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Comment

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

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Received and published: 24 April 2013

We would like to thank the referees for their review of the manuscript. We address each of their comments below.

Anonymous referee #1

Summary:

The manuscript presented an LES study of gravity wave effects on stratocumulus clouds observed during VOCALS. The authors simulated and analyzed the variabil-

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ity of stratocumulus induced by a single or multiple gravity waves and concluded that the dominant mechanism for wave-induced clearing is the additional entrainment of dry air at the cloud top. Drizzling and CCN scavenging were found less important. The manuscript contained some scientifically interesting results and helps advance our understanding of stratocumulus variability. On the negative side, some of their conclusions may be dependent on their model configuration, specifically the small domain size and relatively coarse grid spacings, and accordingly are problematic. Additional sensitivity simulations are needed to justify the current configuration and to strengthen the manuscript.

It is a fair point to say that we should justify our configuration by performing additional sensitivity runs. We are in the process of doing this and will include them in a revised manuscript. We don't expect the conclusions will change as the control simulation is reasonable (see discussion on this point below).

In addition, the authors appeared to be over-ambitious, examining the effects of wave amplitudes, numbers of waves, timing of waves, cloud evolution, drizzling, CCN concentration, and entrainment in one paper. In my opinion, a narrower-scope and better-focused paper with solid conclusions is usually much better than a-little-bit-of-everything one, which runs the risk of being superficial.

We disagree with the referee here. What we are attempting to do is explain observations by using the LES technique as a tool for interpretation. Thus it is imperative to investigate all of these effects. We note that the CCN sensitivities are not over a wide range in CCN activity, but over the observed range encountered in the measurements recorded in the South East Pacific area during the VOCALS campaign; hence, the runs we present are necessary to properly assess how typical uncertainties in CCN measurements might affect our conclusions. We note this in the manuscript on line 287: "It is recognised that these CCN values do not represent a large variation in CCN number concentration, nevertheless the descriptors, 'high', 'medium' and 'low' are relevant in the context of this study.". With this in mind, the simulations with and without driz-

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zle directly address one of the key hypotheses about whether the cloud clears due to drizzle.

These points are further elaborated in my general comments. Overall, I recommend major revision.

General Comments:

1. A major conclusion from this work is that cloud clearing is largely due to additional cloud-top entrainment of dry air rather than drizzling. However, the vertical model spacing they used is 20 m, apparently too coarse for the study of entrainment effect. As suggested by previous studies (e.g., Bretherton et al. 1999; Berner et al. 2011), 5-m or small vertical spacing is needed to resolve small eddies in the inversion, which are important for entrainment. The authors at least should conduct one or two additional simulations to test the sensitivity of the entrainment of the passive tracer to grid spacings.

We note that the Bretherton et al. 1999 study suggests that at the resolutions we have used we could potentially be overestimating entrainment rates by 50%. There are two points that arise: (1) is the question of whether 50% less entrainment is significant to our conclusions; (2) if the control simulation is reasonable (as referee 2 suggests it is) then it is likely that the control simulation is producing the correct entrainment rates, in which case extra entrainment would still occur due to the extra turbulence at cloud top. Therefore we strongly feel that increasing the grid resolution at the inversion is unlikely to affect our conclusions. However, to confirm this, we have decided to perform an additional higher resolution simulation and will include this discussion in the revised manuscript.

LES studies of stratocumulus over recent several years highlighted the importance of the domain size. As demonstrated by Feingold et al. (Nature, 2010), cloud variability results from nonlinear interplay among cellularization and oscillation of clouds, precipitation and mesoscale circulations. A small domain such as

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Interactive Discussion

Discussion Paper



16 by 16 km² used in this study surely inhibits cloud cellularization and mesoscale circulations, which feedback on the precipitation process. While I understand that computation cost is always a limiting factor, I still want to see at least one larger domain (60 by 60 km²?) simulation to show that their key conclusions still hold for a larger domain. Otherwise, the authors should add some discussion of these caveats and in the meantime, restrain themselves from speculating on what we can learn about POCs from this study.

It is explicitly stated in the paper on line 538 that “The question of whether clearing of the cloud will lead to the formation of POCs is difficult to answer with the current set of simulations as the domain size is not large enough to model meso-scale circulations and their self-organisation.” so we have been clear that we cannot explicitly address the POC conundrum with this set of simulations. Nevertheless we are in the process of performing a larger domain run to address the referees comment.

2. A wide variety of “effects” ranging from wave amplitude, wave timing, wave numbers to entrainment and precipitation have been investigated; each was roughly discussed/described on one page. It may strengthen the paper by focusing on some of them instead of all of them, just something for the authors to consider.

Thanks for the comment, as stated above we are attempting to explain real satellite observations and the factors we have chosen to investigate are those that we feel are inadequately constrained by the observations as they stand; hence the need for sensitivity runs. Thus we stand by our approach for the revised paper.

3. The manuscript can be better organized as well. The current version includes many short sub-sections, and the shortest sub-section, section 3.2 only has 2 sentences. The authors may want to make the manuscript flow better by combining some of them. In the meantime, some model description seems lengthy and unnecessary (e.g., lines 20-30 on page 1725, top paragraphs on page 1729; a

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couple of good references should be enough).

We will reword this in the manuscript

4. The following papers are highly relevant and should be cited: Berner et al. (ACP, 2011; entrainment effect on POCs); Wang et al. (ACP 2010; gravity wave effect on clouds); Jiang and Wang (JAS, 2012; wave impact on clouds).

We will cite the papers

Specific Comments:

1. Figure 5 doesn't help much and should be removed.

We disagree that Figure 5 doesn't help. For this referee maybe it was clear enough from the text where the statistics were taken from; however, we do not feel this is generally the case. From discussions with the co-authors we felt that Figure 5 was needed as a visual representation of where the wave statistics were taken.

2. Figure 10: Please change the interval between the tick-marks on x -axis to 30 min.

This will be done in the revised manuscript.

3. P1727, line 18: What's a "time-dependent stationary wave"? Please reword it.

The wave is time-dependent but horizontally homogeneous; therefore we shall describe it as a time-dependent, horizontally homogeneous, wave.

Anonymous referee # 2

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

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cloud fraction. The research question of how important the upside wave is in modulating SEP cloud properties is important. The baseline simulation is reasonable, as is the construction of most of the sensitivity experiments.

Thank you for acknowledging that the baseline is reasonable.

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 up are the passive tracer profiles which seems to show the opposite effect, i.e., that the gravity-wave runs entrain less.

Yes this is a misinterpretation the single gravity wave initiated before sunrise results in the same amount of entrainment as the no gravity wave. The 4×gravity wave and single wave initiated after sunrise all result in more entrainment (i.e. a higher concentration of passive tracer in the cloud layer).

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.

We agree and will discuss this. It is a combination of heterogeneity in evaporation and longwave cooling that drives the turbulence, due to heterogeneity in the thickness of the clouds.

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

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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.

We are attempting to do this for the paper revision. As mentioned above we do not expect this will alter the conclusions.

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.

We disagree. The entrainment rate is a poorly diagnosed parameter from LES, so prefer to show parameters that are better diagnosed.

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 gravity 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.

OK this will be done.

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.

Good point, OK

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.

We will make this point.

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.

Yes, that's correct.

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

This has been done.

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.

This has been corrected

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Larger drops sedimenting out of the cloud result in the drizzle having a mode rather than being exponentially distributed. Within cloud we tend to get closer to an exponential distribution. This will be clarified in the manuscript.

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

No, we don't mean that. We changed the wording to “which the model microphysics and dynamics respond to”

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.

The focus of the paper is to explain observations and the physics behind them. Since aerosols can have a significant impact on the properties of the clouds we feel it is necessary to include details on how they were treated in the simulations.

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

Why not? We observed that sometimes the waves had large amplitude, and sometimes smaller amplitude. Therefore in order to investigate what the possible effect of the wave is on the cloud we need to explore it as a model sensitivity.

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.

The full period of the wave is between 5000 and 6500 seconds (we have made this clear now); hence the model runs actually used 5400s as the full period (this has been made much clearer and the runs are not particularly sensitive to this).

Hence 3/4 times the full period is just over an hour. The wave starts at 5 am so that just after 6 am it is in the trough of the wave.

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

Thank you for pointing this out. We had previously included a plot of $w'w'$ at this later time, but then it was removed. We will include it in the revised paper.

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.

OK, we will do this.

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

We didn't say it was, just that other analyses of the variance showed higher $w'w'$. As noted in our response to your point 12 we will now include this and make it clearer.

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).

We believe this point has been misunderstood. Yes evaporation below cloud normally leads to decoupling, which gives a weaker coupling between the cloud layer and surface moisture; however, here the evaporation is creating cold pools and thus a more convective regime (similar to a cold air outbreak), and, like you say coupling occurs via cumuliform clouds.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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).

We wanted to investigate whether we could suppress any warm rain effects for a deep wave by having high CCN concentrations and also whether we could enhance any effects for the control by having low CCN.

17. Page 1737, Sec. 4.7. I think the wrong conclusions are being drawn from this figure. The manuscript states that the 4×150 -m forcing results in much more mixing of air above the inversion into the cloud layer, but Fig. 19b shows that the 4×150 simulation has the lowest inversion height (smallest entrainment rate) and lower values of tracer concentration in the cloud and subcloud layers.

We think the referee is mistaken. Figure 19b shows the tracer field, not the temperature inversion. The tracer field is highest in the cloud layer for the 4×150 m wave case.

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.

See above

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.

It is really to understand what the cause of the observed cloud clearing was, warm rain or entrainment. Our manuscript shows that simulations both with and without warm rain lead to cloud clearing and that simulations with gravity waves after sunrise lead to extra entrainment, therefore our conclusion is that the dominant mechanism is entrainment of dry air.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Minor comments:

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

Not sure what you prefer, but referee 1 also had some comments and a suggestion so we will respond accordingly to ref 1.

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

The purpose of fig 11 is to show how the cloud clearing in the downward part of the $4 \times 150\text{m}$ wave case evolves relative to the control. This is alluded to in the figure caption. The purpose of Figure 9 is to show how the $1 \times 150\text{ m}$ wave evolves (hence different times need to be shown).

3. 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.

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