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Atmospheric Measurement Techniques Discussions

Interactive comment on "Trends of tropical tropospheric ozone from twenty years of European satellite measurements and perspectives for Sentinel-5 Precursor" *by* Klaus-Peter Heue et al.

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Author's reply to the interactive comment #2 on "Trends of tropical tropospheric ozone from twenty years of European satellite measurements and perspectives for Sentinel-5 Precursor" by Klaus-Peter Heue et al. Anonymous Referee #2
Received and published: 8 July 2016

We thank the referee for the comment, the interesting ideas and literature.

Overall this is a good paper on tropospheric ozone trends but it can be cleaned up a bit regarding clarity of sections with editing/typo fixes, etc. I suggest acceptance of the paper with some needed changes but generally minor as listed below:

The most important result from Beig and Singh [2007] is that they were able to detect decadal increases in tropospheric ozone over southern Asia for the TOMS record (1979-2005). In their analysis they were able to evaluate CCD measurements for 30S to 30N. For greater impact is it possible to extend your product to include higher northern latitudes and also evaluate Asia, but now for later time period? Perhaps averaging your measurements annually or seasonally each year when stratospheric ozone variability is small?

This would really be an interesting study; I will check in the near future whether it is possible to extent the latitude bands to $\pm 30^\circ$. *However, with respect to the uncertainty observed around* 20° *in the winter hemisphere, the results might be good only for the summer hemisphere (see below).*

Your trend results are very interesting, in particular Figures 8-9 showing the regional changes. Comparisons with other products show differences with these results, reflecting both differing time periods and also that all these decadal changes are pretty small and difficult to detect considering signal to noise. The increases you find in TTOC in the tropics through 2015 are not inconsistent to large extent with increases in global TTOC that have been reported the last few years in the BAMS State of the Climate Reports.

In the last BAMS state of the climate 2014 report, the global ($\pm 60\circ$) *increase was 1.9Tg/yr. With an average burden of 269 Tg this is an increase of 7.1% per decade. Our rate is slightly lower 3.4% per decade. But the increase may have latitudinal dependency and might hence be lower in the tropics compared to the data outside the tropics. The reference and a short comment are included in the introduction.*

In the introduction for Feng and Kobayashi [2009] you have numbers stated of 5% to 20% crop loss due to tropospheric ozone which is very large and seems too much of abroad generalization by them given how the assessments are made under many years of controlled environment experiments with wide range of plant species. Wheat, corn, soybeans, etc. will have different ozone sensitivities and very different than many other plant species which may not have any negative reaction to ozone at all (maybe true for some crops also). It would be more convincing for readers to include some other reference(s), perhaps even more recent if possible, that claims similar numbers for crop loss from ozone.

The numbers stated from Feng and Kobayashi [2009] are a bit more clarified with respect to the different crops. Another reference was added. Debaje [2014] studied the influence of ozone on the rabi rice and winter wheat in India and found similar numbers (3% to 11%). There are far more references concerning this topic but most of them focus on the crops typically grown in the US and Europe.

The discussion of the important role of tropospheric ozone is very short in the Introduction. You might mention more directly regarding emphasizing the importance of tropospheric ozone that it is the third most important radiative greenhouse gas [IPCC, 2014], but also has the good property as being main source of air purifier/oxidizer OH. I don't know how much of the photochemistry discussion with details is really needed in the Introduction, maybe okay, but it seems to jump away then from this detailed photochemistry discussion into retrieval methods and measurements (which is the emphasis of the paper).

The photochemistry discussion was skipped. See also reply to referee #1. The importance of ozone as green house gas is more emphasised.

The harmonization of the datasets was a bit hard to follow but I am sure all okay in fundamental approach. The time series regression fits that you show in Figure 6 are for tropical averages – but does a similar plot for $12.5°$N (or $12.5°$S) have any indication of instrument offset(s) despite following the multi-instrument harmonization? This might in part explain the problems in winter latitudes with high standard deviations in Figure 2?

The description of the harmonisation was clarified and also in the introduction a short

summary of the CCD and of the harmonisation was added (see also comment #1). The uncertainties observed outside $\pm 12.5°$ N are mostly caused by variability in the ozone columns. Also the standard deviation for the individual instrumentals is enhanced outside $\pm 12.5°$ latitude in the winter month. So the main cause might be seasonal mitigation of the subtropical jet and the accompanying stratosphere-troposphere exchange (e.g. Sprenger et al. 2003). This might cause a respective variability in the data. It was clarified in the manuscript.

In discussion of Figure 1 the small <2 DU column for tropopause to effective cloud pressure is probably generally typical over the remote tropical Pacific over thick clouds. The Ziemke et al. [2009] Figure 8 used MLS ozone profiles to get the SOC and compared 1-1 with ACCO from OMI. Away from the Pacific the ACCO and SOC difference was much greater than 2 DU – hence the need for applying near zonally invariant SOC to get gridded TTOC everywhere. That Figure 8 seemed to summarize why the simple CCD method works as well as it does including assumption of near-zero zonal variability of SOC (from MLS in the figure).

I am not sure the referee understood the figure correctly, so I have to clarify it. The small <2 DU column is used to harmonize the different ACCOs for the different cloud altitudes. If a cloud altitude is lower than 10 km we have to subtract a correction term because the ACCO also includes the partial column between the 10 km altitude level and cloud top at e.g. 8.5 km. Vice versa a correction column is added if the cloud top is above 10 km. After that the harmonization of the ACCOs, they are averaged within a grid cell or in the reference region. This step of the algorithm is independent of longitude or latitude. The section was clarified.
On the other hand the referee is certainly right, that the CCD technique can only be applied because the stratospheric O_3 column does not show a significant zonal variability as shown in Ziemke et al. [2009] Figure 8. The respective reference was included and a comment was added.

Most of the figures are difficult to make out, especially in terms of text readability.

Please increase sizes.
the fontsize in the figures was increased.

There are several typos and some sentences that can use some re-editing. I found several in the reading, but didn't list them all here (you may have already found them and corrected them):
Page 3, line 6: ". . .increase (Wang et al., 2009). . ." (or reference at end of sentence)
Page 14, line 10: ". . .quasi-biennial. . ." Page 17, line 7: "increase"
corrected

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