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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Enhancement of aerosols in UTLS over the Tibetan Plateau induced by deep convection during the Asian summer monsoon" *by* Q. S. He et al.

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Dear Referee,

Our manuscript acp-2013-860 entitled " Enhancement of aerosols in UTLS over the Tibetan Plateau induced by deep convection during the Asian summer monsoon" has been revised according to your comments. We appreciated your suggestions and endeavor. Two new figures and more statements about the comparison of MPL with CALIOP and the reason of the continuous lidar observation split into two stages were added to the manuscript to support the conclusion. In particularly, the aerosols in UTLS

influenced by Nabro volcano eruption were considered in this study and therefore the title of the manuscript is also changed to "Lidar-observed enhancement of aerosols in UTLS over the Tibetan Plateau during the Asian summer monsoon". Almost all your suggestions have been incorporated into the revised paper with the major revises highlighted. In the following, we will give an item by item response to your comments.

Best wishes. Qianshan He

Referee report Summary: This paper examines lidar measurements at a meteorological station over the Tibetan Plateau during August 2011. A distinct aerosol layer in the UTLS above the tropopause is observed. The temporal variations of the aerosol layers are found to be correlated with the changes of deep convection strength. The authors conclude that deep convective transport is the primary mechanism for the enhancement of aerosols in the UTLS over the Tibetan Plateau. While the suggested mechanism is very plausible, the supporting evidence based on very limited measurements does not substantiate the conclusion. I suggest the authors address my major concerns listed below before a publication can be considered. 1. Quality of the aerosol measurements by the Micro Pulse Lidar: A comparison of the surface lidar measurements with NASA CALIPSO measurements will help to validate your observations. R: The reviewer's comments are very valuable. A new figure (Fig.2) and more statement are added in the manuscript (the second paragraph in Section 3) to compares the average extinction coefficient profile of MPL with that of CALIOP with the detailed CALIOP explain added in Section 2.3.

2. The correlation with convective indices: the authors used Tropopause temperature and OLR as two indices for deep convection strength. However, the samples are so limited ($\sim$12) that the correlations do not appear robust. For example, Figure 4 breaks up the 12 samples into two groups. The two separate correlations are high but the overall correlation for all 12 cases is very low. The authors explained the separation as two convective events although no evidence is provided how these two events are different. I suggest additional analysis of surface emission and low and mid-tropospheric

cloud and aerosol conditions for the two events should be conducted. R: We appreciated the reviewer's suggestions. We employ the daily variation in plateau monsoon index (PMI) to analyze the Tibetan Plateau monsoon variation, which is an indicator of the daily mean intensity of the Tibetan Plateau monsoon, and find that the two stages might be caused by the different circulation systems due to an apparent time interval of about 10 days with PMI undergoing a substantial oscillation, as shown in Fig.3. More explains about relation of the aerosol layer in UTLS with PMI are also added in the 3rd and 4th paragraph in Section 3.

3. The hourly variations of nigh-time aerosols: the authors showed that the night-time aerosols peak around mid-night and claimed this is a further proof of convective influence on the UTLS aerosols. I found this quite speculative. The paper by Nesbitt and Ziper (2003) discussed the development of MCS through the night over ocean. Over land, rainfall cycle shows a maximum in the afternoon and a slowly decreasing trend through midnight. I suggest the authors look for further evidence of convective intensity change at night time. A possible source is the 3-hourly brightness temperature data from ISCCP, which will provide a diurnal variation of convective strength. TRMM precipitation may be useful, too, but I am afraid some deep convection systems do not produce rainfall over the Tibet. R: We completely agree with reviewer's concerns. Indeed, the conclusion of the night-time aerosols peak around mid-night with only several documents support is deficient and farfetched. According to the reviewer's suggestion, we try to collect the 3-hourly brightness temperature data from ISCCP to look for further evidence of convective intensity change at night time, but find the data are only available before June 2008. Therefore, we decide to remove this paragraph and corresponding figure finally. More frequent sondes are planned to launch in the coming field experiment, which aim to provide more useful information about diurnal variation of convective strength.

4. To discount the other mechanism regarding the enlargement of pre-existing aqueous solution droplets, the authors imply there is a threshold value of water vapor for such

aerosol growth. What is the threshold value of water vapor? The Vasisala radiosondes have a known low bias of water vapor mixing ratio in the UT. They don't provide a rigorous test of the "growth" theory. R: We appreciated the reviewer's suggestions. A further explain about the threshold value of water vapor is added to the manuscript, as follow, According to the calculated growth curves of liquid solutions as a function of temperature and water vapor, the high H2O mixing ratios (more than 5 ppmv) are indispensable condition for producing high concentrations of fine particles near the tropopause. Additionally, more discusses about the accuracy of water vapor mixing ratio from Vasisala radiosondes are added in the Section 2.2 of this manuscript. We compare RS92 RH measurements with simultaneous water vapor measurements from CFH on 13 August 2011. After applying the time-lag and solar radiation bias corrections, corrected RS92 RH measurements show agreement with CFH in the troposphere.

The paper is generally well written, but there are a number of grammar errors, which I will defer to the second review if a revised manuscript is submitted and all the major concerns are addressed adequately. R: We appreciated the reviewer's suggestions and endeavor. We have improved the presentation through the manuscript.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C2300/2014/acpd-14-C2300-2014-supplement.pdf

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