Review of "Improving the Representation of Aggregation in a Two-moment Microphysical Scheme with Statistics of Multi-frequency Doppler Radar Observations"

General Comments:
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.

Specific Comments:

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

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.

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.

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?