2 dataset was the reliability of the power supply at the remote field site. We found that interruptions to the data record frequently occurred when the diesel generator automatically started to supplement battery charging, resulting in numerous gaps in the observations. Furthermore, examination of the available data indicated that the observed particle sizes and fall velocities during light snowfall were generally near the lower measurement limits of the instrument. Given the intermittent nature of the dataset and the uncertainty associated with these small particles, we elected not to include the Parsivel2 observations, even as a case study, in the present manuscript.

Since the study period, modifications have been made to the site's power system to improve instrument reliability. As longer, more continuous Parsivel2 datasets become available, we look forward to evaluating its performance for measuring Arctic snowfall, particularly for the small particle sizes observed at TVC.

The Pond Engineering K63 hotplate did not produce usable data during the study period and was subsequently returned to the manufacturer for hardware and firmware updates. Consequently, no reliable hotplate observations were available for analysis.

The manuscript has been revised to include the instrument models and to clarify why the Parsivel2 and hotplate observations were not included in this study.

RC2.9 L86, L261: Fix Reference

Response: This has been fixed in the revised manuscript.

RC2.10 Fig1: Does the box in the small map really show the same region as the big one? To me, it looks like the small one includes the coastal region which the big one does not show?

Response: Thank you for pointing this out. The reviewer is correct; the extent shown by the box in the inset map does not precisely correspond to the area displayed in the larger map. We have revised Figure 1 so that the inset accurately represents the extent of the study area shown in the main panel.

RC2.11 L137: Was the Geonor measuring at 2 m height?

Response: Thank you for this comment. Yes, the Geonor gauge was installed with the gauge orifice at a height of 2 m above the ground. We have revised the manuscript to clarify this in the description of the wind-undercatch correction methodology.

RC2.12 L151: Nice that this finally is tested in the field. When I approached the manufacturer METEK with that idea years ago, they were concerned that the memory (ROM) of the MRR is only specified for a certain number of write accesses, and it could lead to a broken MRR memory within a year or so when switching resolution that often. Can the authors comment on that? Was that overly cautious on METEK's side or has that been resolved differently?

Response: Thank you for this comment. This is an interesting operational consideration that we had not previously encountered, and we appreciate the reviewer sharing their experience with the MRR.

Since the study period, we have continued to operate the TVC MRR using the switching-resolution approach and, to date, have not observed any indication of memory-related problems attributable to this operating mode. We expect to retrieve approximately the last 12 months of data from the site later this summer, which will provide a longer assessment of the long-term operation of this configuration.

In addition, we have operated a second MRR in New Brunswick using the same switching-resolution approach since November 2023 without any apparent issues. Although this instrument has recently been decommissioned pending construction of a new weather and climate sentinel, it operated continuously for over two years without any observed memory-related failures.

While our experience is limited to our own deployments, these observations suggest that frequent switching of the range resolution has not adversely affected the instruments over the periods considered here. Nevertheless, we cannot comment on the long-term effects on the instrument memory beyond our operational experience.

From an operational perspective, one practical limitation we have encountered is that the switching-resolution method relies on the proprietary METEK software and therefore requires operation under Windows. For several of our longer-term MRR deployments, we instead configure the instrument using a Windows field laptop before transferring data acquisition to a Linux-based system, which we have found to provide improved stability for long-term autonomous operation. Although this approach cannot currently be used with the switching-resolution method, we hope that future software developments will allow this functionality to be implemented on Linux-based systems.

RC2.13 L242: These are odd values for dBZ, is it possible the authors reported reflectivity in mm6/m3?

Response: Thank you for pointing this out. The reviewer is correct that the reflectivity values were reported in linear units (mm6 m-3), rather than *dBZ*. We have revised the manuscript to report reflectivity in *dBZ* for consistency with previous studies.

RC2.14 Fig.6: I would add a legend with the shading to the Figure

Response: Thank you for this suggestion. We agree that a legend will improve the readability of the figure. A legend describing the shaded regions has been added to Figure 6 in the revised manuscript.

RC2.15 L372: Regarding the data availability, could the data publication be accelerated so it is available when the paper is published?

Response: Thank you for this suggestion. We agree that making the data available as early as possible would maximize its value to the community. While it is unlikely that the dataset will be publicly available by the time this manuscript is published, we are currently working towards a more comprehensive public release that will include MRR observations from multiple winter seasons at Trail Valley Creek, subject to data quality and completeness. We believe that such a dataset will provide a more valuable resource for future studies than the single-season dataset used in this manuscript. The data availability statement has been revised accordingly.

We again thank the reviewer for their thoughtful comments, which have helped improve both the clarity and scientific rigour of the manuscript.