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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol-induced changes of convective cloud anvils produce strong climate warming" *by* I. Koren et al.

I. Koren et al.

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We would like to thank Prof. Rosenfeld and the two anonymous reviewers for their efforts and for the comments that helped us present clearer and more complete paper.

We have addressed all of the reviewers' comments and we are confident that with the additional information on the analysis and the additional results the proposed chain of events presented in the paper is clearer. As requested by the reviewers, in order to validate the significance of the results, we performed similar analysis on additional datasets over the Pacific as well as on data from other years. Moreover as the reviewers requested, we performed filtering on the data to show sensitivity to the meteorological conditions and how the aerosol effects depend on them. Also, as Prof. Rosenfeld requested, we show similar analysis using the GOCART transport model for aerosol

information.

Details and results of all the new analyses are discussed below and are now presented in the manuscript.

We will start our response to the reviewers with general comments that are relevant to all of the reviewers followed by specific point by point answers.

General comments

In this paper our objectives were to describe a chain of events that are likely to happen in deep convective cloud environments as a function of the increase in aerosol loading. Our analysis (and cited papers) shows that increase in aerosol loading is likely to invigorate convective clouds. Taller convective clouds will likely create anvils at higher levels of the atmosphere, which will be subjected to higher average horizontal winds. Due to the higher wind speeds, the anvil area is expected to increase and the average ice-cloud optical depths are expected to decrease. We now show evidence to these claims in the manuscript and we also discuss the possible effect on the top of the atmosphere radiation balance due to such effects.

Our available dataset has many limitations that prevent us from being able to provide a quantitative assessment of the total forcing derived from such feedback but, we discuss openly the main limitations of the dataset. In fact, because of the level of detail and the heavy analysis required, we addressed the two main problems of this type of analysis (namely: correlation with meteorology and cloud contamination of the aerosol retrieval) in a separate (supporting) paper that is currently also in review in ACPD (The invigoration of deep convective clouds over the Atlantic: aerosol effect, meteorology or retrieval artifact?, http://www.atmos-chem-phys-discuss.net/10/3893/2010/acpd-10-3893-2010.html). Based on the reviewers comments we added to this study an analysis of the aerosol effect for different atmospheric conditions and we emphasized reference to the supporting paper mentioned above that contains more detailed meteorological analysis.

As requested by all the reviewers, we moved the information from the appendices to the body of the paper and added more details on the radiative transfer calculations and other parts of the analyses.

As we reported, the poor coverage of pixels that contains both anvil and aerosol information reduces the confidence level of our proposed trend. To overcome such problem the reviewers suggested two additional analyses:

1) Using the GOCART transport model as a source for the aerosol loading information instead of measurements, and

2) To decrease the data resolution in order to gain more overlap between the cloud and aerosol data.

We addressed these two important points and have the following answers:

1) As we now show in the paper and added reference, we have extensively explored this option. But, as much as we want to have an alternative to satellite aerosol information from transport models (such as GOCART), the models are “not there” yet. For example Chin et al. [2009] show a detailed comparison of GOCART with AERONET using monthly (Table 3) and annual averages (Figure 6) at 173 collocated AERONET sites over an 8 year period. The correlation between GOCART and AERONET annual averages yields R= 0.73 and a RMSE of 0.121. However, overall the monthly mean statistical comparison degrades to R=0.685 and RMSE = 0.154, with individual regions sometimes exhibiting correlations as low as R=0.44 (Europe) and RMSE = 0.245 (Asia) on a monthly basis. To try and do the same on a daily basis would yield even worse correlation and higher errors (Chin, personal communication).

In contrast, MODIS AOD collocated with AERONET on a daily and 10 km basis consistently yields correlation coefficients above 0.90 and RMSE $\sim$0.05 (Remer et al., 2008). There is simply no comparison.

The GOCART model provides an excellent representation of the distribution of aerosol

loading, especially at climate scales (monthly and annual means), but it is still not as good as an observational data set on a day-to-day basis. Still, the promise of an AOD data set, uncontaminated by clouds, was too great a temptation and we explored the possibility of using GOCART in Koren et al ACPD, 2010 (hereafter K2010B). This resulted in an interesting analysis that supported the use of the MODIS AOD in aerosol-cloud studies in the region of interest here. The use of GOCART in K2010B was a side exploration of that paper and was never meant to suggest that the model results should supplant the use of observational data in these kinds of studies. To satisfy the reviewer’s interest (and ours) we have added an analysis using GOCART AOD in this paper, which produced similar trends as the ones using MODIS AOD. Again the use of GOCART AOD is not meant to replace the use of the more accurate and more appropriate (on a daily time scale) satellite data but nevertheless, it helped to increase the confidence on the results.

References:

Chin, M., T. Diehl, O. Dubovik, T.F. Eck, B.N. Holben, A. Sinyuk and D. Streets, 2009: Light absorption by pollution, dust, and biomass burning aerosols: a global model study and evaluation with AERONET measurements. Ann. Geophys., 27, 3439–3464.

Remer, L. A., R. G. Kleidman, R. C. Levy, Y. J. Kaufman, D. Tanre, S. Mattoo, J. V. Martins, C. Ichoku, I. Koren, H. Yu, and B. N. Holben, 2008: Global aerosol climatology from the MODIS satellite sensors. J. Geophys. Res., 113, D14S07, doi: 10.1029/2007JD009661.

Koren et al, ACPD 2010 - The invigoration of deep convective clouds over the Atlantic: aerosol effect, meteorology or retrieval artifact?, http://www.atmos-chem-phys-discuss.net/10/3893/2010/acpd-10-3893-2010.html).

2) In the paper we clearly show that over the ITCZ the likelihood of having both information on clouds and aerosol for the same pixel decreases with the cloud height. This goes inline with the paper’s main message that at higher levels of the atmosphere

due to natural expansion of the capped anvil and to stronger horizontal winds, clouds encompass a larger area (greater horizontal coverage). Therefore the likelihood to find a 1 degree pixel that is fully obscured by cloud or that the cloud free portion is too low for a reliable aerosol retrieval is increased. Increasing the pixel size (or decreasing the resolution) indeed will allow for more high clouds to have coexist with a "collocated" aerosol retrieval; however, not "for free". Averaging larger areas results in a loss of spatial information. Pixels of 2 by 2 degrees (~220km length scale) may smooth out significant aerosol differences. Here we propose and have used a more selective way of extrapolation of the AOD instead of down-sampling. We estimate the aerosol optical depth (AOD) of pixels obscured by clouds by averaging the AOD of their neighbor pixels that have AOD retrievals. We only perform the extrapolation in cases where the adjacent neighbor has a valid AOD retrieval. Such method allowed us to retain the finer resolution of the pixels with AOD values and to estimate in a similar fashion to down-sampling, the AOD for overcast pixels. We have added such analysis both over the Atlantic and the Pacific Oceans showing that indeed we can analyze thicker clouds that produce the same trends as we had before testing this new method. These additional results only reinforced our original conclusions.

Point by point reply to the referees comments -

Referee #1 Prof. D. Rosenfeld

A) Referee - The paper address a very important issue of the aerosol induced effect on anvils producing positive radiative forcing. This is important because it potentially offsets the comparatively strong aerosol induced negative radiative forcing from low clouds. The net effect is so uncertain because it appears to be a small number resulted by the difference between two large numbers with high uncertainties associated with them. While the research objective and idea are excellent, there are serious problems with their application. The authors try to use MODIS for obtaining both cloud and aerosol properties from the same 1x1 degree areas. This creates several major problems:

1. Most of the anvil areas cannot be considered at all, because they are fully cloudy. According to Figure B1 this excludes the vast majority of the anvil area. Therefore, the methodology cannot inherently quantify the effect.

2. There is still the concern of cloud contamination and aerosol expansion near the clouds, which would enhance the indicated effect. The authors discuss this possibility, but beyond recognizing it they do not account for these effects. The authors would better in this case follow the path that they themselves portrayed in their recent submission to ACPD (Koren, Feingold and Remer) and use GOCART. This would solve both problems. While doing so, they should validate that GOCART replicate the AOD at the scales of interest, by comparing GOCART to AOD for scenes having similar cloud conditions. Another or additional possibility is using areas that are much larger than 1X1 degree, which will leave only negligible portion of the anvils unaccounted for. This will require testing whether the aerosols are not changing too much within these larger areas.

A) Author - We thank Prof. Rosenfeld again for his comments. As stated above, in (the reply to all reviewers part), in the revised version we did all of the reviewers proposed validations and more. We added another study area over the Pacific and for both areas we repeated the analysis for extrapolated data, using adjacent pixels to calculate AOD of overcast pixels to get better coverage and to enlarge our dataset. As requested, we also repeated the analyses using GOCART aerosol data instead of MODIS (as a "sanity check" to eliminate the problem of contamination and coverage). Again the results are consistent with our preliminary analysis. Moreover, even though a whole supporting paper is dedicated to the questions of cause and effect (meteorology) and contamination (referred to in the relevant places in this paper) we also added a whole section to this revised manuscript where we reduce the meteorological variance by restricting the analyzed data with the large scale updraft velocity (positive against negative values) taken from the GDAS data.

The additional analyses results are presented in Figures 3, 6 and 8.

In order to provide more details on these issues, the discussion section of the paper is now divided into new subsections entitled:

3.1 The questions of retrieval artifacts and meteorological confusion 3.2 Fully overcast pixels and 3.3 Restricting meteorological variance

B) Referee - I don't understand how the cloudy fraction is constructed in Figure 1c, because it keeps growing to unity at the top of the troposphere. Some higher level of analysis was done here that is not described.

B) Author - On the results presented in Figure 1c: The point of the figure is to show that clouds with higher tops have larger cloud fraction. To do so we have collected all pixels over the Atlantic study area with cloud fraction C and cloud top pressure P information. Then all the pixels were sorted by their P values. In order to reduce the variance, every 60 pixels with similar values of P were averaged into 1 point. The result is that for clouds with cloud tops higher than the 300 hPa pressure level, the pixels are almost completely overcast. This is striking, but it is not a result of any additional data manipulations.

In order to help clarify this issue we added the following information to the revised manuscript:

“The link between cloud fraction and cloud height (measured as cloud top pressure) over the Atlantic study area is shown in Figure 1c. Indeed it is shown that the average cloud fraction (each point is an average of 60 one-degree MODIS pixels after sorting all pixels by their cloud top pressure values) increases as a function of the cloud top height. The change in slope at $\sim$850 hPa marks the transition from the lower (marine boundary layer) to the free - upper troposphere where the clouds have larger average fraction. At 300 hPa almost complete overcast conditions are reached.”

C) Referee - Similarly, the very high cloudy fractions in Figure 1d are clearly not representing the full analysis area. This is especially so when the fully cloudy 1x1 degree

areas are excluded from the analysis. The authors are missing key information in reporting what they actually did. The paper is very generous in explaining the simple things, for example, what is an anvil, but is lacking on the detail of the scientific analysis to the extent of making it incomprehensible.

C) Author - Indeed we missed some of the details (they were written in the figure caption but not in the text). A more detailed explanation was added to the text and are also present in the cited references (Koren et al, 2005; Koren et al, 2008a)

In order to enhance the information requested by the reviewer we added the following discussion to the revised manuscript:

“How does aerosol loading affect the dependence of cloud fraction and height? After filtering out shallow clouds (tops below 800 hPa), the cloud fraction data over the Atlantic was sorted according to the aerosol optical depth (AOD) and divided into 3 equally-sampled groups with mean AOD of each group equal to 0.13, 0.24, 0.41, respectively. Then the same analysis described for Fig 3c was done for each subgroup (cloud fraction was plotted as a function of the pressure level for each group (see for more details: Koren et al, 2005; Koren et al, 2008a). While the cloud fraction of all 3 groups increases as a function of the pressure level, a clear shift of the polluted clouds towards larger fraction is shown for each pressure level. Moreover the invigoration effect is also shown as the polluted clouds are taller (figure 1d). “

D) Referee - The radiative forcing analysis provided in Figure 3d is brilliant. However, here again much background explanation of the assumptions is missing. The assumed vertical profiles of temperature-effective radius for water and ice, and the vertical partitioning of water and ice must be given.

D) Author - As Prof. Rosenfeld requested we rewrote the radiation part and added more background details on how the solar and thermal radiation depends on cloud properties and why the "τ-Z cloud forcing plane" gives important information. As requested by the other reviewers, we moved all of the radiative transfer explanations from the appendix to the main text and added better description (with an extra plot-Figure 4b) to why averaging cloud optical depth would lead to forcing (mostly warming) underestimation. A whole section is dedicated to the forcing estimations titled "2.2 Top of the Atmosphere Forcing estimations – Introducing the τ-Z cloud forcing space"

Point be point reply to Anonymous Referee #2

Referee - Review: The submitted manuscript examines aerosol-cloud interactions in tropical deep convective clouds using a satellite-based approach. Specifically, the work attempts to qualitatively analyze the net cloud radiative forcing due to possible aerosol-induced effects, e.g., convective invigoration, smaller mean cloud droplet size, etc. Using MODIS data, the authors show that an increase in the aerosol number concentration acts to reduce the mean ice optical depth. This is caused by an increase in the ice anvil area with increasing aerosol number concentration. The manuscript describes, in detail, the chain of events that link the increase in aerosol number to a decrease in ice (anvil) optical depth. In regard to climate, the effect of such a perturbation on the ambient environmental conditions is discussed. The authors note that the overall effect of the aerosol-induced anvil enlargement is to generate a positive cloud radiative forcing, or a warming at the surface.

Major Comments:

A) Referee - The study focuses on a region of the tropical Atlantic bounded to the south by the equator and to the north by 14 N during the months of June through July in 2007. The analysis for this region and time provide robust the results described above. However, it would be beneficial to the reader to have a discussion related to this choice of region and time before the beginning of section 2. Some of the points that may be addressed are: Would the results differ if one chose the Pacific Ocean over the Atlantic Ocean? Since the chosen period of time immediately followed an El NiĖIJno event, how might the results change if the data were selected for a different period of time?

A) Author - As the referee suggested we added analysis over the Pacific. We ran all of the new additional analyses (including MODIS extrapolated data for overcast pixels and GOCART aerosol data) on both areas and show the same trends (in fact over the Pacific the trends are cleaner). We also did the same analyses over the Atlantic for additional years, 2002 and 2005 and got the same trends. In the revised paper in the end of section 2.1 we write: "Over the Atlantic we repeated the analysis using 2002 MODIS Terra (10:30 am local-time overpass) and 2005 Aqua data. The trends were similar in all years both for Terra and Aqua."

B) Referee - The introduction proposes in lines 4 - 5 on page 1942 that the authors will "estimate the anvil enhancement consequences to the climate radiative balance". Then, at the end of section 2 it is concluded that the anvil enhancement causes a "significant warming". However, from figure 2d and the associated caption it appears that an attempt at quantifying the warming was made (blue and green circles in figure 2d). I believe that it is prudent to move the discussion regarding these circles from the caption to the main text and include it with the discussion related to figure 2d. This is an important result of the paper and, although there are likely large uncertainties in the estimates of the positive cloud forcing shown in figure 2d, it is somewhat hidden in the caption to figure

B) Author - Indeed the circles were an attempt to start quantifying the forcing (based on the Atlantic results). Although the forcing over the Pacific is even more dramatic, in the revised version we decided to restrict the paper on showing the trends and their possible consequences to the overall forcing without quantification. We do however give one numerical example on the warming underestimation due to averaging cloud optical depth of anvils and towers (new fig. 4b)

In the revised summary we write: " Due to data limitations however, we do not quantify the radiative forcing of the proposed mechanism. Though critically important, reliable estimation of the radiative forcing due to aerosol effects on deep convective clouds and their anvils will have to be done by using a more complete set of information on clouds, aerosol and meteorology, in conjunction with models. Here we demonstrate a pathway of aerosol effects on clouds that can lead to a positive climate forcing. This is an important mechanism that will be required in future estimates of climate change."

2) Referee - Moreover, the results shown in figures A1 and A2 could be combined into one figure and, along with the discussion in Appendix A, moved to the main text. The τ-Z space diagrams are important for the climate response to the anvil enhancement and so the elaboration in Appendix A should be included in the main text.

2) Author - As the referee suggests, we moved the radiative transfer part form the appendix to the main text. A whole section is dedicated to the forcing estimations titled "2.2 Top of the Atmosphere Forcing estimations – Introducing the τ-Z cloud forcing space". This section was rewritten with more details and end examples.

C) Referee - The authors use histograms to specify whether cloud data represents the anvil or the tower of a deep convective cloud. The demarcation for the two cloud regimes is set at = 10. How was this chosen? It is noted that this value was chosen after inspection but it would be beneficial to the reader to know what criteria were used in making this decision. Moreover, how sensitive are the results to changes in the demarcation value? More details on the statistical techniques used are needed.

C) Author - We added a histogram showing the cloud optical depth distribution and why τ=10 was chosen (figure 2). The MODIS histogram is given in a logarithmic scale (small steps for low COD and large steps for high end). We tried to see the sensitivity to the selected τ from 6 to 18 and the trends were the same. In the revised summary we write: "After inspection of many high resolution anvil optical depth data over the region and based on the cloud optical depth distribution (Figure 2), we divided the histograms into two regimes: anvil (τ<10) and tower (τ>10)."

D) Referee - Appendix B and figure B1 include very important information that needs to be included in the main body of text. The discussion surrounding cloud contamination as well as cause and effect in section 3 should be more detailed. Including the contents

of Appendix B in section 3 would add the necessary details to the main text.

D) Author - As suggested we moved Appendix B and figure B1 to the main text and added additional histograms for the extrapolated data (Figure 7). We added more information regarding cause and effect and cloud contamination. To support this we added the extrapolated AOD analysis and the GOCART analysis.

Minor Comments:

A) Referee - On line 4 of page 1941, "Rosenfeld" is misspelled.

A) Author - Done. Thanks.

B) Referee - The sentence on lines 14 - 16 of page 1942 should be reworded to either say ". . . towards the base of the stratosphere" or ". . . towards the tropopause". Also, the units hPa are given as hPs.

B) Author - Changed to "towards the tropopause" and fixed the hPa units.

C) Referee - In line 8 of page 1946, the word "form" should read "from".

C) Author - Done.

D) Referee - The y-axes in figures 1b, 1c, and 1d are different making comparisons difficult.

D) Author - We tried shifting 1d to 1c scale (in Y and X axes) and it misses much of the dynamic range (1d will have only half of the Y-axes range). We emphasize the fact that the Y axes are different.

E) Referee - The green and blue circles in figure 2d are very hard to decipher. They should be enlarged and/or depicted with different colors (e.g., white and black).

E) Author - In the revised paper we do not show them. We show instead figure 3b.

F) Referee - The color bars on figures A1 and A2 are missing units.

F) Author - We moved the appendix to the main text and added units (new fig 3).

G) Referee - The x-axis of figure B1 is missing units.

G) Author - We moved it to a new figure it the main text (Figure 7) with units.

H) Referee - There are many acronyms used throughout the manuscript. The acronyms should be defined when they first appear.

H) Author - We made sure that in the revised manuscript all the acronyms are defined first.

Summary

A) Referee - The submitted manuscript provides a relation between an increase in aerosol number concentration and ice optical depth in anvil clouds. The net result of this effect is a positive cloud radiative forcing.

B) Referee - I recommend that this paper be accepted with the above revisions.

Point by point reply to Anonymous Referee #3

Referee - This manuscript explores the link between aerosol loading and the properties of deep convective anvils. By analyzing the MODIS cloud and aerosol data and the wind profiles from radiosonde measurements, the authors identified that the invigoration of deep convection by increased aerosol concentrations leads to higher anvil heights. The anvils are therefore expanded and diluted by the stronger winds in higher altitude. The authors then examined the sensitivity of cloud radiative forcing to cloud top height and optical depth, and qualitatively concluded that the aerosol indirect effects on anvil clouds can potentially produce an overall positive radiative forcing.

Major Comments

1) Referee - The study emphasizes on the dynamic and thermodynamic feedbacks of the aerosols to anvil height and morphology. A region of the tropical Atlantic is

chosen for the analysis of AOD-anvil relationship, and the clouds are simply sorted by AOD levels. As there are many crucial ambient controls on deep convection besides aerosol, the sensitivity of anvil height and optical thickness to aerosol loading identified here may not be relevant to the other regions. The thermodynamic environmental conditions (e.g., boundary layer moisture, SST, stability, etc) should be included when classifying the clouds. A discussion on how the current results may hold or change with different convection regimes would also be valuable.

1) Author - Thanks to the referees comments we added the Pacific dataset. We ran all of the new additional analyses (including MODIS extrapolated data for overcast pixels and GOCART aerosol data) on both areas and show the same trends. On a deeper analysis of cause and effect and contamination (in the supporting paper Koren et al, ACPD 2010) it was shown that the best meteorological variable to restrict the meteorological variance of deep convective clouds is the broad-scale vertical velocity. In section 3.3 (Restricting meteorological variance) we ran the same analysis for two subsets of vertical velocities showing separation between the effect of meteorology and possible aerosol effect.

2) Referee - Better descriptions on data processing and methodology are needed. For example, where are the radiosonde stations (mentioned in the caption in fig .1 but not in the text), and how frequent are the measurements? Which versions of MODIS aerosol and cloud products are used? How the deep convection clouds are identified? Is it by setting criteria on MODIS cloud top pressure? Why use tau=10 to separate convective tower and anvil, and how the results may differ if a different cut-off value is chosen? The basic information and sensitivity tests on the methodology should be addressed.

2) Author - As requested, we added much more information on data processing and methodology throughout the revised paper (some listed in the answers above). Specifically: The radiosonde stations are now listed in the text. In the revised paper we write “Figure 1b shows a whole month average (August) daytime horizontal wind speed of the free atmosphere (above the boundary layer) as a function of pressure level for 3

stations along the tropical Atlantic: Natal, Br (5.9S 35.2W); Ilha Fernando de Noronha, Br (3.8S 32.4W), Ascension Island (7.9S 14.4W)." We used MODIS Version 5 (added to the text).

As for the anvil threshold, we added a histogram showing the cloud optical depth distribution and why τ=10 was chosen (figure 2). We tested the sensitivity to the selected τ from 6 to 18 and the trends were the same. In the revised summary we write: "After inspection of many high resolution anvil optical depth data over the region and based on the cloud optical depth distribution (Figure 2), we divided the histograms into two regimes: anvil (τ<10) and tower (τ>10)."

3) Referee - The uncertainties and biases mentioned in Appendix B is crucial, and should be moved to the main text. As the histogram indicates there are many cases with complete anvils coverage (and therefore no AOD retrieval), a sensitivity test of gridding the data to a coarser resolution might provide some additional insights.

3) Author - As suggested we moved Appendix B and figure B1 to the main text and added additional histogram for the extrapolated data (Figure 7). We added the extrapolated data tests to include part of the overcast pixels both over the Pacific and the Atlantic. The new improvement in coverage is shown in figure 7c and 7d.

4) Referee - Although the current results indicate that aerosols lead to expanded area and thinner optical depth of the anvils, the lifetime of the diluted anvils can also change, and this effect may have stronger impacts on the radiative forcing than the changes in cloud height and optical depth. Although the tau-Z plots provide important information to the scientific question, they cannot address the effects of anvil coverage expansion and lifetime on the forcing, and therefore may not tell us the full story.

4) Author - We agree that we do not have time resolution to see the possible effects on the anvils lifetime. Indeed thinning anvils may shorten the cloud lifetime. However a satellite snapshot of cloud coverage on a daily basis should include the lifetime effects. The factor that is important to forcing for a given cloud type, phase and height (say

anvil), is the average cloud fraction. Average cloud fraction is a result of the cloud size and lifetime. If a collection of satellite snapshots on a daily basis can represent the average cloud fraction over an area, then the lifetime effect is accounted for. If the typical polluted anvil area is much larger, we can "see" larger cloud fraction despite the fact that the average polluted cloud anvil will last less than a cleaner one. In summary, we believe that for forcing estimation the anvil coverage includes the lifetime effects.

Minor Comments:

1) Referee - In Section 2, the tower to anvil ratio (TAR) is mentioned and analyzed in the 1st paragraph and Fig 1, but the explanation on how TAR is derived from MODIS data is provided in the 3rd paragraph. Please consider re-organizing the flow by moving the 3rd paragraph to the beginning of the section.

1) Author - We believe that the order in which we presented the results of the TAR analysis in Section 2 provides better motivation for the reader to understand the results, before diving at the specific details on how the TAR was calculated. Since the reviewer considered this as a minor comment, and we strongly believe this is the clearer way to present it, we left it unchanged.

2) Referee - Please use a larger font size for all figures, especially the axes.

2) Author - Done.

3) Referee - A legend/explanation for the line color is missing in Fig. 1b.

3) Author - We linked the colors to stations, thanks.

4) Referee - Fig. A1 and A2 can be merged, since one of the panels is identical.

4) Author - It is merged into new figure 5.

5) Referee - Units on the color bar of Fig. B1 and B2 are missing.

5) Author - We added the color legend and moved the figure to be part of new Figure

7.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 1939, 2010.