For final publication, the manuscript should be
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,

Thanks again for your revised version of the manuscript. I went through it once again and now the results section reads much better and provides a fluid description of the Z-S relationships and how yours compare.

I have a few last suggestions:

1. I appreciate the listed Z-S relationships in section one, but I would also add a table in section 4 (which you can reference from section 1 as well) with all the relationships, their acronyms, what particles have been used and what is the departure of your points. I see that you calculated the departure only from Hiley (upper limit), why is that? Why didn't you calculate the departure from all the suggested relationships for a better comparison? I would suggest all the departures in section 4 and add the results in a table.

A table was added. Table 1, in revision3, incorporates all your suggestions. The last two columns of the table have the minimum relative S differences. Please note that the differences are calculated with an absolute value (|(SHP-S)|/S), while in revision2, the differences were calculated without the absolute value ((SHP-S)/S). The S differences are discussed in Sections 4 (Results) and 5 (Conclusions).

2. As I suggested you mentioned something about SSKB being closer to your points (lines 854-855), but you are missing the why. Is it because of the density? Because of the spherical shape assumption?

Since two things are different in SSKB (density and shape), compared to Hiley et al., an answer to your question must be accompanied by uncertainty and discussion. We did that in Sections 4 of revision3. Here is the added text:

"Figure 12 compares our S_{HP}/Z' data pairs to the SSKB S/Z relationship line and Table 1 presents the relative differences between the data pairs and the SSKB line. Compared to the S/Z relationship represented by the top of the orange region in Fig. 12, and compared to the Matrosov 2007 relationship, the SSKB line plots closer to our data points (minimum relative difference ~ 0.3). We note that the only instances of $S_{HP} < S$ are three of four comparisons of our measurements to the SSKB relationship. A possible reason for this is that the density applied in SSKB (Table 1) is not entirely representative of conditions during our study. An analysis of the sensitivity of the SSKB to a change in density is needed to investigate our assertion."

Can you expand this 2 lines sentence you added? Why focusing so much on Hiley calculating departure, errors etc. when it is obviously underestimating because it does not consider the right particles.

We focused on Hiley et al. because those researchers provided an S/Z relationship applied in retrievals of S based on W-band measurements. In revision3, Sect. 4, we expanded on this issue:

"In comparisons of our snowfall rates and the upper-limit S/Z relationship line from Hiley et al. (2011) the relative difference is no smaller than 0.7 and 1.0 on 15 December and 3 January, respectively. These minimum relative differences exceed the hotplate precision (Sect. 2.4) by at least a factor of two. It is concluded that our paired values of undercatch-corrected precipitation rate and attenuation-corrected radar reflectivity provide evidence that a calculation of S based on the Hiley et al. (2011) upper-limit, when applied to rimed snow particles, is associated with a low-biased estimate of S. A retrieval based on Hiley et al.'s average S/Z relationship (not shown), which bisects the orange region in Fig. 12, corresponds to an even larger low bias. This is a concern because Hiley et al. (2011) used their average S/Z relationship to retrieve global snowfall distributions and since global observations reported in Wang et al. (2013) document the frequent occurrence of supercooled liquid within snowing clouds."

It is good to report all the information you provided about Hiley, but I still think it is important to give the same amount of space to relationship that are closer to your points (like SSKB or PV11).

Comparable space is given to SSKB, PV11, and Falconi et al. in Section 4 of revision3 (Results).

3. Why introducing a new plot in figure 12? The plot has only Falconi et al. as new lines, Matrosov and Hiley are repeated. Please consolidate in one plot for easier comparison with your points (not reported in the second plot, how are we supposed to compare?) and maybe put the legend outside the plot space.

In revision3 we redrew Fig. 12 with one panel and with the legend above the plot space.

4. In section 3.5 you introduce the intervals i=0, i=1 and i=2. You should include i=1 in the figures as well (figs. 7 to 10) since it is one of the two points you show in your results in fig. 12. For sure i=1 points are more important to show than i=2 since the i=1 are a crucial part of your analysis and in the figures it is not obvious where they are located (especially because they anticipate t0 as opposed to the ground data that follow). I think showing i=0 and i=2 only creates confusion on what is actually important to focus on.

We think the Figs. 6 schematic of the i=0 and i=1 hotplate averaging intervals and of the i=0 and i=1 WCR averaging intervals is adequate for informing the reader that the i=0 and i=1 intervals are what's most important. That said, we agree that an improvement is to indicate both the i=0 and i=1 in Figures 7, 8, 9, and 10. That change was implemented in the revision3.

Also, we stated (L548 in revision2) that Fig. 8 shows the i = 2 averaging interval as a "special case" and that this is discussed at the end of Sect. 3.5. It is also stated, at the end of the following paragraph (L559 in revision2), after introducing the airborne radar measurements (Figs. 10a-b), that the i = 2 interval is shown and that the reason for this is discussed at the end of Sect. 3.5. The statements about the i = 2 averaging interval, from revision2, remain in revision3.

5. In table 5 can you also add a column with attenuation corrected Z? So we have an idea on the values you are actually showing in the plot (fig.12) since those are attenuation corrected values.

Attenuation-corrected values were added to the final column of Table 5.

6. line 780 you mention 'summit' – do you mean the ridge line?

Yes, that is what we meant. For clarity, the text in revision3 was changed to this:

"First, the two snowfall events had flight-level vertical wind velocities (Figs. 5b and 5d) that were positive (upward) upwind of the ridgeline, and vice versa downwind of the ridgeline.

Except for the strongest downdraft on 3 January 2017, the magnitude of this variance is ~ 1 m s^{-1} (Figs. 5b and 5d)."

If that's the case can you add an indicator as you did in fig. 5 so it is more obvious where to look?

In revision3, a "Ridgeline" indicator is added to the panels showing flight-level measurements of vertical velocity (Figs. 5b and 5d).

7. In the conclusions can you also add some summary of your findings about SSKB and Falconi? You still only focus on PV11 and Hiley and disregarded some relationships that are closer to your results.

Comparable space is given to SSKB and Falconi et al. in the Section 5 (Conclusions) of revision3.

8. I see there are really a few (3) references from 2020, isn't there anything more recent to reference? Most of the references are at least 10 years old.

Two post-2019 references were added to revision3. In revision3, there are five post-2019 references. This count does not include MS theses or web links to data sets.

The post-2019 references added to revision3 are these:

Battaglia, A., Tanelli, S., Tridon, F., Kneifel, S., Leinonen, J., and Kollias, P, Triple-Frequency Radar Retrievals, In: Levizzani, V., Kidd, C., Kirschbaum, D.B., Kummerow, C.D., Nakamura, K., Turk, F.J. (eds) *Satellite Precipitation Measurement*, Advances in Global Change Research, vol 67. Springer, Cham. https://doi.org/10.1007/978-3-030-24568-9_13, 2020

Vogl, T., Maahn, M., Kneifel, S., Schimmel, W., Moisseev, D., and Kalesse-Los, H.: Using artificial neural networks to predict riming from Doppler cloud radar observations, Atmos. Meas. Tech., 15, 365–381, https://doi.org/10.5194/amt-15-365-2022, 2022