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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

D. Painemal and P. Zuidema

dpainemal@rsmas.miami.edu

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REVIEWER 1

This reviewer made the general comment that the paper was difficult to read in places. We took the opportunity of the review process to reassess our writing and also our organization. One change we made was to place the error analysis and satellite/ship comparisons in a separate appendix, towards improving the flow of the writing.

1. I have just one small criticism, namely that the classification proposed here is taken as granted and not discussed anymore. Composite studies rely on a priori, arbitrarily defined, classification. Here, the terciles of the droplet number concentration in the

Arica Bight. I am not saying that this choice is not appropriate. The paper demonstrates the contrary, with interesting differences between MAX and MIN Nd. However, the reader would like to know what the authors think of their choice, if they have tested other classifications, if they think a different one would bring more insights, and which one? I am not suggesting a long section, but just a short paragraph in the last Section, for the authors to share their experience at the end of this study.

We chose the composite approach to avoid problems with assessing Lagrangian back-trajectories based on the NCEP Reanalysis near coastlines. It then seemed reasonable to choose an area with high variability in the satellite-derived cloud droplet number concentrations, in part because, although we were initially unclear on the validity of the satellite Nd retrieval, a region with high Nd variability was less likely to be a retrieval artifact. EOFs are another approach, but we thought the physical significance of satellite-derived composite differences might be easier to interpret. In addition, the satellite data gave us an opportunity to assess/push the satellite data more through the comparisons to the ship-based data, which we felt would represent a unique contribution of the study.

2. Fig. 1a: Use different symbols because it is difficult to discriminate black and grey dots. Done, thanks

3. Fig 1-b: even the line represents a mathematical best fit, I don't like it because it illustrates a non-physical relationship between aerosol concentration and MODIS derived droplet concentration: droplet concentration greater than aerosol concentration at low values. Considering the dispersion of the data points, the best fir anyway has no value. If a line is to be plotted, I would rather start at the coordinates [30;10] and finish at [1000;1000]. The best would be the plot functions currently used to parameterize that relationship, starting at about 30 for aerosol concentration and then saturating at 200 or 300 for the droplet concentration (see the parameterizations of Ghan or Pinty).

This comment did cause us to examine our best-fit line again, and we realized we

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shouldn't be using a standard least-squares fit, which assumes the "x"data is known perfectly. We redid the fit to take errors/variability in both Na and Nd into account. This did improve the fit. We further note that it actually is possible for Nd>Na when Na>0.1 micron; it may simply mean that smaller aerosol are being activated (how likely this is in our dataset, I don't know).

I can understand the reviewer's discomfort with a best-fit line drawn through noisy data. The main reason to do it is that it is already being done, and being used to assess climate models (e.g., Quaas et al., 2009). Additional surface/satellite assessments can only help such efforts, and in this case, we make use of a ship-based dataset, unusual because most surface-based measurements are made on land.

4. Fig. 4. Difficult to discriminate the symbols. Please use the same as in Fig. 5 with open circles and black triangles. Done, thanks

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/9/C12189/2010/acpd-9-C12189-2010-supplement.pdf

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