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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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This is an interesting manuscript with relevant results about a fundamental problem in weather and climate models. It is important to document in detail what the main modeling issues are in terms of the representation of stratocumulus-topped boundary layers. This paper should definitely be published after some revisions are performed. Below I highlight some of the main issues.

1) A couple of references in the first paragraph would help the reader.

2) There are several places in the text where it is clear that the quality of the written language could be significantly improved.

3) Introduction: paragraph starting "While a major goal. . ." should include references to help the reader.

4) Experiment setup: pg. 23914 – line 20: “high” instead of “low”; line 22: “boundary-layer” and “turbulence” schemes usually refer to the same type of parameterizations.

5) Page 23916, line 16: please state the height of CAM’s lowest model level; line 22: should it be “day-1”?

6) Fig.2: Is QuikSCAT being assimilated by any of the data-assimilations systems? I would ask a similar question for the other observational data-sets as well. Why are these model results so good in general?

7) Fig.3: Why are some of these results so noisy? Please be precise in the legends (e.g. is this figure for October 2006?).

8) Fig.4 (top of page 23918): Why is the significant cleared region associated with strong subsidence?

9) At places it looks like the authors could explore their results better and provide a more detailed physical interpretation.

10) Fig.5: Please explain better why do you often select only a few models to show?

11) Fig.9 and similar discussions: What can be learned from previous studies in other regions (NE Pacific and Atlantic) regarding this transition?

12) Fig.10: Do you have specific results or a reference for the RH=60% choice for model boundary layer top? Please provide more details regarding the observations and previous studies using observations of boundary layer and cloud top height.

13) Page 23921: The model underestimation of the MBL depth also happens in other regions and has been documented before. I am pretty sure that some of these earlier studies have advanced reasons for this model underestimation. Line 15: Is it not “observations” instead of “liquid water path”?

14) Diurnal cycle of cloud fraction (fig.11b) – It is interesting to note that the observational datasets do not agree in the terms of the diurnal cycle. Also, the ECMWF

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versions show virtually no diurnal cycle of cloud fraction, while other models show a significant cycle. Could ECMWF be that wrong in this context? How reliable are the observations based on the surface measurements of LW radiation?

15) Discussion: It is not clear what the sentence “The forecast models also benefit from use of analyses which are typically compatible with model physics” means.

16) This entire paragraph about POCs does not fit well into the rest of the discussion (it looks like it comes out of nowhere). If you want to mention POCs I would suggest a couple of shorter sentences.

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