Atmos. Chem. Phys. Discuss., 9, C10127–C10131, 2010
www.atmos-chem-phys-discuss.net/9/C10127/2010/

Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Synoptically-induced variability in the microphysical properties of the South East Pacific stratocumulus deck" *by* D. Painemal and P. Zuidema

Anonymous Referee #2

Received and published: 26 January 2010

Review of "Synoptically-induced variability in the microphysical properties of the South East Pacific stratocumulus deck" by D. Painemal and P. Zuidema (ACP-2009-729)

General comments:

I liked this study because it addresses the important issue of how meteorological conditions differ between cases of high droplet/aerosol concentration and low droplet/aerosol concentraion (e.g., differences in cloud properties cannot solely be attributed to aerosol microphysical influence). It also described some unexpected relationships between cloudiness and meteorology, which illustrated the complexity of cloud processes and gave me things to think about.

This study does have a weakness, however, which is partial lack of focus. The paper is strongest when it is describing how cloud properties and meteorological processes differ between cases of maximum droplet concentration and minimum droplet concentration. It becomes weaker when it delves into the more general issue of the meteorological conditions associated with various macroscale cloud properties. For example, the fact that certain meteorological conditions are associated with enhanced cloud fraction when droplet number is maximum does not necessarily mean those same meteorological conditions are associated with enhanced cloud fraction in general. If the authors would like to explore how free-tropospheric wind affects 850 hPa temperature and cloud fraction, it seems better to drop the constraint of maximum and minimum droplet number. Although all of it is interesting, the study seems less integrated because it starts in one place and ends in another place. E.g., the abstract starts off with "synoptic variations in ... droplet number" and ends with "the synoptic impact on offshore cloud properties is arguably our most radiatively important finding". If that's the case, why not write a paper about synoptic impact on clouds rather than droplet number?

A second weakness is that the authors seemingly promise to investigate the aerosol influence on cloudiness but never get around to doing so (aside from showing a correlation between aerosol number and droplet number). They speculate about aerosol transport from remote regions but provide no observational support. They state that 850 hPa meridional flow is a useful meteorological variable to control for in examinations of cloud-aerosol interactions but do not carry out any such analysis. Why not do so?

I recommend that the authors bring more focus to the study by either addressing my first stated weakness (e.g., move away from droplet number and instead focus on general synoptic impacts on macroscale cloud properties) or addressing my second stated weakness (e.g., move away from the general synoptic impacts on macroscale cloud properties and devote more effort to distinguishing aerosol and dynamical influences on cloud).

Specific comments:

1) It would be useful if the authors briefly described how they derive CTH. I only saw one tangential reference to Zuidema et al. (2009). From my skimming of Zuidema et al. (2009), I gather that CTH is derived assuming constant lapse rate aside from a dependence on boundary layer height. What if lapse rate actually differs between MAX Nd and MIN Nd conditions? For example, perhaps MAX Nd MODIS-derived CTH appears shallower not only because the inversion base height is lower but also because the lapse rate is weaker (e.g., the boundary layer is less well-mixed)? Can the authors identify whether there is a difference in lapse rate? So far as I can tell from the Antofagasta sounding, there appears to be no difference in inversion base height between MAX Nd and MIN Nd (one cannot use the sounding to infer lapse rate because Antofagasta surface temperatures may not represent values for the open ocean).

2) Line 25 p. 25524 and Line 1 p. 25528: What does "primarily reflects synoptic changes" mean? Couldn't synoptic variations influence aerosol and Nd?

3) Line 16 p. 25534: What do "one-half" and "(two-thirds)" refer to?

4) Lines 15-16 of p. 25536: It is not at all apparent to me that Fig. 7a demonstrates that MAX Nd has a shallower boundary layer. The inversion base appears to have the same height for both composities.

5) Lines 21-23 of p. 25536: The authors argue that enhanced easterlies are associated with greater divergence for MAX Nd. Normally, one cannot calculate divergence with winds at only one location. Do they assume that wind is necessarily zero at some interior point due to the presence of the Andes? What about the meridional winds? Couldn't divergence of zonal wind be compensated by convergence of meridional wind such that there is no net or even an opposite effect?

6) Lines 13-14 of p. 25538: I don't see how there is an increase in 850 hPa geopotential

height between 75-85 W. Didn't the authors previously state (lines 11-12) that there was an anomalous trough. It's difficult to read the sign of the contour labels, but they appear negative to me.

7) Lines 23-25 of p. 25538: Perhaps there really is increased subsidence near the coast, but isn't Fig. 10 based on the reanalysis which is already seen to disagree somewhat with the Antofagasta sounding and greatly influenced by the representation of mountain topography in this region? Also, as mentioned in a previous comment, it's not obvious to me how a single sounding location can be used to quantify subsidence and thus "confirm" the reanalysis.

8) Lines 16-18 of p. 25542: "horizontal temperature advection reinforces vertical advection" needs more precision. For total (non-anomaly) advection, enhanced temperature is associated with weaker horizontal cold advection that offsets less of the subsidence warming. For anomalous advection, horizontal warm advection is associated with anomalous ascent and presumably anomalous vertical cold advection.

9) Line 9 of p. 25543: What does "in evidence" mean?

10) p. 25543: I'd be hesitant to attribute differences between MAX Nd and MIN Nd to differences in aerosol transport from remote locations (e.g., southern region of Chile) when no back-trajectories have been calculated. How do the authors know that air in the MAX Nd boundary layer was from coastal Chile whereas air in the MIN Nd boundary was not? Aren't the along-shore winds stronger in the MIN Nd case (e.g., stronger advection of aerosols from farther south)?

11) Lines 15-16 of p. 25544: "encourages free-tropospheric ascent offshore" needs more precision. Presumably there is not ascent in the total (non-anomaly) field. I prefer "weaker subsidence" to "anomalous ascent".

12) Lines 22-24 of p. 25544: "our ... analysis ... indicates that the free-tropospheric meridional wind at 85 W is dominant" strikes me as an exaggeration. I'd agree it is

important, but have the authors really demonstrated that it plays a greater role than anything else. The present study focuses more on Nd variations than comprehensively examines meteorological impacts on stratocumulus.

13) Lines 25-26 of p. 25544: Perhaps 850 hPa meridional winds are a useful meteorological variable to control for in terms of cloud-aerosol interactions, but this wasn't brought out in the present study.

14) Lines 1-3 of p. 25545: And how does anomalous warm advection above the inversion produce a decrease in cloud top height? Through strengthening the inversion and reducing entrainment?

15) Why not put the box shown in Fig. 2 in more of the plots? The reader would then not need to flip back and forth between figures.

16) Why are some figures color and some grayscale?

17) Why not show 850 hPa values instead of 700 hPa values in Fig. 11 so there would be greater comparability with Fig. 10?

18) What does the color shading in the Arica Bight correspond to? It is not described in the figure caption.

Technical comments:

1) Line 4 p. 25527: "principal" should be "principally"?

2) Line 1 p. 25532: "towards" means "for the purpose of"?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 25523, 2009.