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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The first aerosol indirect effect quantified through airborne remote sensing during VOCALS-REx" *by* D. Painemal and P. Zuidema

Anonymous Referee #1

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The paper attempts to calculate albedo relative susceptibility and the first indirect effect in pristine marine stratocumulus clouds using a variety of data from a field campaign. Combining and making sense of retrievals from field campaign measurements is a difficult task, so it is not suprising that the authors encounter a few bumps along the road. The biggest problem seems to be that a large fraction of the CDNCs derived from LWP and tau retrievals (both from aircraft) are unrealistically high. If I understand correctly, they attribute the high values to cloud inhomogeneities (in-situ measurements of LWP and tau –not described in detail– exhibit a narrower range according to Fig. 6a). The authors try to circumvent the high CDNC problem by presenting the analysis in terms of CDNC thresholds. While the effects of smoothing the time series of LWP

is presented in a different context, no such smoothed values are used to derive CDNC itself. I was surprised that eq. (8) itself was not discussed more as a root cause of the discrepancy, but I suspect it is because once it is accepted at face value any universal changes in k and lapse rate can only shift the CDNC histogram. Overall, I have mixed feelings about this paper. It is a courageous analysis, but in the end I'm not convinced about the robustness of the results even though apparently they agree with a prior satellite-based analysis.

Specific comments/suggested corrections p. 25442, line 25: "ideal" instead of "robust".

p. 25444, line 4: Incorrectly, the difference between the two ACI indices is shown.

p. 25444, line 15: A subscript "R" is missing and "Oreopoulos" is misspelled.

p. 25445, lines 20-21: "Such an analysis is also performed here using almost 100 daytime aircraft vertical profiles that were collected during VOCALS-REx." Do those 100 profiles correspond to the 4 flights mentioned in p. 25446, line 11?

p. 25446, line 17: How were the Fig. 1 maps derived? Not using eq. (1) I gather which is expressed in terms of LWP which is not directly available from MODIS, but from eq. (8)? Given the results shown in Fig. 6b, shouldn't the authors use a much higher upper limit for their colorbar? Is the LWP shown in this figure the LWP provided in the MODIS products (assuming vertically homogeneous LWC) or your own from tau and re assuming linear increase of LWC with height?

p. 25451, line 4: The 9/5 factor seems to be inconsistent with the vertically homogeneity assumption (line 24 of p. 25450 and line 12 in p. 25451) which should give a factor 3/2. Also, it should be mentioned somewhere in this subsection that this type of iteration algorithm only works because of the weak dependence of transmitted flux on re (g and SSA effects that partially cancel out).

p. 25451, lines 12-14: Mention also uncertainty in WVP.

Section 2.3.3: What about the sensitivity to initial (guess) values of tau, re (incl. the

Snider CDNC formula)?

Fig. 6a: How exactly was tau determined from the in-situ (cloud probe) measurements?

Fig. 6 caption: Perhaps add that the remotely-sensed CDNC comes from eq. (8).

p. 25453, lines 12-13: I'm not sure it is fair to state that the two histograms in Fig. 6b arein qualitative agreement. They are quite different, only the location of the mode agrees! In p. 25462, line 12 this claim of agreement is repeated, but at least there the tail values of the remotely sensed CDNC are characterized as unrealistic. Such high values are indeed highly unlikley to occur in marine clouds within a pristine environment.

p. 25453, line 16: "overestimates, or both" instead of "underestimates".

p. 25454, lines 2-4: Remove "These values are approximately similar to those reported by Cahalan et al. (1994) based on 18 days of microwave LWP data from Californian stratus." Can you really compare 11s variability with 18-day variability?

p. 25454, line 9: "For the observations collected near-nadir". Not sure what you mean. Cloud optical depth was calculated from hemispheric flux measurements by the pyranometer underneath the cloud deck.

Caption of Fig. 7: Add that these are composite histograms from four flights.

p. 25546, lines 6-7: "The removal of samples with Nd larger than 1100cm-3 mainly affected the smaller LWPs, because large Nd mostly occurred in those LWP bins." I understand that this comes from eq. (8) under a fixed tau, but still seems counterintuitive since tau and LWP should co-vary to some extent. Stating that the smaller the LWP, the more droplets one should expect to encounter sounds at first unphysical.

p. 25458, lines 17-19: "Both satellite and VOCALS susceptibilities were in qualitative agreement with the two-stream SR (Twomey and Platnick, 1994) which increased with SR until reaching a maximum for tau of 13.33." First of all, the reference is Platnick and

Twomey 1994. Second, I don't think this comparison can be made since in that paper no relative susceptibility calculations were made, only absolute susceptibility.

p. 25458, line 23: Change 0.76-0.78 to 0.076-0.078.

p. 24561, lines 15-23. I'm quite confused by this discussion which seems to involve updraft velocity, subsidence, horizontal wind speeds and coastal jets. I understand the role of (presumably subgrid) vertical velocity, but how is it influenced by the other factors?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 25441, 2012.