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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "The PreVOCA experiment: modeling the lower troposphere in the Southeast Pacific" *by* M. C. Wyant et al.

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Thank you for your helpful comments and for your time and effort in preparing your review.

1) A couple of references were added to the introductory paragraph.

2) In addition to the revisions made in response to other comments, the manuscript text was carefully reviewed and a number of revisions in wording were made.

3) Introduction: paragraph starting 'While a major goal...'. It is unclear where to add references to this paragraph. These sentences apply only to the models in this study, and the schemes they used are all detailed and referenced in Appendix A.

4) Experiment setup: p. 23914 Line 20: Replaced ’low’ with ’high’. Line 22: removed ’turbulence’.

5) The lowest cam level height (64m) has been added to the text. line 22: Apparently d-1 is the journal’s choice. day-1 was used in the submitted manuscript, but it was changed to d-1 in the ACPD production process.

6) QuikSCAT is assimilated into ECMWF and the other operational systems and this is added to the discussion about Figure 2. However even these systems are free to drift in their forecasts away from observations. The other monthly mean fields presented here (LWP, subsidence, downwelling shortwave radiation) are not assimilated into any of the runs. A comment on the good agreement of winds with observations was added to the text: "The good agreement of the monthly mean meteorological fields with observations and with each other can be attributed to good initialization and the relative importance of static large scale features such as the subtropical high and trade-wind circulations which are relatively easy to forecast, compared to the time-dependent features."

7) The noise in the subsidence fields in Figure 3 is typical of some high-resolution models. The following was added to the text: "The small-scale spatial variability of monthly-mean subsidence in some models is a common behavior of high-resolution models and depends largely on their numerics and handling of topography." For this and many other figures’ captions "October 2006" was added.

8) Added the following sentence to explain the connection between subsidence and clearing: "This localized strong subsidence forces the entrainment of very warm and dry above-inversion air into the boundary layer dissipating cloud."

9) In the process of responding to other comments by Reviewer #2 (e.g. 6, 8, 11, 15) we have revised the manuscript to explore the results further with a little more physical interpretation. It is not clear from the review which other parts of the study require more attention.

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10) Our reasoning for showing only 6 panels instead of showing all the models in Figures 2,3 and 5, is stated in the paper on p. 23915 line 21: "For many fields we will show means from a representative subset of models rather than showing all models in order to simplify the presentation". If all models were included, all would be smaller plots and more difficult to see and for the reader to absorb. The impetus to describe them all in the text would also partially obscure the main points of the discussion.

11) We revised the discussion of figure 9 with the following text to make a connection with the North East Pacific: "The transect along 20S does not follow a boundary layer trajectory (the boundary layer winds are southeasterly), but it does illustrate the expected deepening of the boundary layer to the west as SST increases. This is similar to the pattern seen with increasing SST in the North East Pacific in observations and models (e.g. Siebesma et al. 2004). Like the North East Pacific, we expect solid stratocumulus nearshore transitioning to more broken trade cumulus convection further west."

12) There is no published reference for the choice of RH=60%; it provides a simple and good cross-model diagnostic which matches cloud-top well yet also works in clear regions to delimit the top of the boundary layer. This method could have problems when moist-air masses are present above the inversion, but the extreme dryness of the air above the inversion in this region basically eliminates this problem. It agrees very well with boundary-layer depth criteria using potential temperature. When applied to radiosonde profiles, it matches well with cloud-top heights measured during the 2006 Stratus Cruise along 20S. The following sentence has been extended: "For almost all models, this level is coincident with a sharp decline in cloud condensate with height, and is near the strongest vertical gradient in potential temperature."

We have provided a little more information on each of the cloud-top/bl-depth climatologies (COSMIC/CALIPSO/MODIS). There are limited large-scale climatologies of boundary layer depth covering this region (e.g. Wood and Bretherton, 2004) and we would prefer not to complicate the discussion of this figure which already includes ample contemporary observational studies and data.

13) I am aware of some model studies of this underestimate issue in other regions (e.g. N E Pacific Siebesma et al. 2004, Lock et al. 2004), but in light of the unique topographic features of this region it is not clear that the discussions in those studies are fully applicable here. Also, many studies bring up lack of vertical model resolution but this isn't directly implicated in our study because most of the better resolved models have the same problem. Line 15: We have corrected this sentence to "To compare modeled and observed LWP,..."

14) The point was noted in the conclusions that the EECRA and buoy based measurements don't agree. This point has now also been added to the discussion of figure 11b. The reviewer points out the ECMWF as being "that wrong" in terms of diurnal cycle of low cloud cover. Studies of the EPIC experiment have shown that the ECMWF does well in terms of diurnal cycle of cloud cover in this region. As ship and buoy climatologies don't agree and variability from year to year is expected, we would like to wait with this judgement until the analysis of the VOCALS field experiment. We think the downwelling LW buoy measurements are a fairly reliable measure of cloud fraction, as high cloud cover is very infrequent in the region. Ghate etal. (2009) shows good correlation of cloud fraction from downwelling LW with coincident cloud fraction from ceilometer.

15) This statement in the discussion was revised from "The forecast models also benefit from use of analyses which are typically compatible with model physics" to "The operational models also benefit because the analyses they use are often created by models with identical or nearly identical model physics, so model adjustments to the boundary conditions are greatly reduced."

16) The paragraph on POCs has been rephrased into two shorter sentences.

Additional References:

Lock, A. P: The sensitivity of a GCM's marine stratocumulus to cloud-top entrainment,

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Q. J. Roy. Meteor. Soc., 130, 3323-3338, 2004.

Siebesma, A. and coauthors: Cloud representation in general circulation models over the north Pacific Ocean: A EUROCS intercomparison study, Q. J. Roy. Meteor. Soc., 130, 3245-3267, 2004.

Wood, R. and C. S. Bretherton: Boundary layer depth, entrainment, and decoupling in the cloud-capped subtropical and tropical marine boundary layer, J. Climate, 17, 3576-3588, 2004.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23909, 2009.