Atmos. Chem. Phys. Discuss., 9, C12199–C12203, 2010
www.atmos-chem-phys-discuss.net/9/C12199/2010/

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

D. Painemal and P. Zuidema

dpainemal@rsmas.miami.edu

Received and published: 20 April 2010

REVIEWER 3

One aspect of the paper linked the synoptic scale meteorology to changes in Nd which was used as a proxy for the coastal aerosol loading. However the discussion on the transport of aerosol from anthropogenic sources in these synoptic situations was speculative and could be strengthened with further model or observational evidence. The other aspect of the paper discussed differences in the macrophysics of the stratocumulus in both this coastal and in more remote regions for the two composite cases. The authors conclude that "the free tropospheric meridional wind at 85W is dominant in es-

tablishing the southeast Pacific stratocumulus cloud coverage at synoptic timescales" and that it is "arguably our most radiatively important finding". It would therefore have been of interest to do the composite analysis based on this synoptic control rather than coastal Nd. Both aspects are interesting in their own right. However it would be of interest to explore the above points in a bit more detail in order to draw firmer conclusions. I would also suggest that the authors think about changing the title of the paper as it is gives the reader the impression that it focuses solely on synoptic influences on cloud microphysics.

These are good points. We agree with the reviewer that in the original manuscript was not clear the main objectives and results (see also reviewer 2's comments and our reply to them). We have attempted to be more specific in our revised manuscript. We have also modified the title of the paper.

Specific comments 1) Line 18 p. 25525: "Current IPCC estimates. . .." I think that this sentence is somewhat misleading as the latest IPCC report gives a median aerosol cloud albedo effect plus direct radiative forcing of -1.3 Wm-2 with a -0.5 to -2.2 Wm-2 90% confidence range (Climate Change 2007: Working Group I: The Physical Science Basis, section 2.92). This range spans the estimate from the Murphy et al. (2009) paper. A reference to the IPCC report is also required. True. The Murphy confidence range of -0.7 to -1.5 W m-2 doesn't extend as much as the IPCC range, with the Murphy study including all aerosol effects while the IPCC report only considers the direct aerosol+cloud albedo effect.

We have clarified.

2) Line 17 p. 25527: "We use NCEP/NCAR reanalysis. . ..and assume it is accurate except near the coast". Can you justify this assumption? The reanalysis certainly looks poor _ 500 km away from the coast as shown in figure 7 where it is unable to represent the strong temperature inversion at the top of the marine boundary layer. What do the reanalysis profiles look like at Antofagasta – do they reproduce the same features as

shown in the observations in fig 7? Are you able to compare the reanalysis against sounding data over the oceanic regions from the ship/aircraft during the VOCALS-Rex period? It is not clear to the reader at present that the reanalysis data is realistic. This is important as it is used extensively in section 4.

We have rewritten this to be clearer, including citing previous studies that have used the NCEP Reanalysis for this part of the world.

3) Line 10 p. 25529: The satellite derived cloud properties and TOA radiation are measured at _10:30 LT but the QuickSCAT winds are measured at 18:00 LT. Munoz (2008) however show a diurnal cycle in the QuickSCAT winds in this region, maximising in an area between 20-30S and from the coast to 75W. Do you expect the time difference between the observed winds and clouds/radiation to impact the results presented?

That is correct. By focusing on the MAX-MIN differences between different days, this diurnal difference should be less pronounced.

4) Line 5 p. 25531: "We attempt to minimize. . ..for overcast (or nearly so) pixels." What is meant by "or nearly so"? Do you place some limits on cloud fraction?

Since "or nearly so' phrase can be confusing, we removed it from the manuscript.

5) Line 16 p. 25533: This paragraph highlights specific research flights which correspond to high and low Nd cases from two of the aircraft that participated in VOCALSREx. These specific flights however are not particularly relevant to the paper. Bearing in mind that there were additional measurement platforms that took part in VOCALS REx (five research aircraft for example) it might be more instructive to simply state all high and low Nd days that occurred during the VOCALS-REx period.

We included a time series of Nd and LWP that is specific to vocals-rex instead, the new Fig. 13.

6) Line 6 p. 25534: "Near Antofagasta (23.43S; small black square)" This square needs to be added to figure 3. Done, thanks

7) Line 10 p. 25534: The time series data for winds, cloud top height and geopotential height are taken from different spatial regions. A bit more information is required as to why these regions are chosen. For example I would envisage that it is the coastal winds that are important for the transport of anthropogenic aerosol and would therefore contribute significantly to the variability in Nd. However the wind data is averaged over a much larger area that includes clean maritime air masses.

We follow the reviewer's advice. In our latest manuscript we choose a more coastally-confined area to calculate the mean winds. We write now: "Mean surface wind speeds (20S-30S, 70W-90W), mean cloud top heights alongshore (20S-30S, 70W-80W), and mean offshore 500 hPa geopotential height (15S-35S, 70W-100W, approximately the location of the climatological anticyclone) are shown along with the occurrence of MAX (dark triangles) and MIN (open triangles) Nd days. The regions used to calculate these mean values tend to better reflect the differences between MAX and MIN cases."

8) Line 17 p. 25534: "MAX (MIN) Nd days co-occur with weaker (stronger) coastal winds" yet as mentioned above the wind data is averaged over an area that extends further out into the Pacific (70-100W) and may not be representative of the coastal wind.

See our response above.

9) Line 14 p. 25536: The height of the temperature inversion (fig 7a) looks the same for both the MAX and MIN Nd cases whereas the text states that the radiosonde data show that the "MAX Nd cases are typically associated with a shallower boundary layer".

That is correct, both profiles show a similar inversion base height. We modify the text to only mention the inversion strength.

10) Line 20 p. 25536: It is inferred that there is increased subsidence in the MAX Nd case from the increase in the observed easterly wind above the inversion as shown in figure 7. A bit more explanation as to how the subsidence is calculated from these

soundings is required. Further Munoz (2005) show a large diurnal cycle in the winds above the boundary layer whereas figure 7 is for 12 UTC only. If observations from a different time were chosen would it impact your discussion? We removed this sentence. We only have the 12 UTC soundings, and agree with the reviewer that the diurnal variability in the winds could be quite significant.

11) Line 1 p. 25543: As mentioned later in the text there are anthropogenic sources above the marine boundary layer along the coast. With enhanced easterlies above the inversion you might expect aerosols from these sources to be advected out above the Sc, particularly if enhanced subsidence has lowered the cloud top height. This aerosol would presumably be entrained into the cloud layer downwind on some timescale. Back trajectory analysis would aid the discussion of potential sources for high Nd days. As the paper stands at present I am not convinced that the discussion on aerosol transport in this section (with no observational or model evidence) answers the question raised in the introduction – "Do atmospheric conditions favour aerosol incorporation into the cloud through free-tropospheric subsidence, or from within the boundary layer?"

We can say that stronger cloudtop temperature inversions are less likely to entrain free tropospheric aerosol, and more likely to keep BL aerosol confined. We limited our statements to this.

12) At several points in the text it is stated that "Along 85W". Should this be "At 85W"?

Done, thanks.

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