Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2015-958-RC3, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric column ozone response to ENSO in GEOS-5 assimilation of OMI and MLS ozone data" *by* M. A. Olsen et al.

Anonymous Referee #1

Received and published: 15 March 2016

This study use GEOS-5 analysis of OMI and MLS ozone observations to examine the magnitude and spatial distribution of the ENSO influence on tropospheric column ozone in the tropics and the mid-latitudes. Overall, the results are a nice contribution to the understanding of the connection between ENSO teleconnection and tropospheric ozone variability, although the time period analyzed in the study is quite short (9 years) in a climate standard. The manuscript is within the scope of ACP. However, there are a number of issues in the current manuscript as outlined in my review below. The referee cannot recommend publication of the paper in ACP unless the authors take serious attempt to address these comments in a revised manuscript.

Major comments:

1. Throughout discussions in the manuscript, particularly in the Introduction section

reviewing previous work on the extratropical trop. ozone response to ENSO (Lines 56 – 85), the discussions will be more clear if you could add information on the data and time period analyzed in each study. It is known that the different time periods or the number of El Nino or La Nina events included in the analysis often gives very different correlation results given the large internal variability of the mid-latitude atmosphere. For example, Langford et al. (1998, 1999) noted positive correlations between mid-tropospheric and lower-stratospheric ozone observed at Fritz Peak, Colorado during 1994–1998 (without La Niña years), reflecting higher than neutral ozone levels during the El Niño events of 1994–1995 (weak) and 1997–1998 (strong). Lin et al. (2015, Nature Communications) finds that their model captures the observed relationship (r2=0.69) for this short record, but when the entire 1990–2012 period (including both El Niño and La Niña years) is considered, the model indicates little correlation (r2=0.18) between mid-tropospheric and lower-stratospheric ozone over the western US. An extension of the Fritz Peak record to 1999 shows that the mid-tropospheric ozone anomaly in April–May is higher following the La Niña winter of 1998–1999 than in either El Niño or neutral conditions (black circles in Fig. 6c of Lin et al., Nature Communications).

By adding the information on the time period and data used, the readers of the paper could get a sense of how robust the results are.

Throughout the manuscript,the authors tend to contrast their analysis with prior work using shorter records, but not with the recent papers that have examined the mechanisms controlling the extratropical ozone sensitivity to ENSO events more carefully using longer observations and model simulations.

2. In the introduction, you should also discuss the findings of Lin M. et al. (2014, Nature Geoscience) and Neu J. et al. (2014, Nature Geoscience) and data used in their analysis. For instance, you could say:

"Using 40 years of ozone observations at Mauna Loa Observatory and a chemistry-climate model, Lin et al. (2014) identified a strong link between El Nino events and lower tropospheric ozone enhancements over the subtropical eastern Pacific in winter and spring. Lin et al. (2014) attribute this to the eastward extension and the equatorward shift of the subtropical jet stream during El Nino, which enhances the long-range transport of Asian pollution. Using mid-tropospheric ozone observations from TES during 2005-2010, Neu et al (2014) found . . . (http://www.nature.com/ngeo/journal/v7/n5/full/ngeo2138.html)"

3. Lines 175-177 and Figures 5, 7, and 8: According to your classification of ENSO events, there are only two El Nino events but five La Nina events. I speculate that this will affect the statistical power of the composite analysis shown in Figures 5-8. Can these events be really characterized as "strong" ENSO events? The boreal fall/winter of 2008/2009 included in your La Nina composite is not even classified as an ENSO event based on the +/- 0.5 threshold used by CPO (http://www.cpc.ncep.noaa.gov/products/analysis_monitoring/ensostuff/ensoyears.shtml)

4. Lines 230: It is not clear what you mean by "ground-based data". Ground-based data of what? UTLS ozone, mid-tropospheric ozone, lower tropospheric ozone, or surface ozone? The sensitivity of ozone to ENSO events can depend strongly on the vertical altitude as demonstrated previously by Lin et al. (2015) using Trinidad Head ozonesonde data and surface ozone observations over the western U.S., which should be also cited here.

Related to this comment, I also agree with the other reviewer that it would be very nice if you could illustrate and discuss show the sensitivity varies with the altitudes. These new results will be a very nice addition to the TCO sensitivity discussed in the current manuscript.

5. Lines 190-192 and Lines 203-206: It is not clear whether the ozone data is deseasonalized before correlating with the ENSO index. If not, the extent which the sensitivity reported in Figures 3 and 4 is influenced by by correlations on the seasonal time scale?

Please discuss.

6. Lines 251-253: This statement is not true. There are a number of recent studies have extensively examined the mechanisms by which ENSO impacts tropospheric ozone over the extratropical regions, i.e. Lin et al. (2014, 2015) and Neu et al. (2014).

7. Figure 10 and associated discussions in the text: It seems like that there is a substantial difference over the subtropical Northeast Pacific. It is surprising that the variance explained by ENSO over the subtropical Northeast Pacific is very weak in the longer record, but analysis of 40 years of observations at Mauna Loa reveals a strong ENSO signature in free tropospheric ozone over this region (Lin et al., 2014, Nature Geosci). Please discuss. Can you also show a comparison similar to Figure 10 but for the sensitivity shown in Figure 4?

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-958, 2016.