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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A laboratory investigation into the aggregation efficiency of small ice crystals" *by* P. J. Connolly et al.

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Response to referees comments for ice crystal aggregation paper

Referee 1: Chris Westbrook

Thank you for your positive comments. Here are our responses to the questions raised:

1. As in the referees comments we understand the issues with defining the collection kernel. Our reason for choosing to define it in terms of the areas is because we can diagnose it for both single crystals and aggregates using the fractal-like dimension ($A(D) = aD^b$) as described later in the manuscript. The problem with using the maximum dimension (as described by the referee) is that this can over-

estimate the area. If we time-average the orientation of the aggregates as they tumble then this is equivalent to the formula we chose – assuming the fractal-like dimension holds true. We've tried to make this clearer in the text.

2. Yes we have now referenced paper on this.
3. OK, converted to SI units.
4. OK, we have now changed the text to reflect this.
5. Your reasoning looks sound and we prefer to include this in the paper, rather than bulk Peclet number. Hence, we have chosen to do this, thanks!
6. OK have done this
7. That cost function is correct. The timing was actually very good so I think we do a good job of simulating the fall-speeds. We have also averaged the concentration over 30 seconds to reduce this problem
8. No we didn't measure air velocity so this is another possibility which we will discuss in the text
9. OK, we will ask for landscape presentation. We have also referenced the paper suggested.
10. By rough estimate unfortunately we couldn't do much better as the images are 2-d and can be sampled randomly.
11. What you've said is correct. If we treat the aggregation efficiency as a random variable between 0 and 1 (based on no a priori knowledge of it) then we are sampling E_{agg} and looking at how it affects the goodness of fit. Clearly the best fit is where the residual is minimised, so this is our best estimate of E_{agg}, but what about a slightly different value of E_{agg} that gives a similar, although slightly higher

residual? Bear in mind that the experiment itself has uncertainties. Therefore we use both model and data to test the null hypothesis that they are the same. The way we test if the model is significantly different to the data is by assigning a confidence level (in this case 75%) above which we say the results are significantly different. This is usually how confidence intervals are defined.

12. We agree

13. Definitely worth referencing Hobbs, which I've now read in detail.

14. Yes, I think this demonstrates further the importance of shape and the interlocking mechanism.

15. I think it implies that not much aggregation occurs over 10 m of fall and we need to think of more ways to do a better experiment where we can increase the amount of aggregation occurring.

16. We've tried this but the log-scale makes it look worse in some cases as less than one order of magnitude is spanned. Hence we chose to expand the axes instead.

Referee 2: David Mitchell

Thank you for your comments. You appear to be asking for major revision; however, we are not convinced your comments justify such a major rewrite. Here are our responses to the questions raised:

1. Well sort of equal. The problem comes when they are not spherical and then what is the diameter? By using the kernel we propose the diameter is the average diameter normal to the flow, which is equivalent to the geometric cross section when time-averaged. The area is the actual cross sectional area, not the cross section area of a sphere, so we feel this more accurately approximates the

geometrical cross section. Regarding your second point about sticking and collision efficiency: we totally agree and have reworded this part of the manuscript.

2. We have read your paper and now included a reference to it in the intro. It seems to be in good agreement to this paper (ie. that dendrites give higher E_{agg})

3. Indeed it was; referenced this now.

4. There were some representative sizes in figures 6 and 7 where a scale showing what 100 microns is equivalent to is shown next to the crystal images. We agree the size distribution provides much more information so the figure is included.

5. Yes, this is an embarrassing mistake – it has been corrected now.

6. Yes see point 5. Thank you for your detailed interpretation on this point, which is much appreciated. Regarding the size sorting issue: thanks for pointing this out as it wasn't clear: size sorting does indeed occur in the experiments and it is also simulated in the ACPIM model so there should not be an issue with deriving E_{agg} by comparing the model to the data (this is now explained more clearly). Regardless of the size sorting, aggregation still occurs otherwise we would not get dimers, trimers and so on.

7. We do recognise that the area of a hexagon underestimates the area when compared to a circle (however our estimate is 0.83 not $2/3$—i.e. $3/2 \times \sqrt{(}3)R^2$ vs $\pi \times R^2$). That said this is probably within the uncertainty when the uncertainty in diffusional growth rates are taken into account. Regarding the area—vs max diameter relation. The latest studies provide measurements and a robust theory for the fractal-like dimension being 1.33 if the shapes are quasi-spherical and inspection of the CPI images shows that they are quasi-spherical (i.e. the aggregates do not form in chains).

8. Thanks for noticing. This was a classical mistake of changing a figure and then failing to change the text.

9. Good point—perhaps in very thin cirrus this is true (comment removed though)

10. Good to note that we are in agreement—I think. However, it is not clear from reading your paper that you are tracking the complexity of the ice crystals using a prognostic variable. Is this what you are doing?

11. Perhaps we were running the snow growth model incorrectly as you say and will revise the manuscript to reflect this. Thanks for your comments on this they are most enlightening. However, our comment still holds true for the way bulk microphysics models are implemented in atmospheric models so we have focussed more on this as the reason why. We didn't measure the IWC profile in the chamber and it isn't in steady-state so it is very difficult to apply the SGM in the way you are explaining. It is actually easier and more realistic to interpret with the bin model

12. Yep, points taken on board and text rearranged to suit.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 25655, 2011.