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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Tropospheric Ozone Seasonal and Long-term Variability as seen by lidar and surface measurements at the JPL-Table Mountain Facility, California" *by* M. J. Granados-Muñoz and T. Leblanc

Anonymous Referee #1

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Summary :

This paper by Maria Jose Granados-Munoz and Thierry Leblanc presents an interesting analysis of tropospheric ozone profiles over the JPL-Table Mountain Facility, California, based on 16 years of regular Lidar measurements and 2 years of co-located surface measurements. The manuscript spans different aspects of what such an extensive data set allows in terms of characteristics of ozone vertical distribution : diurnal and seasonal cycles, vertical variability from 2 to 20 km, interannual variability and

trends between 2000 and 2015, classification of air mass origins, and influence of tropopause folds.

Authors are right, such extensive ozone data set from JPL-TMF is "very valuable due to the rising interest in the detection of long-term trends in the Western United States . . ." as written in the introduction. Such extensive tropospheric ozone data set has not been presented in publication before and thus deserves a reference paper. The title, as well as the last sentences of the introduction are promising and gives expectations on the understanding of the observed long-term variability. However, the manuscript misses some diagnostics and solid conclusions to provide a thorough assessment to be used by a broad audience. It seems that the manuscript is a compilation of independent sub-sections and the primary objective of understanding the long-term vertical variability has been lost at the end. Subsection 3.5 in particular does not help at all in understanding the interannual variability. Similar remark applies for sub-section 3.4, which is also leading to unexpected conclusion on "No outstanding influence from Asia" as highlighted in the abstract. As a consequence, the discussion and concluding remarks sections do not appear robust enough at this stage of the manuscript. They are sometimes in contradiction with previous findings and let the reader with a promise of a following paper after additional analysis is performed. It's unfortunate.

Regarding the presentation of the manuscript, it is also not very comfortable to have to wait the discussion section to read conclusions and explanation on what have been observed and described in previous sub-sections. I would strongly recommend to include the different paragraphs of the section 4 in their associated sub-sections 3.

Undoubtedly, this manuscript is meant to be a valuable contribution, especially because it is an impressive long-term data set but I have major concerns regarding the robustness of some analysis and conclusions. Therefore, I will recommend publication only after (major and minor) revisions as detailed below are considered. Finally, I support what M.-Y. Lin has posted as an interactive comment and recommend the authors to also follow her recommendations.

Major comments :

-Abstract, line 53: This last sentence “No outstanding influence from Asia was identified” is quite surprising, in contradiction with previous studies, and raises issues on the robustness of the methodology. See further comments below.

- Line 99 : The characteristic “only surface or column-integrated measurements” does not apply to aircraft (MOZAIC/IAGOS) platforms. Zbinden et al., (2013) give a recent detailed description of ozone vertical profiles by MOZAIC/IAGOS over mostly documented airports by the program since 1995 (including Los Angeles airport). Logan et al., (2012) also used vertical profiles recorded by MOZAIC aircraft in addition to ozone sondes for deriving a global picture of ozone trends over Europe.

- Lines 167 to 177 : This paragraph as well as associated Figure 1 are questionable. It seems surprising to see on figure 1a that the difference is not varying with altitude. Given the argument from the last sentence of the paragraph that sampled air masses are not the same, one could expect a higher difference in regions with higher ozone variability (UTLS compared to free troposphere). It is not the case. Concluding the section with differences within +/- 15% raises the question on the regional representativeness of the JPL-TMF station. This paragraph deserves much more attention and details to give proof on the consistency between those two independent data sets (if that was the objective of such paragraph ?).

- Line 184 : This paragraph omits to give appropriate accuracy/error procedures of this analyzer and reference (at least from previous use of such type of instrument). Information on operating procedures (maintenance, calibration), or at least a reference paper would be appreciated.

- Line 241 : Figure 3a is not restricted to the tropospheric part of the ozone profile. Either change this adjective in the sentence or the figure. The title of the paper and of the sub-section is clearly on tropospheric ozone variability. I suggest to plot Figure 3 only for altitudes up to 15 km (on average) and modify the X axis for linear concentrations

on the tropospheric typical range [0-150 ppb]. In the seasonal part of the Figure 3, that will give the advantage to further see differences between the averaged seasonal vertical profiles. Is the spring profile that close to the summer profile ? From Figure 4, it seems that the spring profile should highlight higher ozone that the summer one.

- Lines 268-270 : The spring-summer maximum is indeed a common characteristic of northern mid-latitudes over Europe and North-America. However, it seems that these JPL-TMF data as shown in Figure 4 shows a clear spring maximum (at least April-May). This seems actually consistent with what Zbinden et al, (2013) have shown over Los Angeles airport based on MOZAIC data. This paragraph deserves further clarification to highlight this “local” characteristic of a clear spring maximum (if confirmed by Figure 3 plotted with a tropospheric X axis only). This special feature is likely the result of stronger influence from Asia in spring over the west coast of the US (Jaffe et al.2003; Parrish et al., 2004; Cooper et al., 2005; Neuman et al., 2012) as mentioned by Zbinden et al., (2013). Therefore, it is very surprising to read in the abstract that “No outstanding influence from Asia was identified”. I am not convinced by such statement. Further comments below regarding subsection 3.4.

- Lines 302-311 : As far as I know, the common rule for indicator of the statistical significance of the trends with such procedure is to use p-Values lower than 0.05 for trends with a confidence level larger than 95% (and not p larger than 0.1 and confidence level larger than 90%). I recommend the authors to rewrite tables 2 and 3 applying these criteria. It turns out that only spring (in the upper troposphere) and winter (from 4 to 10 km) show significant seasonal trends. It would also be nice to give indication of error in % in first part of Table 3 as done in Table 2.

- Lines 322-328 : This decrease in winter is the most surprising information of this paper. As far as I know, there is no other reference mentioning such significant decrease in winter in the region. In the recent review paper of Cooper et al., 2014 (and references therein including Cooper et al., (2012)), reported stations in California do show significant increase in winter. Therefore I recommend the authors to further argue and explain why such a different behavior at JPL-TMF station. This entire subsection misses further discussion on this trends analysis. See additional comments below regarding section 4.

- Sub-section 3.4 : This section is the weakest point of the draft paper. My major concern can be summarized as 2 questions : why (only) 8-days backward trajectories ? why (so many) 2-days of residence time over Asia ? These arbitrary choices need further arguments, sensitivity analysis and/or references for similar studies. From my knowledge, Cooper et al, (2010) used 15-days backward trajectories and concluded with a significant influence from Asia to the Western US region, especially in spring. To me, the length of the trajectories used here may be too short and the classification criteria is not adapted, to really assess the different influences authors are looking for. The two plots on Figure 8, bottom row, are too similar to trust in this methodology of classification. The criteria for air masses to be classified as ABL or AFT (at least 2 days over there) is too difficult to meet. What is the reason to impose 2 days over the continent to classify air parcels as "Asian" ? It is way too much. This is probably the reason why the Asian influence does not appear as strong as expected from previous studies. I guess that some (or most of) the air masses classified as Pacific (especially in Spring) would be classified Asian with a different criteria. Results and conclusions may be different with longer trajectories and different criteria for classification. For example, I would suggest to change the order of the sequential attributions : By default, air parcels that are not "stratosphere" would be classified as "Asian" unless the trajectories spend the entire period (8 days or longer) over the Ocean or over Central America. Maybe I'm wrong but I recommend the authors to revise this sub-section to make it convincing. Besides, an interesting information from this analysis of classified air masses would be to check if there is a tendency or anomalous behaviors from one year to another. If I understand well the title and the introduction, this subsection should help further understand the trend analysis. This is not the case so far.

- Subsection 3.5 : A figure showing an individual profiles (not averaged as in Fig.14)

with double tropopause would be good to further explain this characteristic to the non-expert reader. This section should make the distinction clearer between the wording "double tropopause" and "tropopause folds". It is not the same. Sentence line442-443 and legend of Figure 12 are ambiguous. Indeed, Randel et al., (2007) should be cited as a reference paper for characteristics of double tropopauses.

- Section 4 : This section is a bit too long. It starts like a summary but then includes analysis and explanations that would be better placed before in the associated sub-sections. More importantly, some highlighted discussions are mentioned as in contradiction with other studies without any further discussions. This needs to be further argued. For example :

- Line 477-484: Either the comparison with recent findings by Lin et al., (2015) and Neu et al., (2014) makes senses and the inconsistency raises important questions (i.e., what makes JPL-TMF station different and not representative of the general behavior ?), or the comparison is not possible (need to explain why ?) and such paragraph is simply to be removed.

- Lines 498-503 : This negative trend in winter observed at JPL-TMF is surprising, in contradiction with most of the studies I'm aware of, and therefore deserves further investigations. According to Cooper et al., (2012) only one site in the Western US shows negative trend in winter, and only for the 95% percentile. This is different from what is presented in Table 2 in this study. "Decrease in background ozone during these months . . . " is definitely an opposite conclusion to that of Cooper et al., (2012) and of similar recent studies. As for previous comment, I recommend the authors to answer the question "why do JPL-TMF measurements highlight different behaviors ?"

- Line 513-517 : Wouldn't it be good to have results from this extended analysis ?

Minor comments :

- Line 61 : "directly emitted" seems to me too ambiguous and may let the reader think that ozone is a primary pollutant. I suggest to replace by “transported down from the stratosphere”.

- Line 88-89 : I suggest to remove the end of the sentence starting at “which has not yet been . . .” because this not true, as written indeed in the following paragraph.

- Line 285-286 : Do R=0.34 or 0.44 really show correlation ?

- Line 287-290 : This is more interesting than the lines before and deserves a brief explanation. Why outliers have to be removed to confirm the correlation. Does this tell us something on specific process at the surface or at 4-6 km altitude? Is there different disconnected influences ? For a specific season ?

- Figure 5 : I am wondering if the choice of the color sale is the most appropriate to highlight (real and significantly positive or negative) anomalies. What about choosing a color scale centered on 0 (same color for -10 to +10) ? It is difficult to check consistency with Figure 6. Is there any explanation for long-lasting anomalies in 2003-2007 ?

- Line 341 : This reference is missing in the list.

- Line 412-414 : This sentence needs to be accompanied by a reference to give argument that an excess of 15 ppbv is what is expected as lightning-induced enhancement of ozone. Is Cooper et al. (2009) as mentioned in section 4 (line 537) relevant for this ?

- Line 429 : MERRA acronym needs to be explained. Reference would be nice.

- Reference list : The following references are incomplete : Ambrose et al., 2011; Cooper and Stohl, 2005; Lee and Akimoto, 1998; Monks, 2005; Petetin et al., 2015 (should check if ACP reference is available).

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