

Interactive comment on “Tropospheric ozone maxima observed over the Arabian Sea during the pre-monsoon” by Jia Jia et al.

Anonymous Referee #3

Received and published: 5 January 2017

This work describes variations in the tropospheric ozone column over the Arabian Sea using satellite data (SCIAMACHY, OMI/MLS, TES), MACC reanalysis data, MOZART-4 results & HYSPLIT trajectories and indicates that spring time ozone maximum over a region in the Arabian sea is largely (50% in 0-4 km and 20% in 4-8 km) due to LRT from India apart from some contribution from Middle East, Africa and Europe.

Major findings are based on MACC reanalysis data and MOZART-4 output (Hou et al. 2014). Quantitative contributions from different regions are mentioned, where detailed explanation and in-depth analysis for these regions was expected. It has been mentioned (page 2, line 20) that spring time maximum ozone is not unique but rather well-known. It has also been confirmed by other studies (line 32) that this is due to LRT from Middle East, Western India, Africa etc. Therefore, it was expected that this work should have provided some additional knowledge with more quantitative analysis.

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There are significant differences in spatial distribution among SCIAMACHY, OMI/MLS and MACC reanalysis data over the Arabian Sea during spring. In my opinion, few of my below comments might be helpful in making it a better manuscript.

1) Introduction, Page 2, 2nd para – Enhancement in ozone over the Arabian Sea is not prominent. It would be better to include some discussion on enhancement in ozone over northern India. Additionally, higher ozone could also be seen over the Bay of Bengal in SCIAMACHY but not in OMI/MLS, any reason for it!

2) Section 3: CO and NO₂ are ozone precursors and springtime CO column (Fig 2) values are observed to be much higher in the Bay of Bengal region than over the Arabian Sea. NO₂ is also seen to be similar over AS and BoB. Any comment !!! Additionally, CO is also higher in southern region of the Arabian Sea and southwest boundaries of India. But higher ozone (particularly SCIAMACHY) is seen in the northern part of the Arabian Sea and close to Oman, Yemen, Pakistan, etc. Why ozone is not higher close to western coast of India? Explanation should be added in this regard. Considering these facts, source of ozone maximum should be discussed.

3) Section 3: Figure 2 is for year 2008 and Figure 4 is for years 2006 and 2010. It will be good to add results for year 2008 in Figure 4 or make figure 2 for years 2006 or 2010. A significant difference in spatial distribution is seen over AS during spring/April. Figure 2 shows higher ozone (SCIAMACHY) having proximity to western region of AS, while this is not seen in Fig 4. It would be better to add monthly variations from MACC reanalysis (TOC, 0-4 km, 4-8 km, 8-12 km, 12-18 km) in a separate panel of figure 3.

4) Section 4.2: Line 10 “...CO and TOC are highly correlated ...” please clarify. This is in contradiction with statement at page 5 line 20 (“... CO and NO₂ show a different ...”).

Figure 6: Backward trajectories (mainly 2 and 4 km) are from southern India or some time from central region. Both these regions are showing much lower levels of ozone than those over northern India (Fig 1, 2, and 4).

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Page 9, Line 14: Is this statement based on backward air trajectory of 15 km (Fig 6) alone or are there other supporting evidences? Lines 15-16 are also very qualitative.

Page 11, Line 7: Considering the broad region, it is better to name it as “South Asia”. It would be beneficial to the reader to give some details on methodology adopted in estimating percentage contribution from different regions (Page 12). What/how was the background/reference levels considered while making these estimates?

5) Section 4.4: It would be worthwhile to list down the contribution from STE in the AS region. As mentioned that it is comparable with that of ‘Euro_FT’, which is significant (17%, 29%, 32%, and 31%).

Minor comments TPH information can be given in the caption of figure 4.

Referencing of figure needs to be in sequence. Figure 12 (@ page 10) is referred after figure 7 (@ page 9).

Hou et al. (2014) used MOZART-4 model run during 2000-2007, however this study used 1997-2007 model run. Please mention it explicitly if a separate run is made for this study.

Page 13: Change “Fig 6.11” to “Fig 10”

Conclusion: Line 12: Reference of Southern hemisphere biomass burning is appearing not to be relevant here. This has also been referred in the main text without much relevance.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-786, 2016.

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