General comments

This paper studies light precipitation events in the Artic, critical to estimate total snow depth but poorly measured by traditional weighing precipitation gauges. The authors use precipitation radar (MRR) data to complement the measurements of a collocated snow gauge in Northern Territories, Canada, during the winter season 2023-2024.

The MRR is considered as the reference, as unlike the snow gauge it is able to capture light precipitation events. This result is assessed by manual observations during an intensive observation period (IOP) near the end of the winter. Raw precipitation from the snow gauge are beforehand corrected using a wind-dependent function from Colli et al, 2020 that heavily affects precipitation rate. After this adjustment, the method proposed by the authors consists in gap-filling the light precipitation events missed by the snow gauge with the MRR data.

While the paper is clear, concise, well referenced, and uses very valuable data in a harsh environment, I find that its conclusions are somewhat limited as the adjusted precipitation accumulation over the whole season *without* the “MRR-filling” method proposed by the authors is closer to the reference. This point is not discussed in the paper. Also, the snow gauge accumulated snowfall with the gap-filling method is greater than the MRR’s, which is not realistic.

Moreover, I do not understand why data from another collocated snow gauge (Wilfried Laurier University (WLU) instruments) are not used to complement the study, maybe with different acquisition parameters or sample rate. Higher-frequency data for the snow gauge (for instance every minute) seem to me important to assess more precisely consistency with the radar.

I would also have appreciated a better assessment of the role of wind conditions at the site. Indeed under strong wind and blowing snow conditions the snow gauge could be expected to overestimate snowfall as precipitated snow may be remobilized in the atmosphere.

Specific comments

Fig. 2 : if I understand correctly, the instruments in the front of the picture are not those used in the study. If so, I find it quite confusing and suggest to use another picture.

l122 : I do not understand why the fact that the WLU snow gauge is used for “research purposes” prevents from using the data. It would be very interesting to compare snowfall rates between the 2 collocated snow gauges to get an estimation of the ECCC Geonor uncertainty. Moreover, are higher frequency “raw” data available for the snow gauges ? This would allow to see if light precipitation events are detected in the raw instantaneous data, and maybe smoothed out afterwards.
The Geonor datasheet specifies sensitivities finer than 0.2 mm for all models. I also suggest you specify the model / capacity in the description of the instrument.

l135 : why do you use the parameters from Marshall and not Haukelister ? Are the Marshall conditions more similar to TVC ? If so, I think it should be specified.

l137 : I do not understand why you use the same extrapolation as Colli et al 2020 to compute the 2-m wind from the 10-m measurements at TVC. If I am correct, it has been derived over grass (see their Fig. 1d) and therefore roughness length will be very different. They used z0 = 1 cm ; for bare snow a more typical value is z0 = 1 mm.

l141 : how was this threshold defined ? During the post-processing ? Is it internal to the snow gauge ? As light precipitation events undetected by the Geonor are crucial in this study I think this point should be developed.

l156 : the MRR “agrees well” with the Geonor. I think it is important to quantify the consistency between the 2 instruments at this point to grasp the percentage of events the snow gauge misses. If the MRR is considered as the reference, you could be inspired from Sect. 2.4.3 of Roussel et al, 2023 (https://doi.org/10.1002/qj.4463) that statistically evaluates the detection of daily snowfall occurrence in coastal Antarctica.

l164 : why observations are not possible during winter ? Is it due to harsh weather, or polar night ?

l172 : using the detection threshold of the camera to separate between snow and light precipitation is an ingenuous idea ! Maybe you should provide a bit more details about this instrument, such as its reference.

l190 : were the MRR switching vertical resolution described in l150 used ? Why didn’t you use the 70 m level ?

l192 : Wiener et al 2023 (https://doi.org/10.5194/essd-16-821-2024) used higher-frequency snow gauge data to test the sensitivity of the Z-S relation to time averaging at Dumont d’Urville, Antarctica. They found that “the parameters *a* and *b* are approximately independent of the integration time“.

l197 : I did not understand the argument…

l208 : to my knowledge, b can also be < 1 (e.g., Grazioli et al, 2017, Wiener et al 2023). Did you check lower values of b as well ?

l210 : the method to derive the Z-S relation seems very robust, but I would have liked to see the cloud point with the best fit to get an idea of the data dispersion. Besides, this relation is established on snow events only (i.e., precipitation strong enough to exceed the snow gauge threshold). Would you know if / how the results would be impacted when you fill the light precipitation events in the snow gauge data, using the MRR light precipitation computed from heavier precipitation ?

l220 : is the snow gauge housing heated ? If so I guess snow cannot accumulate into capping. With higher-frequency data it would have been possible to set a threshold discarding unrealistically large instantaneous values corresponding to capping events ; but those may be smoothed out with an hourly average.

l229 : given the huge impact of the adjustment on the Geonor values, did you test the sensitivity of the function ? For instance by taking the Haukelister parameters ?

l242 : Are you sure these values are given in dBz and not in mm6/m3 ? 400 dBz seems a lot.

l246 : I do not understand this sentence. You did explicitly define a Z-S relation for TVC in Sect. 2.5, didn't you ?

l269 : if the snow gauge is not heated sublimation inside the housing would be negligible, wouldn't it ?

l297 : I wonder if comparing the new amount to the unadjusted value here is relevant, as most of the increase is due to the adjustment and not to the MRR correction. The important value seems to me the +24%.

Fig.7 : I wonder if this Figure is necessary, as it shows an example of a particular event. I would have been more interested to see a mean/median MRR snowfall profile. Plus, the data is smoothed by the colorbar, especially for subpanels (a) and (c). If you want to keep the figure I suggest to refine the colorbar ranges.

l348 : did you try to discard data when wind speed exceeds a given threshold, such as done in Wiener et al, 2023 ?

l351 : if you want to measure blowing snow in the future, you could install 2 FlowCapts (Chritin et al, 1999, https://doi.org/10.1016/S0165-232X(99)00012-9) on top of each other between 0 and 2 m. Although not very precise quantitatively, they are fairly reliable to detect blowing snow events, and operate on low energy.

l360 : however, to complement the discussion, climate models are able to simulate very light snowfall and conversely struggle to simulate the heavier snowfall events (Roussel et al 2023, Wiener et al 2023).

Technical corrections

l65 : about atmospheric rivers in Antarctica, I suggest a more recent and general paper : Wille et al 2025, Nature (https://doi.org/10.1038/s43017-024-00638-7)

l77-79 : I do not understand the sentence, I suggest a reformulation.

l86 : a reference was not found

l157 : benefit instead of benifit

l233 : Arctic (with an upper case)

l256 : I suggest : of no precipitation *nor blowing snow*.

l259 : upper cases for Intensive Observation Period ?

l256 : the -1 is not exponent

l297 : I think you forgot a "by" 24%