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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Limited-area modelling of stratocumulus over South-Eastern Pacific" *by* M. Andrejczuk et al.

Anonymous Referee #1

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General comments:

This study is based on 42-hr regional scale WRF simulations over the Southeast Pacific. The simulated vertical profiles of humidity, temperature, cloud water, and wind speed were compared with those from the BAe-146 dropsonde measurements. Main conclusions were drawn regarding the low MBL heights in WRF simulations. Simulated cloud LWP distributions were compared with those from the GOES-10, and evolution of the simulated cloud-free regions was further explored.

The manuscript showed some interesting results related to the sensitivity of the simulated MBL vertical structures to the selection of model vertical resolutions, MBL schemes, and surface layer schemes. However, the simulations were conducted for a very short period of time and only limited numbers of dropsonde profiles were included

in the analysis. Very few existing modeling or observational studies in the current literature (such as those from the VOCALS-REx) were compared with the results from this study. The discussion of results could be more quantitative. As many as 17 figures were presented and some of them could probably be combined or removed. I am not convinced by the scientific significance of the main conclusions drawn. Therefore, I recommend that the paper should be revised in a way that results with more significant science merit are provided. The detailed comments are as follows.

1) It is not clear why the simulations were conducted only for a short period of time (42-hour) and with a particular selection of 12-13 November as the simulation period considering that the VOCALS-REx is a month-long campaign with profile measurements available from both dropsondes (BAe-146) and radiosondes (Ron Brown Research Vessel). It is likely that WRF simulations have a larger bias in the simulated MBL heights at a particular day.

2) Use of the initial and boundary conditions from GFS was concluded to be one of the two main reasons that led to the low bias in simulated MBL heights in WRF. I think this is a simple technical problem. The simulations should be rerun with a longer spinup time and with a bigger outer domain (especially, extend the domain sufficiently along the direction of inflow boundaries). Note that it is not computationally expensive to extend the outer domain.

3) It is not clear what you mean by 'the scheme confusing the cloud base change of the vertical temperature and moisture gradients with the change at the boundary-layer inversion'. In the case of using YSU scheme, clouds tend to form above the diagnosed PBL heights, which has been noted by other researchers. The MBL height should be diagnosed offline from the simulated thermodynamically profiles. When using the YSU PBL scheme in WRF, the direct model outputs of PBL heights are not the traditional defined PBL heights; they are the heights where the Richardson number is 0 in a model vertical column.

4) It is not clear whether the simulation results (low PBL biases) are consistent with those in the current literature using WRF/WRF-Chem. Very few existing modeling or observational studies in the current literature (such as those from VOCALS-REx studies) were compared with the results from this study.

5) The 'arguable POC' mentioned in this study looks more like cloud-free regions. WRF does not consider aerosol gradients, and assumptions were made in the microphysics schemes, such as a constant droplet/aerosol number concentration. Even if a 3 km resolution (9 km results were used actually for this analysis) is used, it would be surprising to see the co-existence of POC embedded in generally cloudy regions. It is not clear what the purpose is to study the cloud-free regions in the model simulations given that there seems to be large discrepancies in the simulated and observed cloud fields. Those clear spots were called 'POC' for a few times (including in Figs. 11, 14, and 15), which I think should be consistently called 'cloud-free regions'.

Minor comments:

a) Page 25522 Lines 6-9: The PBL heights observed during the VOCALS-REx are about 1000-2000 m, so the 343-m resolution near the MBL top (in the 34-layer simulation) is apparently too coarse. It is surprising that with 81 and 121 vertical levels, the vertical grid spacings are only 120 and 81 m near the cloud top, respectively. It would make more sense to only increase model vertical resolutions for the lowest 2-3 km.

b) Pages 25522-25523: I recommend the authors provide references for the descriptions of different schemes used in WRF.

c) Page 25541, Fig. 2: In panel e, those horizontal lines (black) below 0.5 km in height are confusing.

e) In Figs. 3-6, how the measurement variability is defined? The variability in the figure is difficult to interpret with observed values. I recommend the variability to be plotted as observed values $\pm$ 2*sigma/variability).

f) A lot of statements are descriptive rather than quantitative, such as in Page 25525 Lines 25-26, 'too moist', 'colder', etc.

g) In some panels of Fig. 7, a very different vertical range (y-axis) is used, is there a reason for that?

h) In Fig. 9, I am not sure why erroneous satellite data were used in this Figure. I would suggest filter the data with a solar zenith angle threshold. As you mentioned, 'the large scale pattern does not move significantly in space ...', maybe in this figure only presenting a temporal-averaged field would be sufficient rather than 4 panels with every 15-minute outputs.

j) I would recommend remove Fig. 10 since it was only used to show that the large-scale pattern does not change in space, which apparently is already reflected in Fig. 9.

k) Fig. 11. The y-axis is very confusing. Suggest using two y-axes with one for the LWP/PWP and the other for the height.

l) 'LWP' is typically defined as vertical integration of rainwater and cloud water, however, in this study only cloud water was used in integration. This should be pointed out in the manuscript.

m) Page 25531, Lines 14-16: It seems to too speculative to say 'drizzle evaporation initializes subsidence'.

n) In Fig. 12, the purple lines as described in the caption seem to be missing and the contour lines do not have labels. What is the purpose of plotting lines with theta=299.5 K? The wind speed legend is missing too.

o) Page 25533, Lines 4-9: The connection between the horizontal temperature and moisture gradients and entrainment events is a stretch. The larger variations near the inversion layer are most likely related to the variations of MBL heights.

p) Page 25533, Lines 11-17: In analyzing the evolution, advection was not mentioned. The horizontal wind was blowing mostly from southeast (the wind speeds legend is missing in Fig. 12). Assuming 10 m s -1 wind speed, the cloudy or cloud-free air will be advected roughly a horizontal distance of 36 km in an hour (9 km in 15 min). This might have an impact on the evolution analysis depending on the actual wind speed.

q) Page 25535, Lines 18-27: It is a stretch to connect your results with that of Allen et al. (2011). There are a lot more relevant papers during the VOCALS. Are you indicating that the simulation results reflect the transition from closed to open cells?

l) Page 25536, Lines 4-6: I do not think you need LES models to capture gravity waves/lower troposphere waves.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 25517, 2011.