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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Marine boundary layer over the subtropical southeast Pacific during VOCALS-REx – Part 1: Mean structure and diurnal cycle" *by* D. A. Rahn and R. D. Garreaud

Anonymous Referee #2

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General comments

Overall, this paper is a solid, well-written scientific contribution of interest to ACP's readership and deserves publication. It is a hybrid model-observation study, using WRF simulations to supplement observations in this region to obtain a better understanding of southeastern Pacific regional phenomena such as the upsidence wave and nearshore northerly winds above the MBL.

G1: The authors should clarify the relationship of their use of the VOCALS REx data to those of other participating research groups who have overlapping research and publication goals, and (following the REx data protocol) consider offering coauthorship

to representative of research groups whose REx data they are using, if appropriate citations are not yet available. This seems particularly relevant to results presented in Figs. 4, 6, 10 and 11 (as well as Figs. 3 and 4 of Part II).

G2: Fig. 6 shows an excellent pair of cross-sections with which to quantitatively compare the WRF simulations. Hence, I suggest making the same cross-sections of theta, u and v with WRF and commenting on the similarities and differences. Are there significant model biases in thermodynamic structure along the coast above the MBL? If so, would you interpret this bias as more due to model resolution or more due to model physics? Do the WRF simulations support the existing interpretation of northerly flow on 26040 line 6 as 'a geostrophic response to a meso-high over the Andean slope'?

G3: Please elaborate in the paper on your interpretation of the large negative bias in the WRF MBL height near the coast. The paper implies that the overall simulation of the cloud cover, thermal structure and winds above and below the inversion are quite good, and the horizontal and vertical resolution seem adequate for this purpose. That most other models also have this bias just makes it all the more important to get to the bottom of it.

Specific comments

S1: 26034, Section 2.2, line 7: Is some particular cloud droplet concentration specified in the Thompson scheme? Is the RRTM used for longwave and the Dudhia scheme used for shortwave radiation?

S2: 26038, line 5: What fraction of the coastal soundings at each site have a detectable MBL top? It would be nice to indicate this on Fig. 5.

Technical corrections

T1: 26034 line 1: replace 'was' by 'were'.

T2: 26034 line 5: Put parentheses around the lat and lon.

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T3: 26034 line 23: Replace 'Parameters' by 'Parameterizations'

T4: 26036 line 2: Replace 'When considering' by 'In'.

T5: 26036 line 6: Add 'SST' after 5C.

T6: 26040 line 6: Replace 'Anden' by 'Andean'.

T7: 26044 line 14: Replace 'slower than' by 'of' (since you are saying the simulated upsidence wave should propagate 0.71 as fast as the observed wave).

T8: 26049 Wyant et al. PreVOCA reference is to a conference preprint - you should cite their ACPD paper.

T9: Fig. 3d: For comparison with other studies, please precipitation into units of a rate (e. g. mm/day) rather than accumulation over the simulated period.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 26029, 2009.

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Interactive Comment