Atmos. Chem. Phys. Discuss., 12, C8561–C8563, 2012
www.atmos-chem-phys-discuss.net/12/C8561/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Numerical evidence for cloud droplet nucleation at the cloud-environment interface" *by* J. Sun et al.

Anonymous Referee #1

Received and published: 25 October 2012

General comments:

This article uses a 1.5 dimensional cylindrical model and a LES version of the WRF model to study the role played by pressure perturbations in creating regions of local expansion and cooling near the top of an ascending cloud turret. They show that the 1.5D model produces a thin region of enhanced supersaturation near cloud top due to these pressure effects, and a similar supersaturation maximum also occurs in the WRF simulation. These results are novel and of genearl interest, and I recommend publication subject to addressing the minor points below.

Specific comments:

1) There are some general organizational problems that make reading the paper harder

than it needs to be. The discussion makes multiple passes through the figures, first mentioning figures 1a, 2a-c, 1a again, then 1b, 3a, 3b, then back to 1c and 1d before finishing the section with 3c and 3d. This saves space, but there needs to be some warning at the beginning of section 3 about the fact that the discussion will first look at the pressure perturbation results in multiple figures before returning to discuss the non-pressure perturbation run. Similarly the fact that it's a 1.5 D cylindrical model needs to be mentioned earlier when the "special Eulerian model" (what's special about it? WRF is also Eulerian, isn't the distinguishing feature the 1.5D approximation?) is mentioned (line 26, p. 17725). The bin microphysics scheme is described in detail for the WRF model, but not at all for the cylindrical model. If they are identical then why not move this up to the beginning, if they are different, how do they differ?

2) The reader needs more guidance about what to look for in Figure 1B (line 28, p. 17728) i.e. something like "the 25 meter thick band of elevated supersaturation that occurs at cloud top between 25 and 50 minutes". Similarly the sentence on line 6, p. 17734:

"The temporal and spatial evolution of the saturation ration indicates that the parabolic feature liquid water of the liquid water content is due to the new activation of cloud droplets"

occurs in a discussion of figure 4, but seems to be referring back to figures 1 and 2. And the only obvious parabola in figure 4 is in the number density (4c) not the liquid water content?

3) Grabowski and Morrison (2008) solved the spurious supersaturation problem by writing a new monotonic advection scheme for supersaturation, and then diagnosing thermodynamically consistent water and temperature fields using that prognostic supersaturation. The solution described on p. 17729 instead simply limits the rate of change of temperature when evaporation and condensation are "excessive" (line 19) What is the quantitative definition of excessive? and does this approach conserve en-

ergy and water? Are you satisfied that the more sophisticated approach of Grabowski and Morrison is overkill?

4) On p. 17734 the authors state that "the low liquid water content at the cloud summit cannot be explained by the entrainment mechanism for the simulation" Why not? (for example, are the thermodynamic variables in this region inconsistent with cloud environment mixtures of conserved variables?)

Technical corrections:

Figure 1b – supersaturation colorbar has units of g/mˆ3

Figure 3 – I'm assuming that (Lnm) means natural log of the mass of the bin, but there should be a mention (especially if I'm wrong).

Spelling/Grammar – "Eulerain", "from basic state", "all great than", "should keep spatially continuous",

---

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 17723, 2012.