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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of Asian Summer Monsoon Anticyclone on the Trace gases and Aerosols over Indian region" *by* Ghouse Basha et al.

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Reply to Reviewer#2 Comments/Suggestions

The paper describes the characteristics of trace gases, aerosols, and tropopause parameters during the mature phase of the Asian summer monsoon in July and August within the Asian summer monsoon anticyclone (ASMA) for the time period from 2006 to 2016. The mixing ratios for the trace gases CO, O3, and water vapour are taken from the MLS measurements of the Aqua spacecraft. The attenuated scattering ratio of aerosols is taken from the CALIPSO satellite. The tropopause parameters are identified from COSMIC GPS RO satellite measurements. The inter-annual variability

of trace gases, aerosols, and tropopause parameters in the ASMA region is related to the intensity of the Asian summer monsoon season, active and break spells of the the monsoon, the phases of ENSO (La Niña and El Niño) and the phases of the QBO.

Reply: First of all we wish to thank the reviewer for handling this manuscript and for offering constructive comments/suggestions, which improved the manuscript content significantly. In the revised version, we have taken care of the reviewer's comments/suggestions, and suggestions are incorporated in the text and we hope the reviewer will find the revised version satisfactory. As per the reviewer suggestion, the methodology part and structure of the manuscript is changed.

1 General comment The paper needs major revisions. In general, the topic of the study is interesting and there has been a lot of activity in this field of research over recent years, but the authors miss to highlight which new insights can be gained from their study, that go beyond the already known facts. The descriptions given are often much to general and inexact. E.g., some statements (also too general) about the relationship between the phases of QBO and ENSO and trace gases, aerosols and tropopause parameters are not supported by the data shown in the Figures. Instead the authors refer to some additional analyses, which they do not show in the paper. There are statements in the manuscript about statistical significance. But there are no results of any statistical test included in the figures. There is no statement in the manuscript about the test method that has been applied for the statistical significance. Have the authors applied a statistical test? If yes, the results should be included in the figures. If not, the statements about significant influences need to be rewritten.

Reply: In the revised version of the manuscript, we have incorporated a detailed analysis regarding the relation between tracers and ENSO, QBO with linear regression analysis. We have removed the discussion regarding the EMD analysis and other similar parts in the text. The statistical significance is determined by the paired sample t-test.

The description of the methodology raises more questions than it can give answers. Are the authors sure about the method they have used? Reply: We have rewritten the whole methodology section to the point as per the analysis.

The manuscript still needs to be edited thoroughly. The writing style is still poor, even after some corrections that have been included after the access review for ACPD.

Reply: In the revised version, we have taken the utmost care in editing the manuscript.

2 Specific comments Lines 78–81: As a motivation for the study you mention to bridge a gap. It is not clear which gap you want to bridge with the analyses presented. Open questions should be formulated clearly and also the contributions of the study to answer these open questions need to be stated more clearly.

Reply: We have clearly mentioned the motivation of our study and science questions in the revised version.

The section about the methodology (lines 154–165) needs to be rewritten. The method of multivariate regression analysis, which is stated to have been used, is not reflected in the equation given in the manuscript. The equation given describes a sort of multiple linear regression. The two studies from Diallo et al., listed as references, both do not use the method of multivariate linear regression. How can the reader find some more details about the method and further applications of it in these references? The coefficients a1(t) to a8(t) are described as time dependent what they certainly are not. There is a statement in the summary and conclusion section (The deseasonalized time series subjected to multivariate regression analysis shows ...), which indicates that the time series have been deseasonalized prior to the regression analysis. However, the equation given in the text includes terms of pairs of sine and cosine functions, to account for the seasonal cycle. What method was actually used?

Reply: We apologize for the mistake. The methodology used is mentioned below.

'To identify the contributions of QBO and ENSO in the tropopause parameters, trac-

ers, and aerosol measurements, complete data are subjected to the multiple linear regression analysis. This well-established method considers the relative influence of the considered climate indices on tropospheric variability. The regression technique is expressed as: y(t) = a1 QBO(t) + a2 ENSO(t) + c; t = 1,n where a1 and a2 are the regression coefficients, QBO and ENSO are the normalized QBO time series represented by the biennial modulations of the monthly mean zonal winds at 30 hPa measured by radiosonde in Singapore and normalized Niño 3.4 (ENSO) index, c is the random noise and residual 'c'. For more details about the regression technique and its further applications see Kunze et al. (2016). But unfortunately, the results obtained with this methodology are not significant hence we have removed this part completely in the revised version.

Figure 5. Time series anomalies of (a) tropopause altitude, (b) tropopause temperature obtained from COSMIC, (c) WV, (d) O3, (e) CO at 100 hPa obtained from MLS satellite observations and (e) ASR obtained from CALIPSO (16-18 km averaged) subjected to multiple linear regression based on QBO proxies in ASMA region from 2006 to 2016. The resulting fits are shown here (blue line)

Figure 6. Time series anomalies of (a) tropopause altitude, (b) tropopause temperature obtained from COSMIC, (c) WV, (d) O3, (e) CO at 100 hPa obtained from MLS satellite observations and (e) ASR obtained from CALIPSO (16-18 km averaged) subjected to multiple linear regression based on Niño 3.4 index in ASMA region from 2006 to 2016. The resulting fits are shown here (blue line).

The description of the tropopause temperature in Figure 2 (lines 181–182) ignores the fact that the lowest tropopause temperatures are located south of the ASMA (i.e. outside the ASMA). The increase in tropopause altitude within the ASMA does not coincide with the lowest tropopause temperatures. The authors have to be more exact in describing the ASMA features.

Reply: Thank you for raising this point. We have incorporated this into the text. Also,

we have described the exact features of the figure in the revised manuscript.

Lines 271–287: The discussion of Figure 5 and 6 is too superficial, by stating “The resulting data captures the inter-annual variability of all parameters in the anticyclone region as shown in Figure 5 and 6.” (cf. line 272–273). How much (in %) of the inter-annual variability is captured by QBO (Figure 5) and ENSO (Figure 6)? The time series shown in both Figures are not very convincing, as there are some periods where it is hard to see any relationship. The authors state “The influence of the QBO is seen clearly in the anticyclone region.” and mention a lag–lead difference between the QBO time series and the parameters averaged for the ASMA region, but they do not show any of these results in the paper. The authors mention some more sophisticated statistical methods (EMD, LSP) but do not show any results of these additional analyses.

Reply: As mentioned in the previous reply, we have removed the figures 5 and 6 and related discussion. We have also deleted the text related to EMD analysis.

The SOI is given as a time series to describe the ENSO variability in Figure 6. Why do you eventually use the SOI instead of the Niño3.4 index, which has been used in the multivariate regression analysis? I suppose the Niño3.4 index was also used to get the time series of the fit (blue line) in Figure 6. To get an estimate on the relation between ENSO and the quantities averaged in region of the ASMA you have to use the same ENSO index as was used in the regression analysis. Or, another possibility, you have used the SOI in the multivariate regression analysis and the description of the method is incorrect. On the other hand, you mention to use the multivariate ENSO index (MEI) in the introduction section. What kind of ENSO index was actually used?

Reply: As mentioned in the previous reply, we have removed these figures and related discussion in the revised version.

The volcanic eruption of the year 2011 has had a large impact on the ASR in the ASMA region (Figure 4). This year is included in your composite analysis of weak (2014, 2015) and strong monsoon years (2010, 2011, 2013), shown in Figure 8. How much

of the composite differences of tracer mixing ratios, ASR, and tropopause properties are due to this single volcanic eruption? How robust are the results? Do you have similar anomalies when you omit the year 2011 from the composite analysis in Figure 8? Reply: Thank you raising important point. As per reviewer suggestion we have plotted the difference between strong and weak monsoon years by including (Figure R1) and excluding (Figure R2) 2011 year. Quite different features we have noticed by excluding the year 2011 in spatial difference of strong and weak monsoon years in all the parameters. We have included the figure R2 (as Figure S1) in supplementary material in the revised version and discussed this aspect in the manuscript.

Figure R1: Spatial difference between strong and weak monsoon years. For strong monsoon years, we have included the year 2011.

Figure R2: Spatial difference between strong and weak monsoon year. The years included for strong monsoon years are 2010, 2013 subtracted from 2010 and 2013. We have excluded the year 2011.

There are composite differences for ENSO phases (La Niña – El Niño) shown in Figure 9, where the El Niño composite consists of only one event (2015). The year 2015 is also classified as a weak monsoon year, which may be caused by the El Niño event in that year. This interference is not at all discussed in the text. Why do the authors not show the results of their multivariate regression analysis with respect to ENSO, instead of showing ENSO composite differences? Reply: We agree with the reviewer comment. The year 2015 corresponds to strong El Niño event (https://ggweather.com/enso/oni.htm) and corresponds to week monsoon year and this point was added in the revised version. In the whole analysis of this manuscript, we have removed the multivariate regression analysis.

The last part of the summary and conclusion section (lines 374–394) is more a review of results already published than a summary and discussion of the results shown in the previous sections. The authors should stay focused on their own results and

they should discuss these in comparison to previous studies. E. g., it would be worth discussing the increase in water vapour mixing ratios within the ASMA region during strong monsoon years in comparison to the study of Randel et al. (2015), who state "Strong subseasonal variations in stratospheric water vapor are closely linked with deep convection in the monsoon regions, with the surprising result that stronger convection leads to a relatively dry stratosphere and weaker convection to a wetter stratosphere."

Reply: One of the referees suggested to make a summary and conclusion part as points. Therefore we have changed it before it came for discussion. In the revised version of the manuscript, we made a summary and conclusion by focusing on our results and comparing it with previous studies as suggested.

A kind of outlook is given in the final part (lines 394–400). These two sentences are confusing. You state:"... aerosol effects on the monsoon water cycle may be important in years when the influence from the other controlling factors which will additionally affect the ASMA region.". Something seems to be missing here, as in the current form this sentence does not make any sense. Also the final sentence is incomplete and needs to be rewritten. As I already have mentioned in the access review, how do you know about a significant increases in heating rates, when the analysis has to be carried out in the future?

Reply: In the revised version, we have re-written these statements with better clarity. 'As the tropopause temperature, WV, O3, and aerosols shows distinct features in ASMA the estimation of radiative forcing will have a significant impact in this region which will be studied further'. 'The increase in ASR during strong monsoon years can alter the radiation pattern in ASMA. The increase/decrease in radiation can significant changes the heat budget of the surrounding areas which will be carried out in future'

3 Technical corrections Throughout the paper there are still numerous formal issues, which I do not list here explicitly, as the text is expected to change a lot in the revision.

The authors should read their final text carefully, or engage professional editing and proof reading before the next submission. Reply: We have taken utmost care in revising the manuscript and try to reduce the grammatical mistakes and typos to the maximum possible extent.

Several times the tracers are listed as: WVMR, O3, CO ... In these cases it would be better to write: WVMR, O3, and CO mixing ratios ...

Reply: In the revised version, we have replaced WVMR with WV throughout text. In data section 2.2, we have mentioned the statement as 'The WV, O3 and CO values are taken as mixing ratios in this paper'.

Figure 3, caption, better write: Time–height sections of monthly mean climatologies of (a) ...

Reply: Changed as per reviewer suggestion

Figure 4: The x-axis of the figure seems to be wrong. Currently it is given as: Months (Jul.2006–Dec.2016); with time indices starting with 1 and incrementing by 12. The x-axis should be: Months (Jan.2006–Dec.2016).

Reply: In the revised version, we have changed the X-axis with year 2006-2016 and x-label Months (Jan.2006–Dec.2016).

Captions of Figure 7, 8, and 9 start with: Spatial mean ... Better write: Longitude, latitude section of the ... Reply: Changed as per reviewer suggestion.

Once again, we would like to thank the Reviewer for his/her thoughtful comments and suggestions that led to substantial improvements in the revised manuscript.

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-743, 2019.

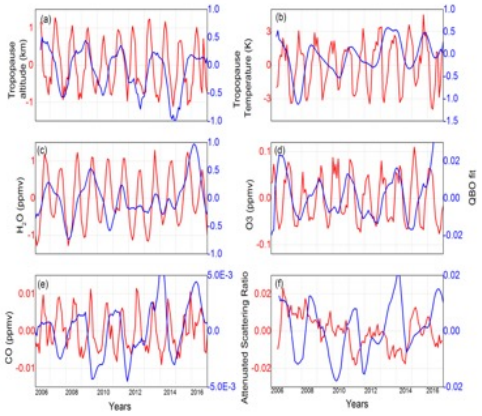

Fig. 1. Figure 5. Time series anomalies of (a) tropopause altitude, (b) tropopause temperature obtained from COSMIC, (c) WV, (d) O3, (e) CO at 100 hPa obtained from MLS satellite observations and (e) ASR

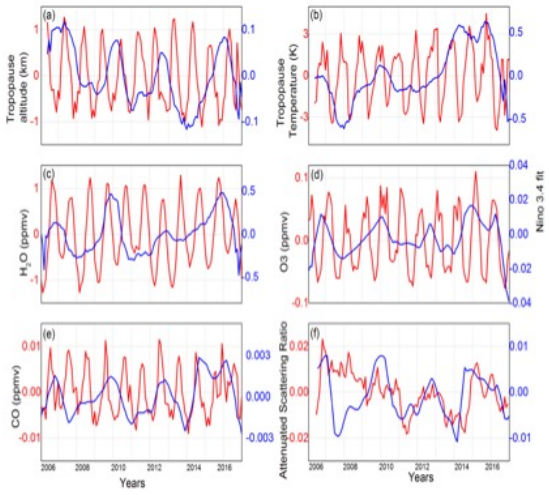

Fig. 2. Figure 6. Time series anomalies of (a) tropopause altitude, (b) tropopause temperature obtained from COSMIC, (c) WV, (d) O3, (e) CO at 100 hPa obtained from MLS satellite observations and (e) ASR

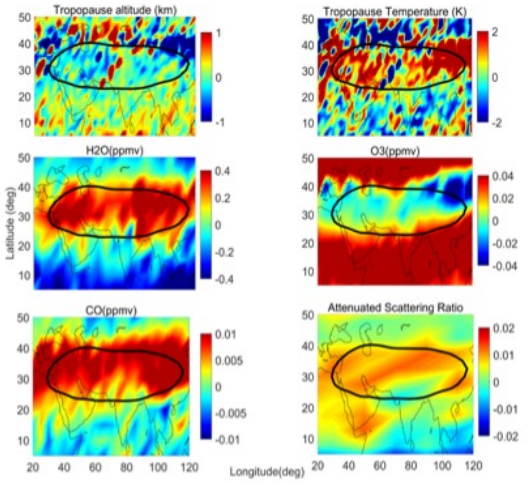

Fig. 3. Figure R1: Spatial difference between strong and weak monsoon years. For strong monsoon years, we have included the year 2011.

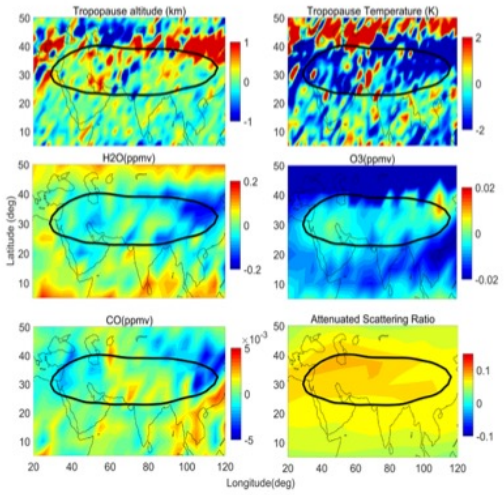

Fig. 4. Figure R2: Spatial difference between strong and weak monsoon year. The years included for strong monsoon years are 2010, 2013 subtracted from 2010 and 2013. We have excluded the year 2011.