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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Dynamics and composition of the Asian summer monsoon anticyclone" *by* Klaus-D. Gottschaldt et al.

Anonymous Referee #1

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Review of "Dynamics and composition in the Asian monsoon anticyclone" by K.D. Gottschaldt et al.

This paper is dedicated to the better characterization of the different processes (STE, convective uplift, dynamics, photochemistry...) that control the composition and specifically the O3 distribution in the Asian Summer Monsoon Anticyclone (ASMA). It is based on (i) in-situ observations taken on board the HALO research aircraft during the ESMVal field experiment in September 2012 to document one particular case (ii) multi-annual simulations from the EMAC global chemistry climate model to document intra seasonal variabilities. The ASMA is a major feature of the Northern Hemispheric atmosphere during the monsoon season and its composition controls large parts of the tropospheric composition (such as over the Pacific and Middle East) during the mon-

soon season. The paper presents observations and model simulations that provide new insights about the contribution of different processes to the O3 distributions in the ASMA. It is well written and well organized and its argumentation is based on a robust methodology. I therefore recommend this paper for publication after a few important comments (one about the presentation of the results and one about LiNOx) and a number of rather minor comments are taken into account.

Major comments:

Presentation: - parts of the paper are too long and divert the reader from the main and strong elements brought by the paper. In particular, some rather general and introductory statements are given all along the manuscript (about the emissions, chemistry and dynamics...). It could be good if the authors try to shorten the paper and keep introductory elements to the introduction. Some examples of lengthy parts are mentioned in the detailed comments. - the paper is very often referencing to results from its accompanying paper which makes the reading and understanding somehow difficult. One example about O3 production regime is given below. - the same is true concerning the supplementary material which makes the paper a bit heavy to handle.

LiNOx and O3 production: Fig. 3h displays higher LiNOx production from EMAC in the Tibetan part of the ASMA during spring than during summer as mentioned P10L4-5. Nevertheless, the net O3 production is larger in summer than in spring down to 200 hPa below the tropopause (Fig. 2h). The authors explanation is that (i) in spring lower COV are uplifted by convection resulting in COV limitation and reduced O3 production (ii) LiNOx are produced locally in spring and not in summer. The latest argument also appears in the Annexe about LiNOx (p20L14-16).

Concerning (i) 1/ LiNOx production is linked to deep convection, especially in the models where both parametrization are coupled (in EMAC flashes are linked to convective updraught velocity as mentioned P20L10-11). Therefore more LiNOx should be associated with larger uplift of pollutants. Why EMAC displays more LiNOx with less uplifted

COV in spring ? 2/ Over South and East Asia the season of largest deep convection takes place in summer during the monsoon rather than in spring. Why are there more LiNOx in spring in EMAC ?

Concerning (ii): Looking at Fig. A1 displaying monthly LiNOx at 168 hPa, we see that they are localized over NW and NE India and Pakistan in May while in August they are more over SE India and Himalaya/Tibet. Nevertheless, the source is much stronger in spring. In both cases, the LiNOx emissions are very "patchy" and localized with also in June a single large emission spot over Bangladesh and in July the LiNOx spot localized over northern central India. Therefore, it is difficult to attribute a lower O3 production to more localized LiNOX emissions in spring. Why are the LiNOx emissions so "patchy" on a monthly scale? The averaging should smooth horizontally the distributions because convection does not always occur at the same place. It could be interesting to compare LIS/OTD distributions of lightnings to EMAC LiNOx distributions.

Finally, in Barret et al. (2016) the LiNOx are not shown but a sensitivity test shows that O3 and NOx produced by LiNOx are the highest during the monsoon season which seems rather logical for the reasons discussed above. This discrepancy between the EMAC and GEOS-Chem models concerning LiNOx should be discussed.

The east-west O3 net production gradient is logical as explained in the manuscript (P10L11-12) and in agreement with previous comparable evaluations by e.g. Liu et al. (JGR,2009) and Barret et al. (ACP,2016). Furthermore, the values of Fig. 2 seems in rather good agreement with those of the above mentioned papers for the monsoon season. A comparison and discussion of the EMAC O3 production with these previous studies could be interesting to strengthen and put the results in perspective.

Referring to the accompanying paper, it is mentioned that O3 production at 168 hPa is rather limited by CO than by NOx (P15L12-13). The exact sentence in the accompanying paper is "Net O3 production seems to depend more on CO (and related precursors) than on Nox". The use of "seems" shows that the authors are rather uncertain. I do not

really understand how is this possible because of the rather high CO concentrations within the ASMA (70-100 ppbv according to HALO and all references cited in the paper). This statement of a generally CO-limited regime in the ASMA at 168 hPa needs to be demonstrated and is not supported by the literature. For instance, according to Brune (IGAC Isuue 21, 2000), in the upper troposphere, the O3 regime is NOx limited for NOx concentrations lower than some hundreds pptv.

Tracer-tracer relationships:

This part is very interesting because the 3 tracers document different transport and chemical processes. HCl in particular which is rarely used is good to trace stratospheric air because O3 is photochemically produced in the troposphere.

The authors explain that "mixing line with negative CO/O3 slope dominate" in Fig 4c (corresponding to positive slopes in Fig. 5c). They correspond to mixing stratospheric air with photochemically processed tropospheric air. Neverthelesse, in Fig. 5c, we also see some horizontal mixing lines (red and green). According to the discussion in p11 and 12 they correspond to mixing of fresh uplifted pollution (increasing CO, horizontal lines not so clear in Fig. 4c) and stratospheric air (increasing HCl in Fig 5c) with antagonist effects on O3. Another line with O3 decrease / HCl increase in Fig. 5c and O3 decrease / CO increase in Fig. 4c corresponding to mixing of fresh pollution in the UTLS can also be isolated. It is difficult to see whether these mixing lines correspond to important part of the sampled air masses but they could be mentioned.

Details: Part 3: the description of the different species at beginning of §3.1; 3.2, 3.3 and 3.4 (origin, chemistry etc.) are too close to "textbook" descriptions and should be shorten for readability. Part 5: this part tries to describe the different processes that control the ASMA composition one by one. It is interesting and well documented but rather lengthy and descriptive. For instance, the description of the evolution of the CO and HCL distributions P17L1-15 is very detailed and could be summarized. Fig2 and 3: the plots are shown in pressure coordinates that makes the region around the

tropopause very compact. Readability would be better in logP coordinates (altitude plots are provided in the supplement but it makes the reading uncomfortable and could be simply removed). p5l13: why choose PV = 3.5 for the tropopause in the extratropics ? Most studies choose 2 or 1.5. P5l27: Fig.1 is referenced after Fig 2 and 3. p9l5: “indicates that relatively. . .” P17L1-14: the dynamics are very detailed with the evolution of the air masses but is it really necessary to give so much details?

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