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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

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This is another interesting paper in the VOCALS special issue addressing physical/dynamical processes associated with pocket of open cells (POC) observed during the VOCALS-REx RF06 using fine-resolution modeling. Compared to the other ones, this study employs a different large-eddy simulation (LES) model and cloud microphysics scheme. The focus is however on the dynamics and entrainment over the POC region that requires a very fine vertical resolution near the top of the marine boundary layer (inversion layer). A vertical grid spacing of 5 m near the capping inversion is used in the simulations. Results are evaluated closely against observations and previous modeling studies. This study adds new insight into the dynamical and entrainment process in POC region. The manuscript is well structured and written. In my opinion,

I recommend publication on ACP after the following specific comments/questions are properly addressed in the revised manuscript.

Specific comments/questions:

1) "Cloud droplet number concentration (Nc)" and "cloud condensation nucleus (CCN) number concentration" are used interchangeably in the paper, which is sometimes quite confusing, for example, when seeing non-zero Nc above the boundary layer where there is no cloud. In the sub-section title 4.1, "CCN advection" is used; however, Nc is used again in the text. I guess the real issue is that the cloud microphysics scheme employed for the simulations doesn't take CCN as an input. It's fine to use fixed Nc like in single-moment schemes, but it would be nice to make it clear upfront that Nc at some places should be viewed as CCN number concentration.

2) I have some other concern regarding the treatment of Nc. As far as I know, the two-moment Morrison microphysics scheme already has the capability/option to predict Nc. Wouldn't it be nice to include that in the model simulations? Please also comment on how this treatment impacts the prognostic raindrop number concentration and derived rain rate in the simulations.

3) On page 13322 in lines 5-25, the mysterious behaviors in the model (the sensitivity of entrainment rate to horizontal grid spacing, and surface fluxes to near-surface vertical grid spacing) need more explanations.

4) For a growing boundary layer, entrainment rate is usually calculated as the sum of the growth rate of the boundary layer and large-scale subsidence rate at the top of the boundary layer. This would give the same entrainment rate for POC and OVC. However, the two methods for calculating entrainment rate used in the paper give different answers for POC and OVC. It's explained in the paper that the actual subsidence rate is an order of magnitude larger in OVC than in POC due to the circulations depicted in Fig. 12 although subsidence was prescribed as a uniform forcing across the domain. This is truly interesting. Does this suggest that the traditional way of calculating en-

trainment rate is not applicable to POC region? Might there be a scale-dependence (e.g., on POC size and Zi) of this kind of POC-OVC interaction and the consequent effect on subsidence rates?

5) It's unclear to me how the two-hour mean stream function (Fig. 12 and at Zi for Fig. 14) is calculated. Was the growth of Zi during the two hours taken into consideration?

6) It is mentioned in the paper that modeled cloud cover in POC is too large (nearly 100%) compared to observations. Is this the reason why POC and OVC have similar radiative cooling rates in the simulation? If there were no such a discrepancy, would the conclusions about entrainment rate and subsidence rate in the POC/OVC system change?

Suggestions for some minor changes:

1) It's recommended to add "REx" to the title.

2) In the abstract, change "resolution" to "grid spacing"; also at a few other places in the text.

3) The units of temperature in "K", not "oK" (a few places on pages 13328 and 13331 and in Figs. 2 and 11)

4) On page 13331, variables in the equation in line 8 should be briefly defined.

5) Four panels in Fig. 13 are referred to as (a-d) in the text but not labeled in the figure. The order of the panels seems to be inconsistent.

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