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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

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REVIEWER 2

1. 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 me-

teorological 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?

This is a valuable comment that encouraged us to be more specific/focused in our writing. Our goal with the study is limited to the investigation of the composites rather than a general analysis of the synoptic variations of the Sc deck. In the latest manuscript, we have attempted to be more explicit and more clear on the limits to our endeavour.

2. 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?

Agreed. We modified the text to be more specific to what we actually did. We were reluctant to perform backtrajectories using the NCEP Reanalysis based on Fig. 6 (comparing NCEP to the Antofagasta radiosondes), yet wanted to keep the work's focus on the microphysical variability above the Arica Bight. Because of our lack of faith in NCEP properties at a particular point, we instead made a spatial correlation map (Fig. 10) which is arguably easier to critique. While this highlights the 850 hPa meridional flow, a sole synoptic analysis wasn't intended to be the focus of this study.

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3. 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).

See responses above.

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).

A full paper has already been dedicated to this cloud top height estimate, and it is summarized again within Painemal et al. (2010). The estimate compares well to aircraft radar-derived cloud tops in Rahn and Garreaud (2010). We are reluctant to once again provide another summary when the CTH derivation is already documented in the peer-reviewed literature. It's quite possible for the lapse rate to differ between MAX and MIN Nd conditions. Our lapse rate/cloud top height estimate is based on ∼150 open-ocean radiosondes and can only provide a mean lapse rate difference based on the mean difference in inversion base heights from those radiosondes. It may not capture every single situation accurately but is nevertheless a first-order improvement to using

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a constant lapse rate. Keep in mind also that a colder cloud top temperature for the same SST should correspond to a higher cloud top –the assumed lapse rate merely affects one's estimate of the magnitude of the cloud top height change, but not its sign.

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?

We mean primarily reflects synoptic changes of the cloud properties. The text was modified accordingly.

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

We modify the text: "About one-half/two-thirds of the MIN/MAX Nd days occur in groups of three or more days."

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.

We modified the text to only refer to the strengthening of the inversion.

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?

We removed these references. It seems fair to assume a drier and warmer free troposphere probably reflects increased subsidence, and left it at that.

6) Lines 13-14 of p. 25538: I don't see how there is an increase in 850 hPa geopotential height between 75-85W. 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

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negative to me.

We modify the text: “Decreases in the 850 hPa. . .”

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.

The reanalysis does not compare well to the Antofagasta soundings, but one aspect they did agree on was a warmer and drier free troposphere for the MAX Nd composite. In response to this comment, we also evaluated additional radiosondes at Iquique and arrived at the same finding.

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.

Since the sentence refers to values along the coast, where the reanalysis are less reliable, we decided to leave out this conclusion.

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

We rewrote this. The basic idea is that multi-day episodes of enhanced stability are likely to keep aerosol within the boundary layer.

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

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boundary was not? Aren't the along-shore winds stronger in the MIN Nd case (e.g., stronger advection of aerosols from farther south)? We merely meant to distinguish aerosol transported within the boundary layer from aerosol entrained into the boundary layer from the free troposphere. The latter seems less likely if the inversion is strong, favoring boundary-layer transport. We rewrote to make this clearer.

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". Okay

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.

True. We could stand to do more work, for example through compositing on the 850 hPa meridional wind at 85W, before making that statement. We have rewritten this.

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.

We agree, the sentence has been removed.

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?

That would seem reasonable.

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.

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Added, thanks

16) Why are some figures color and some grayscale?

We replace some grayscale figures by color ones.

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

Our new figures shows anomalies fields only at 850 hPa.

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

Indicates the location of the Nd plume, it is now included in the caption, thanks

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

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

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

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