Responses to Reviewer 1

We appreciate the reviewer's comments and his/her careful reading of our paper. Their comments definitely helped us improve the quality of the manuscript. We address the reviewer's questions/comments below:

1. "…. 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."

- The reviewer correctly states that any change in the constant parameters in equation 8 would produce a small shift of the histogram. While equation (8) has its uncertainties, these cannot explain why N_d is sometimes larger than 800 cm^{-3}. Regarding the histogram (Figure 6b), we noticed that, contrary to the rest of the analysis, the histogram included all the samples, irrespective of LWP or τ. For consistency, in the revised manuscript we only showed the N_d histogram for samples with LWP >20 gm^{-2} and τ>2. In addition, we found that the remote sensing N_d was dependent on the solar zenith angle (SZA). When removed samples with SZA>35, we observed a significant reduction in the number of samples with unrealistically large N_d (Figure 6b, black lines) and the histogram better agreed with the in-situ one. Although unfortunately we do not count on the appropriate dataset to further investigate this dependence, this might reflect the sensitivity of the retrievals to 3D radiative transfer effects, or a plausible dependence of the pyranometer performance on SZA. In our latest Figure 6, we also included N_d calculated from the 15-s averaged LWP (Figure 6b, blue line). We only highlighted the sensitivity of the analysis to SZA, and limited our investigation to samples with SZA<35˚.

- Despite the better agreement of our retrievals with their in-situ counterparts, we still consider valuable the analysis of the dependence of ACI_τ on N_d (figure 10 and 11). Nevertheless, given the difficulty of defining an appropriate N_d threshold, this time we mostly emphasized those results that are less sensitive to N_d (i.e. LWP larger than 60 gm^{-2}). What is interesting about our findings is that they agree with in-situ observations and a recent modeling study over the same region (Yang et al, 2012, ACP) in the sense that ACI_τ is close to the upper physical limit (see our response to comment #5). Moreover, the similarity between satellite estimates and our results suggest the physical robustness in our results.

- Regarding the estimate of in-situ LWP and τ, their calculation is described in detail in

Painemal and Zuidema (2011,JGR). This time we added a brief description:
"Aircraft-based LWP and τ were calculated by vertically integrating the profile liquid water content and the extinction coefficient, respectively. The extinction coefficient was derived from the second moment of the droplet spectra and assuming a constant extinction efficiency of 2 (Painemal and Zuidema, 2011)."

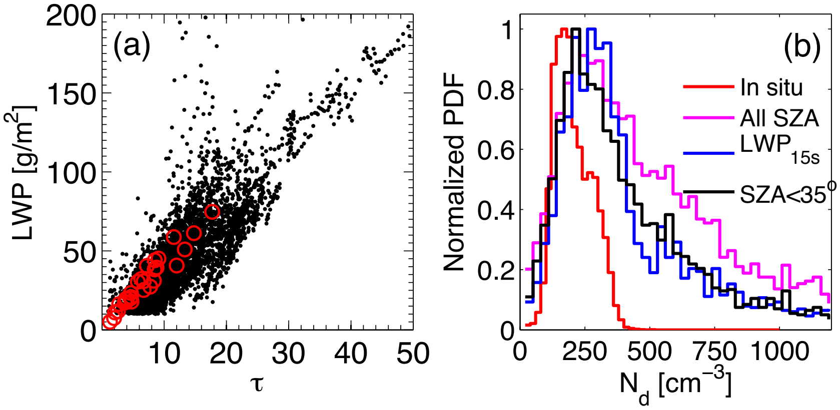

Figure 6: a) LWP versus τ. Black dots represent the remote sensed values, and the red circles are the values derived from the cloud probes. b) Normalized probability density function (PDF) for the remote sensing-based N_d (magenta, black, and blue lines, eq. (8)) and the in-situ N_d obtained during in-cloud legs (red line). The remote sensing-based N_d was estimated from samples with LWP>20 gm^{-2} (magenta line) and solar zenith angle smaller than 35° (black line). Blue line corresponds to N_d calculated with the 15-s averaged LWP. Remote sensing-based N_d retrievals were calculated for τ>2, LWP>20, overcast at 1.4 km scale, and column-maximum reflectivity <-17 dBZ.

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

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

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

5. 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?
We appreciate the reviewer for his/her meticulous comment about the in-situ calculation. In our first manuscript, we included all the measurements collected by the aircraft C-130 during VOCALS- REx. This time, we also highlighted those measurements obtained during the four research flights. This additional analysis is important as it shows that the in-situ ACI_τ estimated from the four research flights is larger than that calculated with all the research flights (0.92 versus 0.76). This increase is consistent with more coupled conditions and less precipitation in these four research flights, relative to other flights that presented more disturbed boundary layers. Moreover, this new result supports the idea that ACI_τ is indeed very large over the southeast Pacific.

6. 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?
We estimated N_d and LWP from the MODIS cloud effective radius and optical thickness, assuming an adiabatic-like behavior of the water content, a linear increase of the effective radius with height, and a vertically constant number of droplets. These calculations are described in our paper Painemal and Zuidema (2011, JGR). We added this information in our revised manuscript.

7. 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).
The reviewer correctly states that the factor in P25451 should be 3/2. We modified the text accordingly. The weak dependence of the transmitted flux on r_e was previously mentioned in p25448:
"The pyranometer-based τ retrieval relied on the strong dependence of τ on the incoming solar radiation and the solar zenith angle, with secondary dependences on r_e and the atmospheric composition (Leontyeva and Stamnes, 1994)"

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

9. Section 2.3.3: What about the sensitivity to initial (guess) values of tau, re (incl. the Snider CDNC formula)?
Although the algorithm is insensitive to the initial r_{e*} and τ^*, appropriate values can considerably reduce the number of iterations. This is the reason why we used the Snider formula instead of a fixed initial guess for r_e and τ. We added this information in section 2.3.3.

10. Fig. 6a: How exactly was tau determined from the in-situ (cloud probe) measurements?
Please see our response to comment #1

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

12. p. 25453, lines 12-13: I'm not sure it is fair to state that the two histograms in Fig. 6b are in 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.
See our response to comment #1

13. p. 25453, line 16: “overestimates, or both” instead of “underestimates”.
We slightly modified that sentence to:

“This could imply a LWP underestimate or a cloud optical depth overestimate.”

14. 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?

We remind the reviewer that the aircraft sampled a horizontal transect of ~ 100 m every second, whereas the surface-based microwave radiometer during FIRE only sampled a ~ 5 m horizontal transect every second. We did quantify the comparison further, however, and modified the sentence to:

“In addition, the standard deviation of the logarithm of LWP distribution for the combined four flights is equal to 0.22, lower than the value of 0.39 found by Cahalan et al. (1994) based on 18 days of microwave LWP data from Californian stratus. The lower value reported here reflects the larger dataset examined within Cahalan et al. (1994), equivalent to ~ 8500 km transect, or a ~15-fold increase, as well as the greater frequency of lower-LWP clouds in our dataset.”

15. 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.
The sentence was modified to:
“At low zenith angles when the Sun is closer to overhead, downward photon diffusion can increase the irradiance measured below a cloud relative to a plane-parallel calculation, leading to an underestimate in the cloud optical depth (e.g., Zuidema and Evans, 1998).”

16. Caption of Fig. 7: Add that these are composite histograms from four flights.
Done, thanks.

17. 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.
We referred in the manuscript to those samples with high spatial (temporal) in LWP, in which the smoothed τ (due to the instrument large field of view) cannot follow the fast LWP transition, especially when LWP decreases rapidly. Moreover, potential artificial

increases of N_d for small LWP might be related to the fact that uncertainties in τ are larger (in percentage) for smaller τ (which occur for small LWP).

18. 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.

Platnick and Twomey (1994) introduced the two-stream absolute susceptibility $S_0 = \frac{A(1-A)}{3N_d}$, which is equivalent to a two-stream relative susceptibility of $S_{R0} = \frac{A(1-A)}{3}$. We clarified this in our latest manuscript

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

Done, thanks.

20. 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?

As part of our revisions we evaluated the dependence of LWP on vertical velocity explicitly, and found a weak positive correlation consistent with enhanced aerosol activation for the higher-LWP clouds. We removed this original sentence from the manuscript and substituted:

"A weak positive LWP-vertical velocity relationship was found in this dataset (r=0.11), consistent with increased aerosol activation. The results are also consistent with those from an adiabatic parcel model (McComiskey and Feingold, 2012)."