Changes in the shape of cloud ice water content vertical structure due to aerosol variations,

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Reviewer #1

This study addresses the question of effect of aerosols on the invigoration of deep tropical clouds. They regard an enhancement of the ice water content (IWC) as an indicator for the invigoration. While invigoration can certainly increase the IWC, the IWC was shown to be potentially enhanced also by microphysical effects of the added aerosols. Fan et al. (2013) showed that larger numbers of smaller ice particles fall more slowly and therefore enhance the IWC. This has to be added to introduction.

The revised paper now discusses Fan et al (2013) in the Introduction in a new paragraph. The paper by Morrison and Grabowski (2011) is also discussed in the new paragraph added to the revised paper.

Major comments:

1. The main response parameters are poorly defined. Please define clearly IWCsum, IWCreg and IWCshape with equations and allocate for that explanation a figure with illustration. Please state the units.

The revised paper has equations 1-3 which define the IWCsum, IWCreg and IWCshape profiles. Newly added Figure 4 of the revised paper summarizes the processing steps. The units are stated for each set of profiles.

2. The authors conclude that cloud adjacency does not affect much their conclusions. The data that they show to support that is not very convincing. The obvious way to show this is to repeat the final results for the various adjacency thresholds, and see the extent that it affects these results. Sample size should not be an issue for the final results shown in Figures 8 and 9.

The revised paper now includes a new figure (Figure 8) which presents means from the previous Figures 8 and 9, as a function of the cloud pixel-distance value. New Figure 8 indicates that cloud-adjacency has a minor effect on the previous Figures 8 and 9 mean values.

3. It is not clear to me how Figure 1 was constructed. It appears that more profiles were averaged towards the larger numbers on the abscissa, because they converge towards ordinate value of zero. How was that done? Were the profiles binned and averaged in some way?

Assuming that the question is in regard to the construction of Figure 5 of the original paper (Figure 7 in revised paper), the x axis value is the number of profiles that is associated with a specific region and season that is associated with the corresponding derivative on the y axis. When Figure7 is first discussed in the revised paper, we add the following sentence to clarify the Figure's construction: "As explained in Section 3 (Step 4 processing), the value of the IWC_{reg} derivative for a 2 km altitude bin is the average of two derivatives, based upon IWC_{reg} values at the first and second, and first and third, aerosol bins".

4. The authors state that "three AOD bins (i.e. 0.05 - 0.15, 0.15-0.35, 0.35 – 0.45), were chosen to represent low, medium, and high amounts of AODs". By doing this the authors ignored the two main properties of aerosol effects on cloud invigoration, as described in two papers in Science: (Koren et al., 2008; Rosenfeld et al., 2008). First, the cloud invigoration responds to the logarithm of the aerosols concentrations. Second, the effect saturates at AOD of 0.25 to 0.3, and may reverse at larger AOD. This is evident in Figure 3, where there is large difference between the lines of AOD 1 and 2, but bins 2 and 3 are practically the same. The authors have to expand the introduction to include the discussion of the aerosols effect, as mentioned here. Furthermore, the analysis has to be redone with re-binning accordingly. The cases with AOD<0.05 are the cleanest and thus expected to have the greatest contrast to the polluted cases. Based on the principle of the logarithmic effect, the difference between AOD<0.05 and AOD of 0.05 to 0.15 should be larger than what the authors found between bins 1 and 2 in the present version,. Why did the authors exclude AOD<0.05? This should be a bin on its own, which I expect to be the most informative. In summary, for the paper to be considered for publication in ACP it has to undergo a major revision. The background has to be rewritten with a more physical basis, the methodology has to be clarified, the analyses have to be completely redone with new binning, the effects of adjacency effect have to be tested on the final results, and the discussion of the results has to commensurate with the newly written physical background.

a) The 0.05 is a typo introduced into the original paper (we apologize for this). The aerosol bins used in the calculations of the original paper, and reported in the figures of the original paper, are 0.01 - 0.15, 0.15-0.30, 0.30 – 0.45. Figure 3 of the original paper has the correct lower bound (0.01) of the first AOD bin range. These three sets of AODs are based upon an examination of probability distribution functions of MODIS AODs such that there are approximately an equal number of AODs for the three aerosol bin ranges. We tried to have the statistics of the three AOD bins to be similar. Since the expected errors (see page 9, lines 5-6, of the revised paper) for MODIS C6 AODs over the Ocean are -0.02 (-10%) and +0.04 (+10%) and over Land by ± (0.05 +15%), a separate bin range (i.e. 0.01 – 0.05) would apply MODIS data of low accuracy.
b) Table 1 was added to the revised paper to state the AOD, AAOD, and CO bins clearly. Reviewer 2 suggested that OMI AAODs and MLS CO data be added to the study, and we did this in the revised paper.
c) A new paragraph is added to the Introduction of the revised paper which discusses Koren et al. (2008) and Rosenfeld et al. (2008) in regard to *saturation* effects. *Inhibition* effects are also discussed in a new paragraph,

with reference to papers by Ramanathan et al. (2005, 2007) who modeled and observed the stabilizing effects of absorptive aerosol offshore of India. These four papers form the basis of the added written physical background.

d) Equations 1-3 clarify the methodology (i.e. how the IWCsum, IWCreg and IWCshape values are calculated).
e) The reviewers' requested binning (i.e. lower bound is less than 0.05 for the first bin) is actually the original binning.
f) New Figure 8 illustrates the effects of cloud adjacency on the final results (original Figures 8 and 9, Figures 11 and 12 in revised paper). Curves of the means for the All AOD, 2, and 4 pixel-distance AOD fields, are presented in Figure 8 as requested by the reviewer. This Figure is placed in the text when Table 2 (previous Table 1 listing of the pdf means) is first discussed. It is apparent from Figure 8 in the revised paper that the cloud adjacency issue did not have a large impact upon our particular calculations.
g) Additional text has been added in the Introduction and Discussion in regard to the added written physical background. Table 4 has been added to the text which indicates the percent of MODIS observations for which invigoration and inhibition scenarios are apparent. The saturation scenario in the MODIS data is present twice as often as the inhibition scenario.

I did not comment on minor issues, because they likely will not survive the revision, if done as I expect it to be.

References:

Fan J., L. R. Leung, D. Rosenfeld, Q. Chen, Z. Li, J. Zhang, H. Yan, 2013: Microphysical effects determine macrophysical response for aerosol impacts on deep convective clouds. Proceedings of the National Academy of Sciences, 110(48), E4581-E4590.

Koren, I., Martins, J. V., Remer, L. A., & Afargan, H. (2008). Smoke invigoration versus inhibition of clouds over the Amazon. science, 321(5891), 946-949.

Rosenfeld, D., Lohmann, U., Raga, G. B., O'Dowd, C. D., Kulmala, M., Fuzzi, S., ... & Andreae, M. O. (2008). Flood or drought: how do aerosols affect precipitation?. science, 321(5894), 1309-1313.

All of these papers are included in the revised paper.

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