Atmos. Chem. Phys. Discuss., 11, C12097–C12115, 2011
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Atmospheric Chemistry and Physics Discussions

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11, C12097–C12115, 2011

Interactive Comment

Interactive comment on "Cloud invigoration and suppression by aerosols over the tropical region based on satellite observations" *by* F. Niu and Z. Li

F. Niu and Z. Li

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Interactive comment on "Cloud invigoration and suppression by aerosols over the tropical region based on satellite observations" by F. Niu and Z. Li Anonymous Referee #1 Received and published: 11 February 2011

Niu and Li present a study of the statistical correlations of cloud top temperatures and cloud ice water paths, as well as precipitation intensities, with aerosol index over ocean and aerosol optical depth over land. Thanks to the wealth of satellite data from the A-Train constellation, such statistical correlation studies have become very popular in the recent literature. The goal is to gain insights in the role of aerosols in cloud and precipitation developments.

The study by Niu and Li is interesting since it separates the correlations by cloud-base and cloud-top temperature to obtain opposite signs. A rudimentary exploration of the large-scale meteorological context is also provided. It thus contributes to knowledge about how aerosols and clouds/precipitation might be related. The study is certainly pertinent to Atmos. Chem. Phys, and it is written in a very good English, and has a good choice of figures.

So in my opinion, the study is a very interesting one. However, I suggest the authors re-write the manuscript to choose a more scientific language. The statistical correlations Niu and Li find are interesting enough, and there is no need to describe these as cause-effect relationships which this statistical method cannot establish. So a re-writing in a way that just the results are presented, and not over-interpreted, would in my opinion strengthen, not weaken the study. To put it polemically, one frequently reads correlation studies where, if aerosol optical depth is positively correlated with cloud cover, a "cloud lifetime effect", and in the opposite case, a "semi-direct effect" is postulated. Similarly, if AOD is positively correlated with cloud-top temperature, a "drizzle effect", and otherwise, an "invigoration effect" is postulated. Now I accept that Niu and Li put forward arguments which may corroborate this claim. Still, in my opinion, it is sufficient, and of better scientific style, to choose a more careful language. I provide a suggestion for a revised title and abstract at the end of this review. A revision of the text in a similar way would be needed for the rest of the manuscript.

We are pleased that the reviewer accepts our observational findings, and agree that we may "over-interpret" the results in terms of their physical processes just based on the findings of this study. However, this is a companion investigation that complements another more insightful study by taking advantage of much richer information from 10 years of ARM data as published just recently in:

Li, Z., F. Niu, J. Fan, Y. Liu, and D. Rosenfeld, Y. Ding (2011a), The long-term impacts of aerosols on the vertical development of clouds and precipitation, Nature-Geoscience, doi: 10.1038/NGEO1313.

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The ground-based study allows us to tackle the causal issues much more comprehensively. Having stated this, we honor the reviewer's suggestion to soften our interpretation. Extensive changes are made throughout the paper from the title to the conclusion. The coincidental findings from totally different observations from ground and space provide at least indirect evidences to back our reasoning arguments pointing to the two dominant aerosol effects beyond a reasonable doubt. Very careful language is used regardless.

Other major comments The effect of wet scavenging of aerosols is not discussed enough. Why is there no negative correlation between AI/AOT and precipitation (at least for "mixed-phase")? AI and AOT are retrieved only where there are neither clouds nor precipitation. Does this play a role? More discussions on the effect of wet scavenging of aerosols are provided. The invigoration effect of mixed-phase may offset the scavenging effect of aerosols, leading to no obvious negative correlation between AI/AOT and precipitation. Viewing this mater from a different perspective, one would argue that the very fact that the opposite correlations found for two different types of clouds itself can negate the scavenging effect that would behave as ubiquitous negative correlation between rain rate and aerosol loading.

The assessment of the meteorological context is rudimentary. Most important is for both cloud formation and aerosol swelling/ increased AOT in the vicinity of clouds is the relative humidity and its small-scale fluctuations. At least a discussion of this would be necessary.

We agree the assessment is rudimentary. Unraveling the meteorological influences from observational data is always a daunting task, especially for satellite-based studies due to the inherent limitations of satellite observations. With the multi-sensor products, we are at least able to provide more constraints to single out the effect more clearly than many previous studies, but it is hard to assess meteorological influences. A discussion of the reason why we use LTSS and CWV is provided. Again, we refer to our ground-based study using 10 years of extensive ARM measurements in which various effects

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were investigated thoroughly. The fundamental findings are consistent. At the two papers were submitted about the same time, we did not refer to that study then. Now, we refer to it as an indirect but much more compelling support.

Specific comments p5006 l5: It would be good to recall the reader that at daily resolution, only a "curtain" of Cloudsat data are available within each 1ox1o grid-box This is added to the text.

l6: Choosing the Tropical region does not ensure that the dominant cloud type is convective. Also in combination with the other criteria (in particular, cloud base temperature), this is not assured, and given that a typical LTSS is > 20 K (Fig. 4d), this would most certainly be stratiform clouds. Indeed, we cannot guarantee all clouds are convective, or even if they are dominant, but it is safe to say they are more dominant than those occurring at high latitudes in general. Per the principle of the reviewer's major comment, we soften our argument about the role of convection.

l10: Specification is needed on how these temperatures are computed. Are they computed for the CloudSat cloud base heights as obtained at original resolution? Or rather, after averaging the curtain over 1o? If the latter, the temperature criteria would not necessarily define the temperature very well. It would be useful to check the claim using the CALIPSO or MODIS cloud-top phase retrieval. The temperatures are computed at original resolution and then averaged within each 1ox1o grid-box.

l15: How are the bins chosen? AOT over land is binned with equal intervals. AI is binned with equal intervals of log(AI). Therefore, on a logarithmic scale in Fig. 1A and 1B, the x-axis shows equal distance between each point.

p5007 l3: The latent heat release by freezing is a main assumption if a cause-effect relationship shall be proven. Is there a way to use the re-analyses or CALIPSO to assess this? Using current observations to assess the latent heat released in cloud development is really challenging. A major reason lies in the lack of continuous high-resolution observations of individual cloud development, especially changes in cloud

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phases. Most satellite observations, including CALIPSO, are snapshots of different development stages of different clouds. Therefore, statistical methods are used to combine different observations together, to portray a gross picture of cloud development and aerosol effects on it. An indirect evidence of more latent heat release caused by aerosols is the increase of ice water path with AI/AOT in Fig. 1B and 1D, but a direct evidence is hard to find at present, which may require a dedicated field campaign like the SEAC4RS coordinated by NASA in the Southeast Asia. We plan to use the data to tackle with this particular problem.

l11 and elsewhere in the manuscript: It might be a better choice to say “high-aerosol” (or maybe “polluted”) rather than “dirty”. All “dirty” in the manuscript are changed to “polluted”.

l22: the wording here should be revised to be more careful about causes for the correlations found The wording is revised according to the reviewer’s suggestion.

l28: this is not necessarily the case. The scavenging depends on precipitation intensity, frequency, droplet size distributions, mixing of aerosol into the cloud and below cloud. These characteristics may well be very different for the different cloud types. The noted complication is mentioned, and more discussion on the scavenging effect is added.

p5008 l14: the most important quantity is relative humidity and its fluctuations at the cloud scale. We agree that the most important quantity is the relative humidity and its fluctuations at the cloud scale. The reason that the column water vapor is used here is that the cloud-scale relative humidity is hard to obtain using current satellite observations. The column water vapor, however, can be retrieved from satellites with relatively high accuracy. Therefore, the column water vapor is used to represent the large-scale available water vapor.

l17: this does not seem to be true for liquid clouds? For liquid clouds, the LTSS significantly increases with AI. Therefore, we cannot draw any conclusion that the changes in clouds are caused by aerosols.

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l24: don't the meteorological data include data assimilation? Yes. The meteorological data are from data assimilation. The meteorological data represent the large scale forcing, while the invigoration effect is limited to smaller scales.

Table 1: precipitation intensity is missing from the table. The information about the precipitation rate is added to the table.

Captions Fig. 1, 2, 4: It would be necessary to explain the statistical quantities at the upper right of each figure. Are these computed from the entire dataset, or from the binned data? The statistical quantities are added to figure captions. They are computed from binned data.

Fig. 1, 2, 4: It would be necessary to show the amount of data within each bin of AI/AOT The sample sizes of each point in Fig.1 are shown in Table 3 and 4.

Fig. 1: Why is R2 = 0.54 so large for liquid given the curve is rather flat (and given also the author's interpretation of it)? The rather flat curve is due to the relatively coarse y-axes scale. In another word, the correlation is high but the absolute change is small. It's hard to draw any conclusions here, since the LTSS for liquid clouds increases with AI as shown in Fig. 4B. We cannot rule out the influence of meteorological conditions here.

Fig. 2: It would be necessary to explain the difference between 2b and 1a in the caption. Is it just that 2b selects only cases where the Cloudsat-retrieved precipitation rate is > 1 mm/h? Yes. In Fig.2b, only clouds with the precipitation rate > 1mm/h are selected.

Fig. 4: The substantial variation in LTSS for "liquid" clouds is interesting and needs discussion. We don't know the exact reasons that cause the variation in LTSS with AI for liquid clouds.

Suggestions for a revision in a more careful language: Title: Correlations of satellite-retrieved cloud-top temperature and precipitation intensity with column aerosol concen-

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tration for different clouds over the Tropics Abstract, starting p5004 line 7: ...we identified two distinct correlations of clouds and precipitation with aerosol loading. Cloud-top temperatures are significantly negatively correlated with aerosol index (AI) over oceans and aerosol optical thickness (AOT) over land : : :; no significant correlations were found for liquid clouds. The distinct correlations might be explained by two mechanisms... Aerosols may invigorate... Precipitation rates are found to be positively correlated with AI for mixed-phase clouds, but negatively correlated for liquid clouds. If the correlations are due to a cause-effect relationship where the aerosol influences cloud and precipitation, these effects... A similar revision would be needed for the Conclusions section.

A similar title as suggested is used now and thorough revisions are made according to the reviewer's suggestions.

Interactive comment on "Cloud invigoration and suppression by aerosols over the tropical region based on satellite observations" by F. Niu and Z. Li Anonymous Referee #2 Received and published: 17 February 2011

This manuscript uses a combination of datasets (CloudSat, CALIPSO, MODIS, ECMWF-AUX) to examine the effect of aerosol particles on cloud development and precipitation. They use statistical analysis to conclude that increasing aerosol pollution will reduce precipitation in warm clouds and increase precipitation in mixed-phase clouds. They specifically show that cloud top temperatures decrease with increasing aerosol index over oceans and with increasing AOD over land for mixed-phase clouds. They conclude these results are due to aerosol invigoration effects and microphysical effects, which is consistent with ideas previously proposed. The topic certainly should be of interest to ACP readers, and the results are consistent with ideas presented in previous work that they cite. The paper is concise and to the point, and it is written fairly well. My main issues relate to the lofty conclusions drawn without a sufficient amount of attention to the lack of identification of true causal relationships. The authors need to relax some of their statements. In particular, I believe the analysis to produce Fig-

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ures 1, 2, and 4 should be revisited to more carefully present results to try to isolate an aerosol effect rather than allow cloud water to vary when comparing cloud top T or precipitation to AI/AOD. I recommend publication after significant revisions.

We agree that the statistical analysis alone cannot identify true causal relationships. Therefore, some statements have been relaxed and more careful wording is used thorough the paper.

As shown in the new Table 3 and 4, the relatively small sample sizes for warm base mixed-phase clouds do not allow more detailed constraints on clouds water or other conditions. However, we did a simple test by constraining cloud liquid water path within a limited range: 800-1200 g/cm2. The result also shows a negative correlation between cloud top temperature and AI as shown in Figure 1. Please note that we only used bins with the sample size larger than 10 in the test. Another two tests on the constraints of CWV and LTSS are also done and shown in the following responses.

Specific Comments: In certain parts of the paper some of the claims and conclusions should be relaxed. It is far-reaching to jump to quick conclusions as in page 5007 (line 3) that increasing ice water path is due to enhanced ice processes and aerosol invigoration effects. This claim appears four sentences into the results section without giving serious treatment to the importance of meteorology (this begins to be addressed around page 5008, line 3. This certainly should be modified, at least the order that these sentences are constructed. The authors should note that they cannot truly identify cause-and-effect relationships owing to the nature of their analysis, but can only suggest what the possible causes are for the interesting relationships they found.

Most of those claims were removed, modified or re-sequenced per the suggestion, and some are further explored as a possible explanation. More careful wording is used.

The way Figure 1 is presented is misleading as only the relationship between aerosol and cloud top temperature is shown without any consideration of other factors that could influence cloud top temperature such as meteorology. Figure 2 plots precipitation

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rate versus aerosol without any consideration of meteorological effects. Shouldn't rain depend on the amount of water in the clouds?, so why isn't this treated in the figure? Again, this plot is misleading like Figure 1 as a greater emphasis needs to be placed on separating aerosol and meteorological effects.

Sorry, we did not explain the reasoning behind our analysis method. It is definitely true that cloud and precipitation are affected by meteorology and much more so than aerosols. However, meteorology changes day to day rapidly. By analyzing the relationship as shown in Fig. 1 and 2 for a large ensemble of cases, we have effectively lessened them considerably, if not removed, the influence of meteorology as each point in the figure represents a large ensemble of all-kinds of meteorological variables whose overall effect would be statistically insignificant with those for other data points of different aerosol loading (more analyses are made and given below). Had we show the result for an individual weather episode, meteorology would have a dominant effect over aerosols. Besides, for each correlation between clouds and AI/AOT, we tested the correlations between any indices of meteorological conditions (e.g. CWV and LTSS) that we can obtain and AI/AOT.

The following is added "While cloud and precipitation are affected by meteorology much more than by aerosols, the influences are generally fast-evolving as seen in the changes of weather patterns/episodes. By analyzing the relationship between cloud and precipitation with aerosol loading for a large ensemble of cases, we intend to average out the influences of meteorological conditions. For the large volume of satellite data as employed here, the meteorological conditions corresponding to a fixed range of AOT for all the data analyzed would have little difference from those for a different range of AOT. As such, any dependence on AOT is more manifestation of the influence of aerosols than that of meteorology."

We agree that the best way to do it is to examine the relation between cloud top temperature and AI/AOD under fixed meteorological conditions. Let alone difficulties in defining meteorological conditions, the sample sizes are not large enough to apply any

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detailed constraints on T, P, LTSS, CWV and LWP, etc. Here, we did perform two more tests using a fixed range of CWV or LTSS as shown in Figure 2. We can see that the relation between cloud top temperature and AI dose not change even with limited changes in LTSS or CWV. In a response to an above comment, we also showed a similar correlation between cloud top heights and AI with a fixed range of LWP. However, the sample size prevents us from doing more tests using more ranges of both CWV and LTSS.

As for rain, indeed, a lot of factors could influence rain. The amount of water in clouds may be just outcomes of meteorological conditions such as available water vapor and atmospheric stability or even aerosol effects. So we did two more tests on the LTSS and CWV under conditions AI>0.3 and AI<0.3. The results are shown in Figure 3. We can see that both of them have no systematic difference under different AI conditions, implying that the difference in rain distribution may not be caused by systematic difference in the meteorological conditions.

Why is AI plotted on a log scale and AOD on a linear scale? Please clarify. The AI is a product of AOT and the angstrom exponent. Its value is not evenly distributed. For example, about 56.4% data are less than 0.1, about 92.4% data are less than 0.2, and about 97.9% are less than 0.3. Plotted on a log scale, the changes of cloud properties or the precipitation rate with AI can be better demonstrated. The AOD is relatively evenly distributed, therefore a linear scale was used.

Why is AOD used over land and AI over the ocean? This clarification is needed. AI is preferred in the study since AI serves a better proxy for cloud condensation nuclei (CCN) than the AOT as shown by previous studies (Nakajima et al., 2001; Feingold et al., 2006). However, over land, AOT is used instead of AI because the Angstrom exponent retrieved from MODIS over land is not quantitative and much less reliable than over oceans (Levy et al., 2010), which is stated in the paper.

How was the AI and AOD binning performed for the x-axes of Figures 1, 2, 4? Clarify

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on page 5006, line 15-17. AOT over land is binned with equal intervals. AI is binned with equal intervals of log(AI). Therefore, on a logarithmic scale in Fig. 1A and 1B, the x-axis shows equal distance between each point.

Why is the LTSS range so different over land versus over the ocean? Don't know for sure, but note that continental weather has very different regime than marine weather, which may be the reason.

More discussion is needed to share the sample sizes used in each of the curves of the plots. Presenting correlation coefficients with sample sizes would be much more meaningful. How many points are used in each AI and AOD category (clarify somewhere in the figures or in a table)? Do these sample sizes typically agree across the range of the x-axes values (if not, could this affect the results?)? Also, what is the total sample size that the y-axis of Figure 3 is based on?

The number of samples for each point in Fig.1 is given in Table 3 and 4. By removing some of the bins with much less samples, for example the first and the last bins, we find the slopes of the regressions will be different, but they won't change the conclusions.

Figure 1 would be easier to interpret if the y-axes were changed. For example, currently it is hard to see the differences in cloud top T for liquid clouds in the top left panel. The y-axes for liquid clouds (right y-axes) was chosen as the same magnitude as the mixed-phase clouds (left y-axes). It could be misleading if we use a more fine scale for liquid clouds to show the small changes but a coarse scale for mixed-phase clouds in the same figure. Besides, the changes for liquid clouds may be partially contributed by the positive correlation between LTSS and AI, and we cannot draw any conclusions on aerosol effects based on the changes.

How did the authors choose the temperature thresholds for the different categories of clouds (Page 5006, line 10-12)? The thresholds were chosen based on the review article of Rosenfeld et al. [2008].

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On page 5008, lines 12-24, the authors should state if they have any reason to believe how AI and/or AOD should be related to LTSS or CWV. Due to hygroscopic growth of aerosol particles, there might be a positive correlation between AOD and CWV. Under a stable atmospheric condition (higher LTSS), pollutants may be harder to disperse, leading to higher AOD. As such, there may exist a positive correlation between AOT and LTSS.

I would suggest that the authors re-do Figures 1, 2, 4 in such a way that they examine the relationship between AI/AOD versus cloud top T or rain at fixed conditions of LTSS/CWV. Otherwise, the plots as I noted above are misleading and can lead to false conclusions. This comment has been addressed in a previous response.

Page 5008, Line 20: Are the authors suggesting that there is a cause and effect relationship between AOT and atmospheric stability? If not, words should be more carefully chosen in this line. We are not sure about the cause of the correlation, and thus we do not suggest there is any cause and effect relationship.

Technical Corrections: The authors cite work in the body of the work that is absent in the references section. They should make these corrections. The reference list has been corrected.

The x-axis label on Figures 1 and 4 (opthical) is spelled wrong. The error is corrected.

Interactive comment on "Cloud invigoration and suppression by aerosols over the tropcal region based on satellite observations" by F. Niu and Z. Li Anonymous Referee #3 Received and published: 6 April 2011

The authors try to estimate the aerosol effects on warm and mix phase clouds by using aerosol information from MODIS and cloud properties from CloudSat and CALIPSO. They show clear invigoration for mix phase clouds with warm bases and no effect (or opposite in case of rain) for warm clouds. While appreciating the scientific question, I see two major problems in this work: 1) I see no real efforts to separate meteorological from aerosol effects and 2) scale mismatch between the MODIS and CloudSat /CALIPSO data.

1) I see no real effort to reduce the meteorological effects. It is well known that the local thermodynamic conditions play a key role in determine the cloud properties. The authors cluster clouds only by their base and top height (or temperature) mixing many tropical and sub tropical profiles aiming to show that the two meteorological parameters: lower tropospheric static stability (ltss) and column water vapor can not explain the observed trend. Such statement is far from being enough to make the point that the observed effects are due to aerosols.

As stated above, unraveling the meteorological influences from observational data is always a daunting task, especially for satellite-based studies due to their inherent limitations. With the multi-sensor products, we are at least able to apply more constraints to investigate the effect of aerosols than many previous studies, but it is hard to directly assess meteorological influences.

The very reason that we employ a large ensemble of data is try to minimize, if not remove, the influence of meteorology. For any individual weather regime, cloud and precipitation are affected by meteorology much more than by aerosols. However, meteorological condition changes rapidly. By analyzing the relationship as shown in Fig. 1 and 2 for a large ensemble of cases, we have effectively averaged out the influence of meteorology as each point in the figure represents a large ensemble of all-kinds of meteorological variables whose overall effect would be statistically same/similar to those for other data points of different aerosol loading.

As shown in the responses to other reviewer's comments, the relation between cloud top temperature and AI does not change even we used fixed range of CWV (55-60mm) and LTSS (15-20oC).

We admit that the tests conducted are insufficient to rule out the impact of meteorology. Per the comments of other reviewers, we have softened our arguments.

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In light of the publication of our companion study (Li et al., Nature-Geosci. Nov. 2011), we would refer the review to read that paper that took advantage of ample and a wide variety of ARM measurements to untangle meteorological and aerosol effects. The same approach is adopted in the two studies. In that study, we are able to examine any potential connections between aerosol and other meteorological variables (both surface and atmospheric profiles), but did not find any. Another key piece is the totally flat line between cloud base and CN concentration, while cloud top changes with CN of different sensitivity pending cloud height, convection, season, etc. Literally, we proved the assumption that meteorological influences are abated with increasing data samples, as all sound physical relations stand out clearly. The perfect agreements between surface-based and space-borne relationships between the two studies constitute more compelling evidences to support the argument, unless if anyone can come up with a silver-liner alternative explanation for so many coincidental findings.

2) The analysis is done on one degree grid resolution (MODIS level 3) by scaling the CloudSat and CALIPSO curtains to the grid box. It is not clear why the analysis is done in such way. What portion of the grid-box the CloudSat and CALIPSO cross-sections represents? To the best of my knowledge these instruments have narrow swath that cover very little surface area in the middle of the MODIS (AQUA) larger swath. Why not working with level 2 and comparing aerosol data that is relevant to the CloudSat and CALIPSO footprint? More in this direction: it is not clear what resolution of the ECMWF-AUX they used. Is it for the 1 degree grid box or only along the CloudSat and CALIPSO path?

We agree that the Cloudsat and CALIPSO curtain only covers a very small portion of the 1o$\times$1o grid. While one may use the level 2 data to study the aerosol effects, we chose the level 3 data in this study for the following considerations: a) AOD data were not generated at the native resolution of 1km, compatible to that of CloudSat/Calipso, but at $\sim$10km due to various reasons somewhat similar to the factors given below. b) Using the large-scale data helps reduce the problems of artificially high remotely

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sensed aerosol burdens near cloud boundaries. In generating the level 3 aerosol product, a QA confidence flag was used to reduce possible cloud contamination. c) In the study of the aerosol indirect effects using satellite data, cloud and aerosol parameters must be retrieved from cloudy and clear pixels respectively. There is little chance for the two to co-exist only among immediate adjacent pixels for which cloud-contamination is so serious that would make AOD retrieval invalid. On the other hand, aerosol usually has a much smoother spatial variation in space than clouds do. It is thus a more sound assumption that AOD retrievals within 50km (maximum distance) are less variable than the contamination near a cloud edge.

More comments: 3) The classification between aerosol invigoration and microphysical effects is confusing. Both effects starts from the microphysical (or the droplet evolution) scale and both affect cloud dynamics. Aerosol invigoration is balanced by early warm rain suppression and by enhanced evaporation which is not discussed in this paper and can be important.

Although both effects originate from the microphysical scale, the invigoration effect is associated with latent heat and dynamics, while the microphysical effect is linked with cloud microphysics: particle size, droplet number and ensuing collision and coalescence processes. The effect of evaporation is much more complicated. On one hand, the invigoration effect could be balanced by the enhanced evaporation. On the other hand, studies show that evaporative cooling can enhance downdrafts, generate stronger squall line and secondary clouds. We have much less quantities to address this issue, but to mention the effect more thoroughly than we can elaborate with the highly limited satellite products. More discussions are added.

4) Why AI? Andreae (2009) suggests that AOD by itself is a good measure. Do the authors see better correlations when using AI? (Andreae, M. O.: Correlation between cloud condensation nuclei concentration and aerosol optical thickness in remote and polluted regions. Atmospheric Chemistry and Physics 9, 543- 556 2009).

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Based on (Nakajima et al., 2001, Breon et al., 2002), for a given optical thickness, the number of CNN is much larger for a sub-micronic aerosol (typical for biomass burning smoke and pollution) than for larger sized particles such as dust. In this regard, the angstrom exponent can serve as a rough index of the proportion of CCN out of total aerosol number concentration, as it decreases with increasing the particle size increases. The combined aerosol index (optical thickness * angstrom exponent) can partially account the size effects and is better related than the optical thickness to the CCN. As an example of this approach, Breon et al. (2002) use the unique aerosol sensing capabilities of the POLarization and Directionality of the Earth's Reflectance (POLDER) instrument to demonstrate the negative correlations between Re and AI. Therefore, we used AI as a measure of the column CCN. We tested the correlations using AOT, and found better correlations when using AI.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C12097/2011/acpd-11-C12097-2011-supplement.pdf

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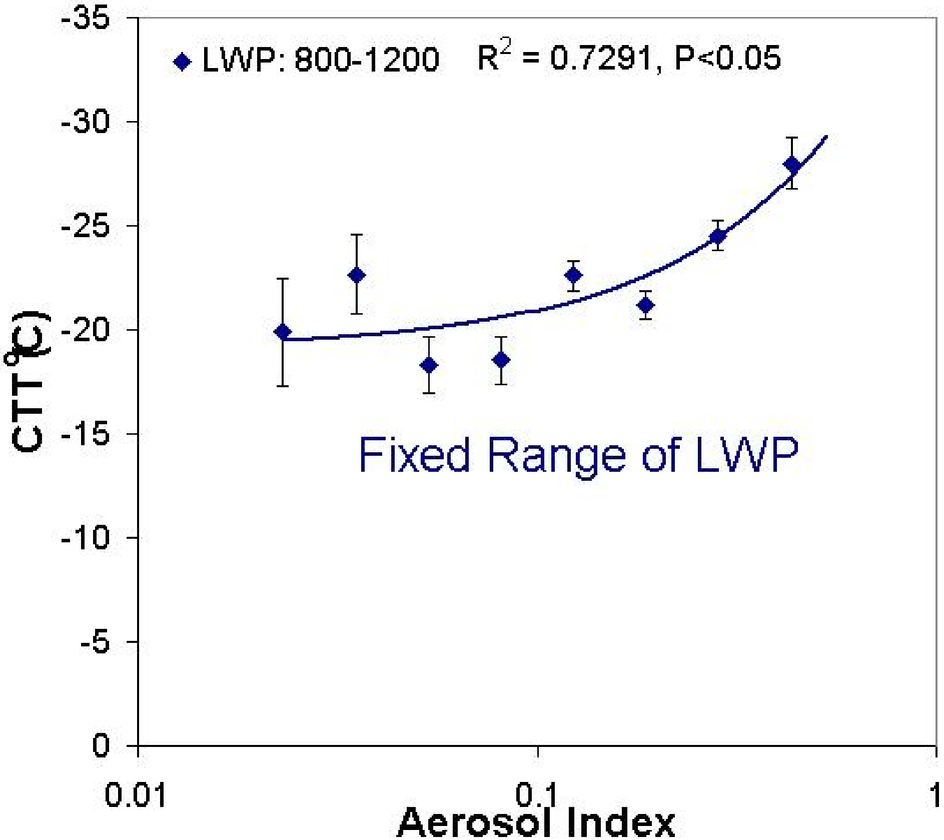

Fig. 1. Correlation between cloud top temperature and AI for clouds with LWP in the range of 800-1200 g/cmˆ2

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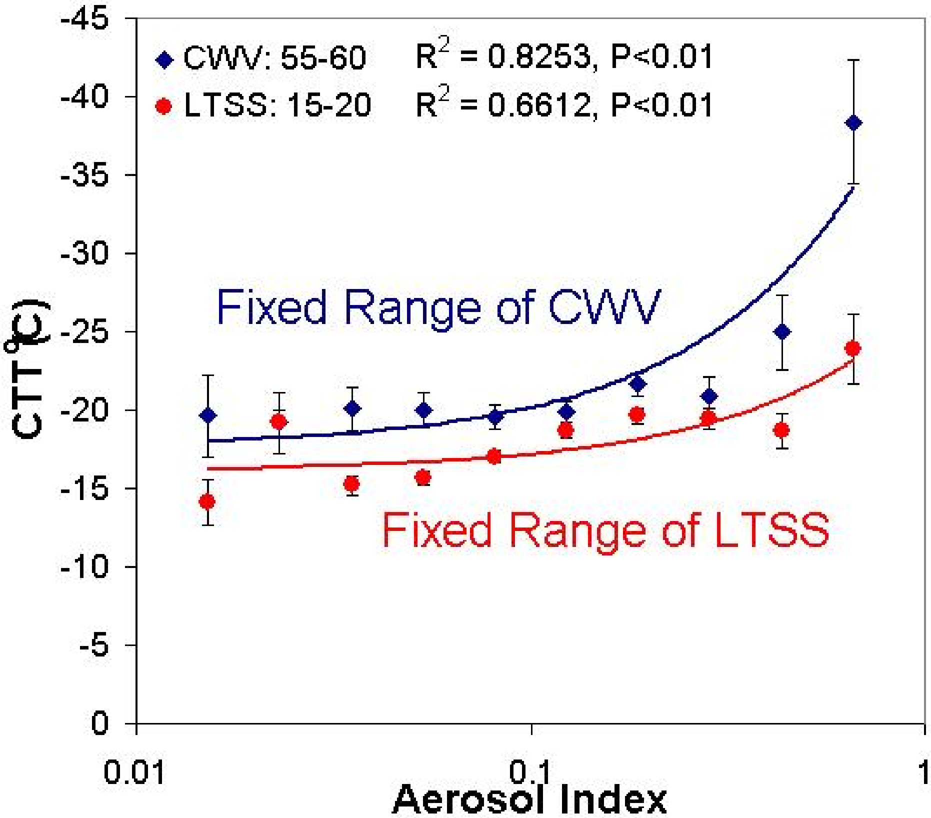

Fig. 2. Tests on the relation between clout top temperature and AI using fixed range of CWV(55-60mm) and LTSS (15-20 degree Celsius)

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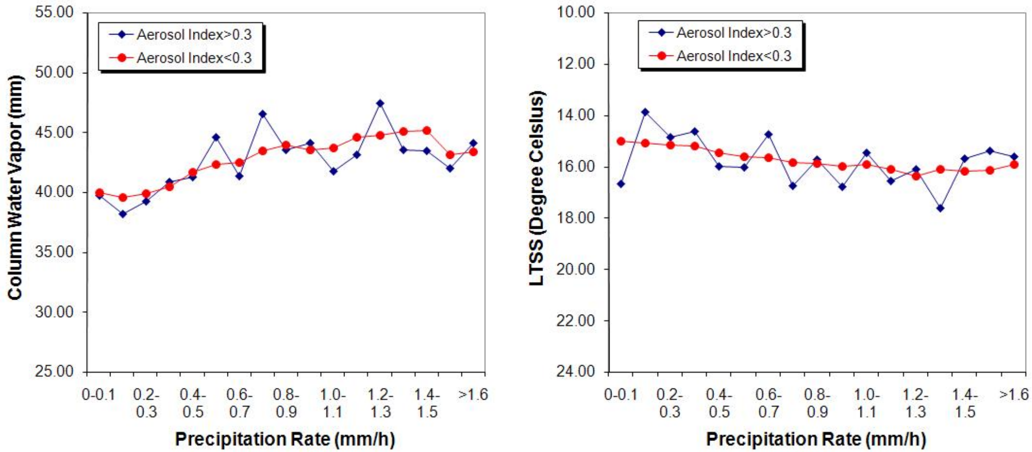

Fig. 3. CWV and LTSS as functions of precipitation rates for conditions AI>0.3 and AI<0.3

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