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Interactive Comment

Interactive comment on "Large-eddy simulation of mesoscale dynamics and entrainment around a pocket of open cells observed in VOCALS RF06" *by* A. H. Berner et al.

A. H. Berner et al.

andy.berner@gmail.com

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We thank Dr. Allen for his helpful suggestions and comments. Responses to the individual points are made below.

1/ The simulations ultimately presented in this case study are for a fixed Nc cross-section across the horizontal domain (the NCADVECT analysis is quickly removed from further analysis after small differences in LWP structure etc were observed). As far as I can tell, this is the only (and fixed) initial difference (microphysical or otherwise) between the POC and overcast regions in the domain. Assuming proportionally the same droplet size distribution in all regions, then I would have expected LWP to be *greatly different between regions at the spin up of the run but the difference doesn't manifest until 2 hrs in if I read correctly? Can the authors clarify that even active precipitation is not allowed to reduce Nc in the run?*

Re1: The Nc distribution is indeed the only specification that is initially not horizontally homogeneous. The model diagnoses cloud water content from saturation adjustment based on q_t and θ_l. Initially, both of these are initially horizontally uniform with a layer of supersaturation. Hence the initial condensation by saturation adjustment leads to identical values for q_c and hence LWP at the first timestep in both the POC and OVC. Because of the different N_c in the two regions, this implies the initial droplet size distribution has a much larger mean radius in the POC than the OVC region. The difference in LWP is then established as the differences in droplet size alter the characteristics of the cloud and precipitation in each region, so that the LWP difference between OVC and POC is established on a timescale set by the precipitation differences between the regions. The text has been amended to clarify this.

Active precipitation is not allowed to reduce N_c in the run.

2/ Subsidence is assumed to be constant (see next comment) and prognostic aerosol is not employed. Given these constraints, it is not surprising that the cloud cover in the POC does not reduce to observed near clear sky and the authors correctly note that the fixed Nc is the cause of this. However, I am concerned that gradients between the POC and overcast region gradually reduce and converge throughout the simulation, which extend into the daytime. This is the opposite of satellite observations, which show that the POCs "open up" in the daytime and "fill in" in the night time. The discussion on the role of cold pools undercutting and limiting LHF is valid but can the authors comment on why all properties converge between regions throughout the simulation whilst Nc remains fixed (but different) in each region. One would expect that whatever control is leading to the initial dynamical perturbation observed in the simulation should remain active throughout; and if not, why not? Understanding the causes of this convergence in the model may add insight into the true observed multi-day sustainability of POCs

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that this simulation is unable to capture.

Re2: This simulated daytime convergence is interesting. Since it occurs in the NC-FIXED case as well as the NCADVECT case, it is not just a result of mix-out of the aerosol gradients between POC and OVC. Our interpretation is that the daytime radiative stabilization and cloud thinning cut off the precipitation in the POC, and that the simulated cloud cover is close to 1 in both POC and OVC regions, so there is little difference in how turbulence is driven in the POC and OVC regions, hence the cloud evolves to become more similar in the daytime. Evidently, this does not occur in reality. Instead the cloud fraction in both POC and OVC regions can become greatly reduced, such that the POC region appears to spread outward into the OVC. At this point we do not know if this discrepancy is due to microphysical oversimplification, problems with large-scale forcing, or something else. We have added a paragraph calling out this issue in the revision.

We have also added a description of an additional sensitivity study performed in the smaller domain where N_c is reduced further to 5 cm^{-3} and 1 cm^{-3}, along with an additional run at 10 cm^{-3} in which subsidence is set to zero. These runs show simulated POC mean cloud-cover is sensitive to N_c, and suggest that it would match the observations if we had chosen Nc = 5 cm^{-3}. The corresponding changes in LWP are smaller, and there is virtually no change to area averaged precipitation intensity as Nc is decreased. We are rerunning an NCFIXED case like our control run but with N_c is reduced further to 5 cm^{-3} in the POC, causing cloud cover there to be more broken even during the night; it will be interesting to see if the daytime convergence still occurs in this simulation.

3/ Subsidence: I am concerned that the model runs performed here go from local night time into day (i.e. cover the full range of the diurnal cycle) whilst a constant divergence is assumed. Subsidence is correctly noted by the authors to undergo a strong diurnal modulation, yet a constant is assumed for simplicity with the rational that it is difficult to ascertain from observations. Since the strengths of this paper are its insights into

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POC mesoscale dynamics, this issue is potentially important to the central conclusions of the study. A good attempt has been made to obtain average divergence profiles from ECMWF and Quikscat data yet it would have been equally possible to take a more useful diurnal peak-to-trough amplitude in subsidence from e.g. GFS/ECMWF reanalyses. Was this not considered for reasons of numerical instability? Could the authors comment on the potential implications of their assumption? The POCs are known to undergo a dramatic diurnal cycle of their own and the relaxation/strengthening of subsidence is likely to have a highly significant role. Too often, and to detriment, free tropospheric dynamics are ignored or over-simplified in boundary layer simulations. I would be happy with the assumption used if the authors could flag the potential errors that may occur, to at least alert readers. I'd be even more interested to see what model runs with diurnally modulated subsidence looked like.

Re3: When designing the study, we felt that the subsidence cycle on the day of observation was not sufficiently well constrained by observations to include. In addition, the focus of our study is the nocturnal and early-morning POC evolution (which is what was observed in RF06) rather than the full diurnal cycle of POC evolution, which can be captured adequately with a single representative subsidence rate.

Other studies (e. g. the LES study of the 6-day cloud-topped boundary layer evolution sampled in the EPIC 2001 Sc cruise by Caldwell and Bretherton 2009 (JAS)) have looked at the role of the diurnal cycle of subsidence. They find that although the subsidence modulates the inversion height, it has a much lesser effect on the diurnal cycle of liquid water path and cloud cover, and hence is not as important to the clouds as one might envision. This is mentioned in the revision.

4/ In essence, this study simply captures the evolution and interaction between regions with different, but fixed local Nc. This is indeed a useful result and helps to pin down some of the resulting dynamical controls but perhaps a clearer summary of what the simulation does and doesn't do, and what it aims and doesn't aim to achieve, stated earlier on in the paper (it is clearly stated only in the final para of the conclusions) would *aid the reader. The strengths of this paper are the analysis of the mesoscale structure, entrainment and dynamics of POCs in general and those should be strongly pitched (section 4.3 and 4.4 are especially interesting).*

Re4: Agreed; the last paragraph of the introduction has been modified to more clearly describe the study's aims.

Technical notes:

Abstract, line 2: Consider adding "NSF-C130. . ." before "Research Flight 06" so as to distinguish this flight from other VOCALS aircraft. The simple designation of Research Flight 06 could be confused by casual readers not familiar with VOCALS.

Done.

Abstract, line 17: Revise sentence: "A secondary circulation sets up..." to "A secondary circulation is initiated in the model. . ." so as to distinguish between modelling and true observation and also potential ambiguity surrounding how the circulation is "set up".

Revised to "A secondary circulation develops within the model...".

P. 13318, line 22. Consider revising use of word "emblematic" to something more appropriate and descriptive such as "symptomatic" perhaps.

Done.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 13317, 2011.

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