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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Modelling the effects of gravity waves on stratocumulus clouds observed during VOCALS-UK” *by* P. J. Connolly et al.

Anonymous Referee #2

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The authors perform a series of numerical simulations of a VOCALS case in order to evaluate the importance of gravity waves and precipitation mechanisms on low-altitude cloud fraction. The research question of how important the upsidence wave is in modulating SEP cloud properties is important. The baseline simulation is reasonable, as is the construction of most of the sensitivity experiments.

The manuscript makes the claim (in the abstract and elsewhere) that the additional entrainment of warm, dry air is responsible for lowered cloud fraction in the cases where gravity-wave motion is imposed. Unfortunately, no entrainment rates or fluxes are ever shown. Perhaps I am misinterpreting (see comment below), but the only evidence presented to back this claim âĂŤ the passive tracer profiles âĂŤ seems to show the opposite effect, i.e., that the gravity-wave runs entrain less. The manuscript

invokes enhanced evaporation associated with greater entrainment but neglects to discuss changes in longwave cooling accompanying the thicker or thinner clouds. Both the longwave and evaporative cooling are important in driving turbulence.

The sensitivity simulations seek to identify differences in entrainment between the runs, but the vertical grid spacing (20 m) is very crude for such a focus on entrainment. This grid spacing amounts to differences of only a few grid points separating the inversion among the different simulations. Numerical simulations with radiatively active smoke clouds (Bretherton et al. 1999) suggest that vertical grid spacing needs to be <5 m in order to correctly represent entrainment. Many studies (even recently) have used coarser grids, and using 20 m does not mean this effort is fatally flawed, since what is important here is the difference between entrainment between the runs (as long as the control simulation is reasonable). But the authors need to do some sensitivity experiments to demonstrate that the entrainment sensitivity is reasonable. Perhaps doing a couple simulations with the same number of horizontal grid points but at much finer grid spacing would be in order. The even smaller domain would not permit any mesoscale organization, but it would serve the purpose for evaluating the reasonableness of entrainment sensitivity in the model.

The analysis seems rather preliminary and at this point insufficient for drawing much in the way of mechanistic conclusions. The number of figures is excessive for the evidence presented, and I suspect the time series results could be better presented another way (maybe cloud fraction as a function of entrainment rate). Showing additional evidence for claims made and tightening up the presentation will help immensely.

Specific comments:

1. Page 1721, lines 13–16. “We point out that the gravity wave packets under consideration in this paper have a different source to the ‘upsidence’ wave described by Rahn and Garreaud (2010), which is due to mechanical blocking by the Andes of the westerly flow above the boundary layer.” This is very unclear. If the intent is to say that the grav-

ity waves discussed in this paper arise from blocking effects, then the sentence needs to be reworded. Also, the appropriate citation for the upsidence wave is Garreaud and Munoz (2004), though it's fine to include Rahn and Garreaud (2010), too.

The recent gravity wave paper by Jiang and Wang (JAS 2012) needs to be cited and discussed.

2. Page 1721, lines 19–26. It should be pointed out here that these hypotheses 1. are not necessarily independent, and 2. may both be in play.

3. Page 1721, lines 29–1. More evaporative cooling at cloud top. Note that this will also result in more radiative cooling at cloud top.

4. Page 1723, lines 16–18. "The LEM is an anelastic, nonhydrostatic numerical model, with prognostic equations for the advection of momentum, mass continuity and the advection and diffusion of scalars such as potential temperature and moisture variables." This sentence is not clear. The prognostic equation would be for momentum not "advection of momentum." Also, Most anelastic models do not have a prognostic equation for mass continuity but rather solve a diagnostic elliptical equation.

5. Page 1724, lines 23–30. This level of detail in describing the sounding is unnecessary; just reference Fig. 4.

6. Page 1725, lines 4–5. This is a redundant restating of lines 13–14 on the previous page. The statements about the wind profile and the geostrophic wind should be combined.

7. Page 1725, lines 24–26. "...larger drops tend to sediment out of the cloud..." Why is this bad? Please clarify.

8. Page 1727, line 33, "compensated." This word does not make sense here. Perhaps you mean "accomplished."

9. Page 1729–1730, lines 15–19. This level of detail about constraining the aerosol

distribution seems excessive, given the focus of the paper is not on aerosol.

10. Page 1731, lines 20–21, "variability in amplitude" as motivation for why sensitivity to amplitude is being explored. This doesn't seem right.

11. Page 1730–1731, lines 9–12 and Fig. 9. The discussion accompanying this figure is confusing. The text says that figure panel f corresponds to the downward part of the wave, but this is inconsistent with Eqs. 3 and 4 for a wave starting at 05:00 local time. Fig. 9f is in the upward part of the wave (given the period of 6500 s). Please clarify.

12. Page 1732, lines 4–6, "more intense turbulence and more Cu-like clouds." No evidence is provided to evaluate this claim.

13. Page 1733, lines 8–19. If both simulations contain single, 150-m waves, with the only difference being when they occur, the explanation is excessively confusing! How the simulations are configured makes sense from Fig. 14 – just make the explanation and simulation names clearer.

14. Page 1735, line 27–30. Figure 17 shows only cloud fraction, not variance.

15. Page 1735, lines 30–32, evaporation stabilizing the boundary layer so the cloud layer is better coupled to surface moisture. This is purely speculative (no evidence presented), and evaporation below the cloud leads to decoupling, which typically means a *weaker* coupling between cloud layer and surface moisture (or a coupling via cumuliform clouds).

16. Page 1736, Sec. 4.6. Please explain why the two low vs. high CCN concentration runs used different wave amplitudes (instead of the same amplitude, i.e., varying only one parameter at a time).

17. Page 1737, Sec. 4.7. I think the wrong conclusions are being drawn from this figure. The manuscript states that the 4x150-m forcing results in much more mixing of air above the inversion into the cloud layer, but Fig. 19b shows that the 4x150 simulation has the lowest inversion height (smallest entrainment rate) and lower values

of tracer concentration in the cloud and subcloud layers.

18. Page 1740, Conclusion #2. The manuscript has not established this convincingly, and Fig. 19 seems to show the opposite, i.e., that the wave simulations entrain less.

19. Page 1741, Conclusion #6. Both hypotheses are true to an extent. Was the point of the study to rank the importance of the two hypotheses? The experimental design is not optimal if this is the purpose.

Minor comments:

1. Fig. 10. The x-axis minor tick interval is an odd choice.

2. Fig. 9 and 11. The chosen time cross sections aren't consistent. How do you determine which sections are chosen?

The writing is somewhat rough in places. Please make sure the final version is carefully read through.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 1717, 2013.