Reviewer #2:

This paper is concerned with the representation of ice particle aggregation in a bulk microphysics scheme. The paper uses observations from multifrequency and doppler radar to revise the necessary parameters. The methods seem to be very detailed and well-justified. Perhaps the weakest point is that the model used is 1-D and tuned to the observations, but these shortcomings are recognised and a reasonably well-described method for tuning the 1-D model is described.

Overall, I feel that the paper is an excellent contribution to the literature. It is a very dense paper, and I felt that sometimes the main message was lost a little in the text. Perhaps some of the technical detail (e.g. line 609 about the statistical metric used) could have been moved to the appendix and the main findings stated in the abstract? – at the moment this is more general findings that are stated.

I have no strong arguments against the paper, and recommend publication after considering some minor points.

General comment by the authors to Reviewer #2:

We thank the reviewer for the time and effort in reading the manuscript and providing constructive comments and suggestions.

We reformulated and extended the abstract to more clearly state the main findings of aggregation parameters and the proposed methodology.

In our opinion, it might be more convenient for the reader to find short definitions (e.g., of the Hellinger distance) directly in the main text without the need to read the appendix. However, we also agree that most readers might prefer a more concise text. Out of this consideration, we included only the lengthy deviations of the bulk collision rates and thermodynamic fields in the appendix and left other points in the main text.

In the following, we address the comments point by point. Line numbers refer to the non-revised manuscript.

Point-by-point reply of the authors to the questions and comments raised by reviewer#2

Line 33: I thought the sentence was confusing to the uninformed reader about the fallspeed of ice. This could be easily reworded to make it less ambiguous.

A: We have rephrased the sentence to describe the typical velocity-size relationship from small to large particles, which is hopefully more intuitive and understandable in this version.
“For smaller particles, vt increases strongly, but the increase in vt flattens with size and, finally, vt approaches a constant value of 1 m/s for centimeter-sized aggregates (Lohmann et al., 2016).”

Line 56: Atlas-type vt-size relation? Wasn’t clear what this meant at first, but I see this is defined on page 13, line 322. Maybe this could be stated sooner.

A: We avoided the specific term “Atlas-type vt-size relations” in the revised manuscript until the point when it is properly introduced and explained. On the previous occasions, we replaced it with “more complex vt-size relations”. This seems more suitable for the introduction because also other vt-size relations are imaginable that consider the asymptotic behavior of vt at large sizes. Furthermore, in the methods section, a reference to the equation of the Atlas-type vt-size relation is added.

Section 2.3: should rho_air be included? Mass mixing ratio is kg/kg. Assuming that f(m) is number per kg per mass interval, this would mean the units of Q are kg/m-3, which is ice water content.

A: There have indeed been two mistakes in our original sentence. First, the mass density and not the mixing ratio is predicted by the scheme, which is a moment of the mass distribution (see Section 2.1 in Seifert & Beheng (2006)). Second, the mixing ratio equals the mass density divided by the air density. f(m) has units of number per kg and volume (#/(kg m^3)).

Section 2.4, line 205: DWR is calibrated to remove attenuation using disdrometer measurements. Here the authors argue that DWR is correlated to the mean mass of the distribution. I did not understand the arguments why this is the case. Could this be made clearer?

A: We revised this section strongly to illustrate the effect of mean size on DWR more clearly and clarify why we can disregard differential attenuation effects in this analysis:

“Although also differential attenuation contributes to DWR (Battaglia et al., 2020), we did not include this effect in Eq.(3) because the processing of D18 already corrects for the impact of differential attenuation on DWR. D18 evaluated the absolute calibration of the observed Ze's from the Ka-Band radar using disdrometer measurements during rainfall. After correcting differential attenuation due to gases at all three frequencies, the Ka-Band radar was then used as a reference for estimating calibration biases and differential attenuation effects due to hydrometeors by comparing the three Ze's at cloud top. The DWRs caused by differential scattering are usually close to 0 dB for small ice particles present at the cloud top. Calibration biases can be identified as DWR bias which is relatively constant over time; differential attenuation effects due to supercooled liquid water, rainfall, or the melting layer vary more strongly on shorter time scales (minutes to hours). The path integrated differential attenuation estimated at cloud top was then used to correct the DWRs in the entire profile. A more in-depth discussion of various correction methods for multi-frequency radar observations is also provided in Tridon et al. (2020). If differential scattering effects are the only contributor to DWR, it correlates well with the mean mass of the distribution f(m) (Sect. 3.1.1), as can be seen from Eq. (3). For small particles, the Rayleigh approximation is valid for all frequencies and sigma_b scales with the mass squared. However, for larger particles and shorter wavelengths, sigma_b is smaller than predicted by the Rayleigh approximation and $\sigma_b(m,\lambda_2)$ is smaller than $\sigma_b(m,\lambda_1)$. As a result, particle populations that contain larger particles, e.g., due to their large mean mass, have larger DWR's than particle populations with smaller mean masses.”