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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Upper tropospheric CO and O_3 budget during the Asian Summer Monsoon" *by* B. Barret et al.

Anonymous Referee #2

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General comment: This paper addresses the chemical budgets of ozone and CO during the Asian monsoon, based on a comparison between observations from IASI, IAGOS and model simulations. Particular focus is laid on the budget within the upper tropospheric monsoon anticyclone and effects on the buildup of the tropospheric ozone enhancement over the Middle East. Sensitivity simulations are used to isolate the effects of different source regions. For CO as well as for ozone (via NOx-precursors) South Asia is found to have a stronger contribution on the composition of the anticyclone, compared to emissions from East Asia, but lightning-NOx is identified as the largest contributor to ozone formation. The tropospheric ozone maximum over the Middle East is related to downwelling ozone fluxes to the west of the monsoon.

Overall, the paper is well written and addresses a topic of interest for the ACP-readership. In the following, I have two major and several minor comments which

should be considered before publication.

Major comments:

1) Asian monsoon boundary:

My first main concern is related to the separation between the monsoon anticyclone interior and its surroundings, based on geopotential height, as used in this paper. I personally think that PV would be a more suitable quantity describing the confinement of air masses in the anticyclone. At least, there are some recent papers showing that trace gas contours in the anticyclone align more closely with PV than geopotential, and that enhanced PV-gradients even indicate the existence of a transport barrier (e.g., Garny and Randel, 2013; Ploeger et al., 2015; Garny and Randel, 2015).

Presumably, the results presented here are not very sensitive to the usage of either GH or PV, as values are always calculated for the whole anticyclone (e.g., Table 3) and differences in the total area (defined by either PV or GH) are not very large. However, it would be nice to have some sensitivity study quantifying the uncertainty due to using either GH or PV. At least a thorough discussion about defining the anticyclone boundary and a reasoning why a geopotential anomaly is used here, should be included. (There are already some related text parts in Sect. 4.1, but these could be extended).

A related question is: Have daily GH fields been used for defining the anticyclone and calculating the fractions (e.g., Table 3), or the monthly mean as plotted in the figures? I would strongly suggest to do the latter, if this has not already been done.

2) Middle East ozone maximum:

I'm confused about the discussion concerning the buildup of the ozone maximum over the Middle East. It is concluded (e.g., Sect. 4.2) that this ozone maximum is largely related to downward flux from the Asian monsoon. However, Fig. 7 shows that outflow from the monsoon in the upper troposphere to the west during July–September lowers ozone mixing ratios at levels between 400–150hPa over the Middle East (e.g., Fig. 7j).

Below, at levels of 700–400 hPa, ozone mixing ratios remain high. In my opinion, these higher values are related to African LiNOx and STE and not to transport from within the anticyclone, as Fig. 13 shows. I think the reasoning here needs to be clarified.

Minor comments:

P2, L36: I wouldn't consider the Asian monsoon as an extra-tropical phenomenon, but rather subtropical or even tropical (e.g., Fueglistaler et al., 2009).

P3, L1: How is this fact (*...convection from the Tibetan Plateau, highlighted as predominant to fill the AMA*) related to the recent analysis by Tissier et al. (2015), stating that "the Tibetan plateau ... (is) a minor overall contributor..."?

P3, L78: *...high altitude...* Which altitude?

P14, 476: *These combined effects...* I think the dominant effect causing the low ozone anomaly in the Asian monsoon is vertical transport. Models without tropospheric ozone chemistry included do a reasonably good job in simulating the low ozone concentrations in the monsoon anticyclone (e.g., Konopka et al., 2010).

P15, 495: Figure 10 shows upward mass fluxes at both peaks in ozone pruduction rates (also for the western one). Therefore, I would say *...the double maximum...is associated with a double peak structure in upward mass flux.*

Fig. 14 / and related discussion: The fact that South Asian emissions fill out the anticyclone whereas East Asian emissions are transported around seems very consistent with the findings of Vogel et al. (2015) regarding the transport from various surface regions to the anticyclone.

Technical corrections:

Title: O3 → O_3

P4, L113: (Clerbaux et al. (2009)) → (Clerbaux et al., 2009)
Similar wrong bracketing occurs several times throughout the paper (e.g., P4/L126, ...).

P6, L182: no number before °S.

P7, L219: 10S° → 10°S

P8, L259: Here, the latitude band is defined to be 21–29°N, while in the figure caption it is written to be 23–29°N. This discrepancy occurs also later in the paper several times.

P8, L260: correspondS

P10, L312: Should be *equation (1)*.

P11, L362: highlights → highlight

P11, L378: Shouldn't this read *...eastern part...*?

P14, L453: thann → than

P14, L470: ...convective uplift of...

P14, L471: resultS

Fig. 9 / caption: I guess the level for the mass flux contour should read 225 hPa.

References:

Fueglistaler et al. (2009), Rev. Geophys., 47, RG1004.

Garny and Randel (2013), J. Geophys. Res, 118, 13421–13433.

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Ploeger et al. (2015), Atmos. Chem. Phys., 15, 13145–13159.

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Tissier et al. (2015), Atmos. Chem. Phys. Discuss., 15, 26231–26271.

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