Referee1

Reconsidered after major revisions.

Were a revised manuscript to be sent for another round of reviews:

I would not be willing to review the revised manuscript.

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

1. It appears that one of the main outcomes of the paper is adding 4 more data points (2 red and 2 blue dots in Fig. 12) to those already suggested in PV11. While it is informative but it is, probably, just an incremental result.

Below are the Conclusions from revision2-Sect. 5. In this review, you also recommended that results from Falconi et al. (2018) be considered. We did that in this revision (revision2). Discussion of Falconi et al. is in revision2-Sect.1, revision2-Sect.4, and in revision2-Sect. 5.

5 - Conclusions

The reported measurements consist of surface measurements of S and near-surface measurements of Z. The latter came from overflights of a ground site, where a precipitation gauge was operated, and were acquired using an airborne W-band radar. The values of Z were corrected for attenuation. The reported S/Z pairs plot at or above the S-versus-Z best-fit line of PV11. However, the points do not depart beyond the variability evident in a replotting of S/Z pairs from PV11. The PV11 data came from airborne measurements of W-band reflectivity, acquired within ± 100 m of flight level, and from coincident measurements of snow particle imagery. PV11 used a density-size function and a fall speed-size function, and measurements (PSD and particle images) to calculate S.

There is an offset between the S points, reported here, and reflectivity-dependent S values calculated at an upper-limit S/Z relationship for unrimed snow particles (Hiley et al. 2011). The offset is larger than the precision of the S measurement. This suggests that a measured Z and the Hiley et al. (2011) upper limit will produce an underestimate of precipitation in scenarios dominated by rimed snow particles.

New research is needed to refine the S/Z relationship for rimed snow particles. This could be computational – e.g., investigating the utility of parameterizing S in terms of both Z and density – or could be observational. Unlike the investigation of PV11, where only an airborne platform was employed, we have demonstrated that useful information can be obtained using coordinated ground-based and airborne systems. Another approach would be with only ground-based instrumentation. This would avoid some of the complications encountered in this study, including W-band attenuation and a reliance on particle imagery acquired aloft. A study with both ground-based and airborne systems would be useful for understanding a S/Z mismatch, apparent at $Z < 8$ mm^6 m^{-3}, and which is larger than the offset summarized in the previous paragraph. Elements of the mismatch are the S/Z measurements reported by PV11, the measurements reported here, and the measurement-based S/Z relationships reported by Falconi et al. (2018). These three research teams reported measurements relevant to the development of a S/Z relationship for rimed snow particles.

2. I am still concerned about an assumption that microphysical and thermodynamic information inferred aboard the aircraft is representative for the entire layer below, which is more than 1 km thick. Indeed, Figs. 9 and 10 show that there was significant inhomogeneity in the precipitating cloud between the flight level and the ground.

We interpret this as a critique of our analysis of attenuation (Sect. 3.2). An important element of that analysis is the depth of liquid cloud vs. the depth of precipitating snow. We assert (revision2-Sect. 3.2) that the vertical extent of liquid is smaller than the vertical extent of snow particles. This follows because the relative humidity measurements (Table 2) indicated subsaturation (RH < 100%) at the AmeriFlux site (AF). Our assertion is stated in revision2-Sect. 3.2.

3. I tend to think that the two-way attenuation (Table 3 in the reviewed manuscript) might be underestimated. For example, from Table 1 in Liebe et al. 1989 (i.e., the reference used by the authors) one can see that at W-band one-way liquid water attenuation at LWC=1 g/m3 is around 4.6 dB/km. At LWC=0.08 g/m3 and pathlength 1.19 km (the data for the 3 January 2017 case from Table 3 in the reviewed manuscript) it will amount to about 0.9 dB two- way attenuation only due to liquid water. Table 3 shows 0.82 dB total (including attenuation by water vapor, snow, cloud water).

Four clarifications are needed before we address your comment:

A) For cloud water, the extinction coefficient, per g/m^3 LWC, varies inversely with temperature.

B) The temperature we applied in the revision1 was the AmeriFlux temperature. This was changed to the Aircraft temperature in revision2. Since the Aircraft temperature is smaller than the AmeriFlux temperature (Table 2), the change increases the extinction coefficient per g/m^3 LWC in revision2-Sect.3.2 relative to that in revision1-Sect. 3.2.

C) The extinction coefficient, per g/m^3 LWC, in revision2 is larger than the value (4.6 dB/km per g/m^3) in your comment. What we applied (revision2) is 6.1 dB/km per g/m^3 (for 20170103). The latter is equal to the ratio of what we state in footnote d of Table 3 (0.49 dB/km for 20170103) divided by the LWC (0.08 g/m^3 for 20170103). The calculation is based on the formula on p. 191 of Vali and Haimov (2001). The aircraft-measured temperature (Table 2) was applied in the calculation (revision2).

D) Compared to what you state, the pathlength we applied ("Cloud Water", Table 3) is smaller than the value in your comment (1.19 km, for 20170103). We applied 0.59 km (for 20170103). We applied a smaller pathlength for cloud water, compared to that for oxygen (1.19 km for 20170103), Vapor (1.19 km for 20170103), and Snow (1.19 km for 20170103). This is consistent with what we state in both revision1-Sect. 3.2 and in revision2-Sect.3.2.

Now we address your comment that the attenuations might be underestimated. The “Overall Two-way Attenuation” in revision2-Table 3 (for 20170103) is 1.01 dB (for 20170103). Contributions are 0.07 dB (oxygen), 0.17 dB (Vapor), 0.58 dB (Cloud Water), and 0.18 dB (Snow). In revision2, compared to revision1, attenuation by cloud water is larger because we applied the WKA temperature (colder than the AF temperature applied in revision1). Also compared to revision1, the snow attenuation is larger because we accounted for the profile of IWC below the WKA. The revision2-Sect.3.2 has details.

Table 3 immediately follows the revision2-Sect.3.2.

Please also note: The vapor concentration (for 20170103) was entered incorrectly in Table 3 of revision1. The correct value is 1.3x10^{-3} kg/m^{3}. This correction has been implemented in revision2.

Dry air (oxygen) attenuation was not considered.

In revision2-Sect.3.2, we accounted for attenuation by oxygen.

4. There were recent studies of W-band Z-S relations in rimed snow specifically, which the authors did not mention (e.g., https://doi.org/10.5194/amt-11-3059-2018 , see table 4 in this reference). If the reviewed paper is published, it would be useful to show relations from this previous AMT-published study (for example, in this manuscript Fig.12) for comparisons with the PV11 and these author results and discuss differences.

Thank you for bringing the Falconi et al. relationships to our attention. Three S/Z relations from Falconi et al. (2018) are discussed in revision2. There are three ramifications: A) Revision2-Sect.1 has a description of what Falconi et al. (2018) measured and where they report their findings (their Table 2). However, we did not evaluate the S/Z relations that Falconi et al. derived using TMM-based calculations of Ze. The latter are in Table 4 of Falconi et al. B) Our Fig. 12b shows the S/Z relations from Falconi et al. C) We discuss Fig. 12b in revision2-Sect.4 and briefly in revision2-Sect.5.

Why Table 3 is on the last page of the manuscript?

The Table 3 is placed correctly within revision2.

Lines 37-38: Reflectivity factor represents the range corrected backscattered power not just the measured power.

This was corrected.

Can you show error bars for your four points in Fig.12?

Error bars were added to Fig. 12a in revision2.

Referee 2

accepted subject to minor revisions

Were a revised manuscript to be sent for another round of reviews:

I would be willing to review the revised manuscript.

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

The authors have made a good faith effort to address the points raised during my initial review. I appreciate their efforts.

My main concerns have largely been addressed. The revised manuscript's narrative is greatly improved by an updated and reconfigured introduction and key changes that have been implemented within the methods and results sections, including expanded content on measurement uncertainties, W-band attenuation corrections, and placing the results of this study more effectively within prior research. These changes elevate the revised manuscript substantially compared to the original submission.

A few minor comments are included below. I hope the authors consider them before the manuscript is published.

1. Lines 124-125: Since AMT is a European journal with a large international readership, I recommend adding more geographical context to the site description. Perhaps everyone knows where Wyoming is located within the larger United States footprint, but I would at least add "United States", "Rocky Mountains", "Intermountain West", or other generic wording to better describe the location.

These descriptors complicate our presentation. We did changed the axis labeling in Fig. 1a. These are now "North Latitude, ^{o}" and "West Longitude, ^{o}." We expect that this adds the necessary geographical context.

2. I do not know how to most optimally incorporate this other minor comment within the manuscript, but the study described in the manuscript is another example of just how incredibly difficult it is to extract meaningful information about snowfall properties using disparate observational sources. Perhaps this notion could be explicitly added in the introduction or conclusion (or both). The details presented in the methodology and results sections highlight the

painstaking steps required to blend airborne and ground-based measurements to extract a few meaningful data points.

In revision2-Sect.5, we accept that there are complications in the approach we took. We also say that a significant discrepancy remains among what PV11 and we report compared to what Falconi et al. (2018) report. All these research teams report measurements relevant to the development of a S/Z relationship for rimed snow particles.

3. Related to the last point, the authors did change the revised manuscript title to "On the S/Z relationship for rimed snow particles in the W-band". This title is rather generic and does not fully encapsulate the observational aspect of this study. A more appropriate title should encapsulate the observational complications of deriving S/Z relationships of rimed particles using combined airborne and ground-based measurements. A few suggestions to consider:

a. W-band S/Z relationships for rimed snow particles: Observational evidence from combined airborne and ground-based observations
b. The complex task of deriving W-band S/Z relationships for rimed snow particles using combined airborne and ground-based observations
c. New observational evidence of W-band S/Z relationships for rimed snow particles using collocated airborne and ground-based sensors

We adopted your first suggestion. Thank you.

I strongly recommend adding some sort of "observational" component to the title to highlight that S/Z relationships are derived solely from observations. Adding an airborne and ground-based component to the title also inherently advertises that matching these disparate data sources will be a key component of the manuscript and helps alleviate the notion that a rather limited dataset (4 data points) is extracted from the observations.

These are all minor points, although I would argue that a more effective title could amplify interest about this manuscript to the larger community.

Referee 3

accepted subject to minor revisions

Were a revised manuscript to be sent for another round of reviews:

I would be willing to review the revised manuscript.

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

Dear authors, many thanks for revising the manuscript. It looks like many of my comments were addressed and the reading is now more clear and fluid.

I still have one main comment about this work, which arises from the Results section and the main goal of the manuscript.

I appreciate the fact that you introduced more Z-S relationships in your fig.12 plot (Hiley et al, SSKB), it really helps contextualizing your work. The comparison in the result section though is a bit confused and it jumps all over the places, mentioning Hiley et al, but then describing Matrosov to suddenly go back to PV11 (describing the cut at Z>10mm6m-3 and the best fit line). Then back to Hiley with a mention to SSKB (but without really comparing SSKB) and then numbers to quantify the departure from Hiley and so far so on. All this to say that it would be great to get a systematic description/comparison between the Z-S relationships obtained in this work and all the others, starting for example from Matrosov, describing what kind of particles they consider and why there is this departure from the points measured in this work and so on. Also, SSKB looks like the best curve, the one that most resembles the points obtained in this work, but it is not described or emphasized. I think it should take some more space in the comparison. Also, the 4 points definitely fit within the PV11 variability and I think it would be safe to say that you confirmed the PV11 relationship with measurements (at least within its range, which is not just the best fit line). I think this might be the main goal of the manuscript as far as I can read from what you presented.

Sect. 4 of the manuscript was rewritten. Please see revision2-Sect.4.

Finally, even if the goal of this manuscript is not to provide a rimed particle Z-S relationship, it would be very nice if you could list the four relationships you obtained.

S/Z relationships, formulated algebraically, are in Sect. 1. The formulas are graphed in revision2 (Figs. 12a – b). In the Figs. 12a – b caption, and in discussion of Figs. 12a – b (revision2-Sect.4), we refer the reader to Sect. 1 where the formulas are provided. It is our opinion that this is sufficient.

Other comments:

l.21: Add "is" in "it IS also shown".

This is corrected in revision2.

l.105: Probably you mean Fig.12?

We meant Fig. 11. That figure shows the fit ("S(ρ_1)/Z best-fit line") and the S/Z pairs. The sentence was revised. The revision2 text, and the prior sentence (unchanged), is provided here:

"In addition to variance in their values of S, coming from a dependence on density, PV11 state that a value of S derived via their best-fit line is uncertain by a factor-of-ten. That uncertainty is evident in the variance of S/Z data pairs about the line in Fig. 11 of PV11."

l.133: it would be useful for contextualization to add here that WKA was flying from Laramie to Saratoga (if I understood it correctly from the response to reviewers) as test flights in preparation to SNOWIE. I see this mentioned in l.182, so not a big deal, but I feel like it would be better to contextualize it earlier on (so around l.133 when you mention the overflights or at the beginning of section 2.2 where you introduce the WKA).

The statement was moved to revision2-Sect.2.1 where it is stated that the "…flights…were flown from the Laramie, WY airport…" We hope this revision/change does _not_ leave the impression that the overpasses were flown east to west. In fact, the overpasses were flown west to east (upwind to downwind). This is stated in revision2-Sect.3.1.

L.203: "The latter was not corrected for snow particle undercatch; however, in what follows we describe that correction" – I am not sure I fully understand this sentence. With "the latter" do you mean the liquid-equivalent snowfall rate? Is it or is it not corrected for undercatchment? If there is a correction method ("in what follows we describe that correction") why isn't it applied? Please clarify this sentence to make clear if a correction has been applied (if not, why bothering describing it? If relevant it should be explained why it was not applied).

The offending paragraph, the paragraph that proceeds, and the paragraph that follows, were revised. Please see revision2-Sect. 2.4.

L.276: I assume this note is just for the manuscript and the table will be put around here in the final paper?

This is fixed in revision2.

l.342-346: I still think that mentioning this comparison does not provide any useful information.

We removed this. We also removed the Vaisala (2012) citation.

l.597: Here you mention the i=0 and i=1 intervals while in the figures I see i=0 and i=2. I haven't seen the i=2 while you were introducing the intervals in fig.6 and paragraph from l.510 to 514 where it looks like you are introducing the intervals for the first time. Maybe you want to introduce i=0, i=1 and i=2 at the beginning (paragraph 510-514)? Also fig. 8 shows two intervals, fig.7 only one, is there a reason why? Try to be consistent and if this is the way the analysis needs to be done then just specify the 3 intervals from the beginning so the reader doesn't wonder what i=2 is for most of the section before finding the explanation.

This was corrected in revision2-Sect.3.5.

L.998: the link to Marlow, S.A, J.M. Frank, M. Burkhart, B. Borkhuu, S.E. Fuller, and J.R. Snider, Snowfall measurements in mountainous terrain, in revision for the Journal of Applied Meteorology and Climatology, http://www-das.uwyo.edu/~jsnider/JAMC-D-22-0093_6.pdf, 2023 1000 does not work and it is not possible to check this reference (which is heavily used for methodology explanation).

We took down (from the web) our first submission to JAMC. We did that after responding to critiques and producing a revised manuscript. However, we did not post the revision. We apologize.

The citation is corrected, and the link is available, in revision2 of this paper:

Marlow, S.A, J.M. Frank, M. Burkhart, B. Borkhuu, S.E. Fuller, and J.R. Snider, Snowfall Measurements at Wind-exposed and Sheltered Sites in the Rocky Mountains of Southeastern Wyoming, in revision for the Journal of Applied Meteorology and Climatology, http://www-das.uwyo.edu/~jsnider/manuscript_revision2.docx, 2023