## Review of Towards a Bulk Approach of Local Interactions of Hydrometeors

## **General comments**

Baumgartner and Spichtinger derive a modified set of differential equations to describe diffusional growth of hydrometeors, one that incorporates local rather than far-field vapor density and temperature. They incorporate the new set of growth equations into a parcel model and illustrate the effects on evolution of water and ice mixing ratios and ambient temperature.

I think there are a number of interesting ideas throughout this work and that describing hydrometeor growth with local fields could ultimately be influential on mixed-phase microphysics in more complicated frameworks. But the model derivation and analysis need to be better explained before publication. For example, the coupling distance definition is not completely clear. On Page 4, Lines 10-11, you state that coupling values are those "at distance  $r_E$  from the droplet"; however, Figure 2, shows the coupling vapor density and temperature existing on a surface through the droplet center of mass. And in Equation 5, you define the coupling temperature existing at  $R_d$ . Would it help to define a coupling distance  $R_*$ , distinct from  $r_E$  or  $R_d$ , in Figures 2 and 3?

Then, I do not understand the notions of "smearing droplets" on the spherical shells or "bloating" these shells. On page 4, line 8, you state that "assuming spherical symmetric fields means that the droplet is smeared over the sphere with radius  $R_d$ ." Does this actually mean that the same vapor density is assumed for the whole shell as would exist at the surface of a single droplet? On Page 4, Line 12, you state that the sphere with coupling radius  $R_d$  is "bloated" to a shell with size  $2r_{E}$ , but presumably  $R_d$  is much larger than  $r_E$  (although  $r_E$  cannot be explicitly calculated). It could help to more thoroughly explain the significance of the gray region in Figure 3. In both instances, the wording needs to be more rigorous.

I am also still uncertain how saturation is evolved in time. It seems like this is done algebraically with the inline equations on Page 13, Line 1, rather than differentially in the parcel model. Is this correct? And the values listed in Equations 52 and 53 are initial values? Also all the parcel model simulations are performed for a single ice crystal, right? *N*<sub>ice</sub> in Equations 46 to 49c equals 1?

Clarification issues aside, I believe that the assumption of spherical ice crystals is an influential one and needs more discussion or rigorous justification than simply that "the vapor fields are very similar to those for a spherical ice crystal". You cite Lamb and Verlinde's Figure 8.24 to support this similarity, but they also describe the far field as being many crystal dimensions away, which is not the case for about half of the results you show in Section 3.2 when  $D_0 = 5R_i$ . These are also the simulations for which the effect of local interactions is most important, as you state on Page 25, Line 12. And studies like Sulia and Harrington 2011 *JGR* and Jensen and Harrington 2015 *JAS* have illustrated the large influence of aspect ratio on depositional growth of ice. The use of a saturation- and temperature-dependent deposition density as in Chen and Lamb 1994 *JAS* would also be more accurate than a bulk ice density.

Finally, I think dynamical effects could be better considered. Does the w = 0 case of Section 3.2.1 correspond to anything physically realistic? Would the large ice crystal not immediately sediment, obviating the establishment of any "spheres of influence"? Could hydrometeor sedimentation establish boundary layers that modify the local vapor densities? If the cloud parcel is turbulent, with droplets in

constant motion relative to the larger ice crystal, is the concept of a "sphere of influence" really meaningful? These kinds of questions should be considered in the conclusions, and particularly before any LES implementation.

## **Specific comments**

Page 1, Abstract: The abstract would be made more compelling by explicitly stating what the "specific scenarios" are for which cloud droplets last longer.

Page 3, Line 29: Here you could go ahead and more rigorously define "non-negligible" crystal influence being when the relative deviation of the vapor density from the environmental value is greater than 0.1% as stated in Section 2.2.2.

Page 6, Equation 6 and Line 15: I think it makes more sense to immediately give the physical explanations of these three terms  $I_1$ ,  $I_2$ , and  $I_3$  after Equation 6 and then proceed to expand them mathematically. Otherwise, Equation 6 appears rather arbitrary.

Page 6, Lines 21-26: To me, it makes more sense to first write out the rate of water vapor exchange through the spherical surface (or just say water vapor flux) prior to Equation 3a and then only mention it again in the description of  $I_2$ .

Also, because you have already introduced the notion of "radii of influence", it seems clearer to me to define the water vapor flux with a double integral over this radius of influence and all angles, rather than introducing the notion of a "ball of influence" too and defining flux with a surface integral.

Page 6, Equation 9: I understand that you cannot simply assume that the gray shell in Figure 3 is filled completely with droplets; however, I do not understand the justification for instead assuming that it is filled with these droplets' "spheres of influences", as in this *Z* factor. Is there a physical reason that these "spheres of influences" could not overlap in-cloud?

Page 8, Lines 5-6: Given that *D* is generally a diffusion coefficient, it would be less confusing to use another variables for the positive constants in Equations 17a, 17b, and 18.

Page 9, Lines 20-22: It is a small difference, but I calculate  $D_0$  = 8.66 x 10<sup>-4</sup> m. Have I missed something?

$$\begin{aligned} R_d &= \frac{\sqrt{3}}{2} \mathscr{L}_k = R_i + \mathscr{D}_0 \\ \frac{\sqrt{3}}{2} \frac{1}{\sqrt[3]{\mathcal{N}} - 1} &= 100 \times 10^{-6} \text{ m} + \mathscr{D}_0 \\ \frac{0.866}{\sqrt[3]{\mathcal{N}} - 1} &= 100 \times 10^{-6} \text{ m} + \mathscr{D}_0 \\ \mathscr{D}_0 &= \frac{0.866}{\sqrt[3]{1000} \text{ cm}^{-3} - 1} - 100 \times 10^{-6} \text{ m} \\ &= 0.866 \left(\frac{1}{9}\right) \times 10^{-2} \text{ m} - 100 \times 10^{-6} \text{ m} \end{aligned}$$

Page 9, Equation 23 and Page 10, Lines 10 and 11: If you are already using the far field pressure and temperature to calculate the surface water vapor density in Equations 23 and 28, what do you use to

calculate the far field water vapor density? Does the temporal derivative of the far field vapor density in Equation 20 come from the Clausius Clapeyron equation also?

Section 2.2.4 and Figures 5 and 6: I do not see where n = 1.8 appears in these figures. You need to clarify why and how the droplet and ice mass evolutions allow you to derive this parameter. As a side note, you spent Section 2.2.3 justifying the use of  $N_d = 40$ . Why don't you use this value in this section rather than the seemingly arbitrary values of 14 and 38?

Page 14, Lines 1-3 and 11: Will it actually be possible to make the necessary measurements to constrain parameters or assess the validity of the Z factor? You might mention the apparatus or technique that would be appropriate to measure vapor densities at the requisite spatial scales.

Page 15, Lines 20-27: This paragraph needs clarification. Introduce all the number variables first  $N_i$ ,  $n_d^j$ ,  $n_d^o$ . Then introduce all the mass variables  $M_{ice}$ ,  $m_d^j$ ,  $m_d^o$  and finally the temperatures  $T_i T_d^j$ ,  $T_d^o$ .

At the moment, I am unclear why the liquid mass variables are introduced as  $M_i^j$  and  $M_i^o$  here, as they have been denoted  $m_d$  earlier and appear this way in Equation 46 also. Extending this point, although it is a detail, it will help with clarity if you use consistent subscripts for the liquid phase, either *I* or *d*, and the ice phase, either *ice* or *i*, throughout. It would also be helpful to consistently use the upper case letters for the ice crystal variables and lower case ones for the droplet variables, as you mention early on in the derivation.

Page 16, Line 12 to Page 17, Line 2: It would be easier to read this list of simulations in a table format.

Page 16, Lines 17-20: I understand that these three values of saturation ratio are chosen as the three cases shown in Figure 1. Is there any other reason for the exact values chosen? i.e., why  $S_{\infty}$  = 0.847 rather than simply 0.8 or 0.9? Are the results quite particular to the  $S_{\infty}$  value?

Page 17, Line 6: Why is  $N_d$  limited even to [30, 800]?

Page 17, Lines 7-9: The purpose of the list of citations is not clear. Please reword, perhaps like "*Previous studies indicate a large scattering of microphysical parameters, especially LWC and IWC [Fleishauer et al., Hobbs et al., Noh et al., ...]. We use typical values ..."* Then what do you mean by "typical values" for 0.045 and 0.013 g m<sup>-3</sup>? These are mean or mode values from the cited observations?

Page 18, Figure 7: Really, the x-axis here does not need to run for 10 minutes because all the activity that you discuss occurs within the first 100 seconds. I think that you have included the full axis to be consistent with Figures 9, 11, etc. If this is the motivation, I think the clearest way to show results for section 3.2.1 would be a 2-by-3 figure where you show  $q_i$  and  $q_j$  for the  $N_d = 40 + D_0 = 5R$ ,  $N_d = 500 + D_0 = 30R$ , and  $N_d = 500 + D_0 = 5R$  simulations, i.e., combine Figures 7, 9, and 11 to ease comparison. Show the temperature fields separately.

Page 18, Figure 7 and Section 3.2.1 discussion: It would be informative to include the timing of the "kinks". At what time does your local interaction model stop following the no relaxation one in the left hand panel

of Figure 7? You could also include time units on the Figure 7 inset. What is the additional time for which "droplets inside the influence sphere exist"?

Page 19, Figure 9 right hand panel and Page 20 Figure 10: What causes the kink at about 400 s in the classical model?

Page 22, Figures 12 and 13: I think these figures could also be combined into a 2-by-2 panel to ease comparison at  $w = -1 \text{ m s}^{-1}$ .

Conclusions section: I think it would help readers understand the various trends you simulate if you organize them in a small table. Something like

	Stationary	Ascending	Descending
S∞	Effects most important		
	for subsaturation		
T∞			
N <sub>d</sub>			
$D_0$	etc.		

## **Technical comments / suggestions**

Page 1, Line 24: "much more severe impact"

Page 4, Line 14: "However, the idea is as follows": This transition does not seem natural. You are not contradicting anything that just proceeded.

Page 7, Lines 24-25: Reword for clarity. "Since V is the volume into which vapor diffuses in unit time."

Page 10, Line 26: "all droplets with distances smaller than Rd/3 have larger influence"

Page 11, Lines 8-9: This is a run-on sentence. "the artificial spherical shell. If n > 1, only a fraction  $n^{-3}$  is incorporated, and the remaining water vapor is released to the atmosphere". Also it would be better to say "affects the coupling values" than to say "is incorporated".

Page 13, Line 23: "where the authors also consider"

Page 15, Line 7: Better to say "normal to the surface of the hydrometeor  $\delta w_k$ . The surface integral..."

Page 16, Line 9: Remove the "503.61377pt" text.

Page 17, Line 14: "In the subsequent sections"

Page 18, Figure 8: I would suggest to move Figure 8 to supplemental information for conciseness. It is mentioned once briefly.