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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 #3

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This manuscript present results from a short simulation (42hr) of the WRF regional model over the SE Pacific. The first part of this study examines the sensitivity of the vertical profiles (but not the radiatively important cloud properties such as cloud cover and LWP) to the PBL mixing scheme used in the model. The second part examines two mesoscale clearings in the model and concludes that they are produced by patches of strong large scale subsidence.

Overall, I don't think this is worthy of publication because the simulations offer very little that is truly new. The analysis is rather superficial. The overly shallow stratocumulus-topped PBL problem has been known for a long time and there are solutions to the problem stemming back over a decade (e.g. UK Met Office PBL scheme of Lock et al. 2000, plus Bretherton+McCaa+Park, Mechoso and others have all worked on im-

provements), and there are versions of WRF that include moist physics PBL schemes (perhaps these are not yet publicly available). The second part of the study demonstrates that strong subsidence can cause cloud clearing, but again this has been known and explored for two decades (e.g. Randall and Suarez 1984, JAS). What would be new is to understand how the mesoscale patches of strong subsidence originate from (e.g. gravity wave breaking, land-atmosphere interactions), but this wasn't explored. I provide some more comments on each of the parts below.

Part 1: The sensitivity to the PBL schemes available in the WRF is found to make a major difference to the simulations, but all three PBL schemes tested produce a PBL that is too shallow and poorly mixed. This is a well-known problem with large scale models in regions of marine stratocumulus (e.g. Bretherton et al. 2004, BAMS), and has even been examined across a whole suite of regional and global models in this very region (Wyant et al. 2010, ACP), which the authors seem to be unaware of. It is conjectured that some of the PBL height underestimation is associated with initial conditions (which are derived from a model that itself has a rather poor quality PBL scheme), while some clearly comes from the poor representation of mixing in the PBL schemes (none of which were designed with much consideration of important processes in the marine PBL). This conjecture could be, but wasn't, tested by comparing against a simulation initialized with a better analysis (e.g. ERA Interim), and so I was left wondering what new knowledge has been created from the first part of the study.

Part 2: The model clearings are clearly related to mesoscale subsidence patches and not to processes that cause POCs in the real atmosphere. So there seems to me to be little point in discussing POCs at all since the model features are completely unrelated. I wanted to know whether the subsidence patches are created internally in the model (e.g. from land-atmosphere interactions) or whether they are memory of the initial conditions, but this was not explored. The most interesting figure in the manuscript in my view (Fig. 12) shows intriguing NW/SE striations in the vertical velocity field, but the authors seem confused about what these are (gravity waves or not?). Looking more

ACPD
11, C13892–C13894, 2012

closely at their origin would be interesting.

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11, C13892–C13894, 2012

Interactive Comment