Referee1

We appreciate your review and critique of the manuscript. Thank you.

This manuscript describes a study to relate snow fall rate and W-band reflectivity based on two observational events. Overall, the results of this study may add some new incremental knowledge of mm-wavelength radar-based snowfall remote sensing. However, some revisions are needed.

Main comments.

You should, probably add some information about radar calibration. How well is the radar calibrated?

This was added to the revised Sect. 2.3:

"Ground-based calibrations of the WCR's up-looking antenna and correlations between in-flight retrievals acquired using its up-looking and down-looking antennas were used to estimate the absolute accuracy of the WCR-derived values of dBZ. This is ±2.5 dBZ (PV11)."

Did you account for the two-way radar signal attenuation by gases and hydrometers between the aircraft and the radar resolution gate, which was used?

We did in the revised Sect. 3.2.

What are the uncertainties of the hot plate for measuring snowfall rate? Given that sometimes you are getting negative snowfall rates as much as -0.3 mm/h (Fig.8), these uncertainties can be substantial.

The revised Sect. 2.4 includes a description of the hotplate precision. This was based on a comparison between the hotplate and SNOTEL pillow systems (Marlow et al. 2023). The gauge comparison has 57 paired measurements from the HP (hotplate) and SN (SNOTEL pillow) gauges operated at the HP and SN sites in Figs. 1a-b of the revised manuscript. In the revised Sect. 4, we apply the S precision when considering the departure between our S measurements and computation-based values of S. Marlow et al. (2023) was reviewed at AMS/JAMC; we submitted revisions back to the journal two months ago.

As I understand your results are shown only by a couple of points representing mean Z and S values. Why do not you show more detailed information on the S-Z correspondence?

We do not completely understand your question.

Perhaps you are saying this: Why didn't you consider time intervals smaller than 60 s (one minute) for averaging of the hotplate data? If that is correct, then our rationale is in Sect. 3.5:

"We temporally and spatially averaged the values of Z we compared with time-averaged values of S. There are two reasons for this: 1) As discussed in Sect. 3.1, the WCR did not sample Z exactly over the hotplate, and furthermore, the width of radar beam at 1500 m range - roughly the distance between the aircraft and the ground at the overflight times - is 30 m and thus considerably smaller than the minimum horizontal distance between the aircraft and the HP. 2) Compared to the WCR, the hotplate is a relatively slow-response measurement system whose output is commonly averaged over one-minute intervals (Z18)."

Or, perhaps you are saying this: Why didn't you average further forward in time (hotplate) and further backwards in time (WCR)? We addressed this in the revision, Sect. 3.5:

"As discussed earlier in this section, the averaging scheme initializes with 60-second blocks of HP data between t_o and t_o +120 s. When we applied the scheme to data from 3 January 2017, but outside the specified time range, an inconsistency was documented. This is apparent in Fig. 8, where the t_o +120 s to t_o +180 s interval (i.e., the $i = 2$ interval) has negligible average S, while in Fig. 10, the $i = 2$ interval has a non-negligible average Z (~ 0.3 mm^6 m^{-3}). A firm explanation is not available for the inconsistency, but a factor may be the convective nature of the fields in Figs. 10a-b. Because of the inconsistency, only averages corresponding to the $i = 0$ and $i = 1$ intervals were analyzed further."

Note that the Matrosov (2007) relation was derived for Z > 0 dBZ. It needs to be stated in the paper and shown in Fig. 12 (like it is done in the PV11 paper).

Yes. In fact, some of Matrosov's points (his Fig. 5b) plot slightly smaller below 0 dBZ. Also, some of his low-Z points are for dendritic crystals while most points in his figure are for aggregates. In the revised Sect. 4, we discussed the relevance of Matrosov's calculations as a comparator for our measurements:

"Figure 12 shows our S/Z measurements after we corrected the reflectivities for attenuation. Below we compare those plotted S/Z pairs to calculations reported Hiley et al.

(2011), but first, we consider the computational S/Z relationship reported by Matrosov (2007) and its relevance to our measurements. Since the particle images (Figs. 11a-b) reveal no compelling evidence for the aggregates modeled by Matrosov (2007), a model based on that particle type is not a useful comparator. Moreover, the overlap of PV11's S/Z measurements and Matrosov's S/Z calculations has already been discussed in the literature (PV11). However, before going forward, two clarifications will be made about PV11's data points in Fig. 12: 1) Presentation clarity was what guided our selection of the S and Z axis ranges in this figure but with the consequence that 32 of PV11's S/Z pairs are not shown at Z > 10 mm^6 m^{-3}. 2) The scatter of PV11 data at the largest values of Z in Fig. 12, combined with the fact that PV11 points at Z > 10 mm^6 m^{-3} are not shown, could lead to the interpretation that the slope describing the relationship at Z approximately > 2 mm^6 m^{-3} should be decreased relative to the slope of the PV11 best-fit line. Readers who view PV11's Fig. 11 will conclude that this interpretation is not warranted."

How the reflectivities were averaged? Did you average them in linear scale (mm^6/m^-3) or in the logarithmic scale (i.e., in dBZ units)?

In the original submission, and in the revision, we averaged the Z values (mm^6 m^{-3}). In the revision (Sect. 2.3), we explicitly state that.

How well the snowfall rate and reflectivity measurements were collocated? What was the vertical separation between radar Z and hotplate S measurements used in analysis of Z -S pairs?

Section 3.6 explains this:

"Figure 11a shows imagery from 12 s of measurements acquired near the end of the sequence in Fig. 9a (00:01:02 to 00:01:14). This time interval was selected by tracing forward from t_o, along the slope of the fall streaks, to the flight level."

From Fig. 9a you can see the vertical separation between flight level and the altitude of the hotplate. The hotplate is at the overflight time (~3010 m) and the flight level is at ~4550 m. The vertical separation is therefore 1540 m. That vertical separation is also equal to the pathlengths for vapor and snow particles in Table 3 (revised manuscript) where attenuation is estimated.

Section 2.3: How did you separate components of the Doppler velocities (i.e., the reflectivity-weighted fall speeds and vertical air motions)?

We did not do that. Rather, we averaged Doppler velocities in a WCR averaging interval/domain and used Eq. A8 to calculate v_p. The latter is our "maximum likely snow particle speed toward the ground. Details are in the revised Sect. 3.5 and in the revised Appendix.

Was your assertion that particles were rimed based for the most part only on the analysis of the 2DP particle images?

We used both optical array probes. This is stated in Sect. 3.6.

Did you utilize 2DS particle measurements?

Yes. This is stated in Sect. 3.6.

You suggest that the 2DP particle images are representative of those that fell from the flight level toward the hotplate. It might be not so since the height separation was very significant.

Yes, but we don't have ground measurements of particle shapes, so, Sect. 3.6 and Figs. 11a-b are the best we can do.

Minor comments

Line 91: what are rho_ 1 and rho_3 ?

We thought this was clear from Sect. 1. Since it wasn't, we added the following to the revised Sect. 3.7:

"…In the figure legend, results from PV11 are specified as $S(\rho_1)/Z$ because those authors applied the lower of two density-size functions (ρ_1) with airborne measurements of optical particle images to calculate the snowfall rates (Sect. 1). Our data pairs plot above the $S(\rho_1)/Z$ line but within the variability of PV11's measurements."

The manuscript could benefit from additional editing.

Yes. We worked on that.

I wonder if you need any permission to reproduce the figure from PV11 paper (their Fig. 11), which is copyrighted by the AMS.

We don't know. In the Acknowledgements, we do acknowledge Gabor Vali for providing data values published in Fig. 11 of PV11.