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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Trends of ozone total columns and vertical distribution from FTIR observations at 8 NDACC stations around the globe" *by* C. Vigouroux et al.

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Answers to Referee#1

We thank the referee for his/her constructive remarks and suggestions. We answer below to the specific comments.

p. 24628, line 18-21. Can you please clarify in the text if ozone isotopologues were fitted in the retrieval of all stations or only Harestua? Or what is the difference in the fit of the Harestua. May be it is better to describe algorithm for all stations with the exception of Harestua and then describe algorithm for Harestua

station separately.

Harestua does not fit the ozone isotopologues and is the only station that does not do it. We have clarified the section by adding the common parameters in Table 2, to highlight the common parameters (see comment Referee#3), and the differences at Harestua appear then clearly.

p.24630, lines 20-25. Is it know that the homogenization of the two records for Wollongong has been made to remove any step functions associated with the use of two different instruments that have unknown spectral characteristics?

Unfortunately, they were only 6 days of measurements with both instruments measuring simultaneously. It is too few to apply a correction based on direct comparisons. As the ILS is fitted for the Bomem spectra (so not fixed to ideal), one could expect that the ILS is correctly represented by the fitted values so that the ozone amounts are well determined. This is why we do not specify anything for the Wollongong time-series. An argument against the use of the ozone absorption line shape to retrieve simultaneously the ozone profiles and the ILS is that a change on the ozone concentration at a given altitude may be interpreted wrongly as a change in the ILS. It was found that at Jungfraujoch fitting the ILS instead of assuming that it is ideal, improved the agreement with correlative ozone profiles measurements (Barret et al., 2002). But the situation may differ at Wollongong and it would be indeed worth making careful comparisons there. Another solution to deal with periods without cell measurements, would be to retrieve independently the ILS using N2 and CO2 lines in the historical solar spectra, and then fix the derived ILS values during the ozone retrievals. The control of the quality of the measurements can also be done such as in the recent paper of Barthlott (AMTD, 2014) making use of the measured XCO2. Discussion about the ILS has been added in the text (see also comment Referee#3).

p. 24631, line 19 – It would be good to provide additional AK from another stations for comparisons, the one that is a different in vertical distribution.

We have added a plot of the AK at Izaña.

p.24633, lines 8-9, you stated that "To reduce the auto-correlation in the residuals, we use here the monthly means time series." The autocorrelation for monthly mean ozone time series is still significant (see estimated by Bruner et al, send of section 2) and thus affects the uncertainty of trend analysis.

Indeed, the monthly means reduces the auto-correlation, but it is still significant. That is why we also used a Cochrane-Orcutt transformation to have better estimate of the uncertainties on the trends. This was explained at p. 24636, l. 5-8. To clarify this, we have moved this small paragraph on Cochrane-Orcutt transformation, to make it appear just after the text on monthly means.

p.24634, lines 18, 24-28, p 24635, lines . The explanation for the ELL, ELS and ELU abbreviation is not provided the first time these parameters appear in the text. Few sentences later these proxies are related to the low, middle and upper stratospheric layers. However the layers are also not clearly defined in the text. I found the description of layers in Table 3, which should be discussed a bit more in section 2.3, and the LowS, MidS and UppS should be defined there as well.

Done.

Also, please provide ftp reference for the NCEP dataset in Table 4.

We prefer not to include NCEP in Table 4, since NCEP data are not used as a proxy (except for tropopause pressure whose ftp reference is already in Table 4) and since the purpose of Table 4 is to summarize all proxies that have been tested.

p. 24635, lines 3-10 . As your trend fitting model follows the Brunner et al (2006) approach for an accumulation process over a year, please clarify if you use different tau constant for different stations (tropics vs high latitude station) and seasons.

Indeed, I also follow Brunner et al. (2006) work in using different tau constant depend-

ing on season and station. This has been clarified in the new manuscript.

p.24637, line 9-10 when describing difference in Harestua trends, add "(results are not shown)" However, it might be better to add Harestua tropospheric ozone data in Figure 5. for visual comparisons with Ny-Alesunds.

The plot for Harestua tropospheric columns has been added to Fig. 5. And we have added the text that at Thule the trend in the troposphere also occurs in the 2004-2012 period (but the plot is not shown).

p.24637 line 11-14. It is not relevant to compare ozone-sonde trends at 500 hPa level and Ny-Alesund's integrated tropospheric column (ground to 10 km) as upper troposphere (between 500 hPa and tropopause) can have a significant contribution to tropospheric ozone column variability.

The work of Hess and Zbinden (2013) linked the ozone variability at 500 hPa (about 5-6 km) to the ozone variability observed in the lower stratosphere at 150 hPa (13-14 km), and they studied the influence of stratospheric variability on tropospheric ones. So, indeed the upper troposphere is probably linked also to ozone at 500 hPa. We believe that we can expect that the whole tropospheric column at Ny-Alesund can show similar patterns as sondes at 500 hPa since the measurements are sensitive to the ozone variability occurring in the whole column, so at 500 hPa (and up to tropopause) as well. Hess and Zbinden showed similar patterns in the 500 hPa and 150 hPa ozone at northern Europe stations, in particular in 2005. We wanted to point out that we also see the correlation between a stratospheric influence (via VPSC proxy) and the tropospheric column at Ny-Alesund, also in 2005. To clarify that there is a connection between Trop and LowS columns at Ny-Alesund, we have added in Fig. 5 the plot for LowS, together with the associated VPSC proxy. However, in addition to the different altitude, a difficulty of linking the work of Hess and Zbinden (2013) to ours is due to their 12-month running means which highlight the long-scale processes while we use monthly means and therefore show the month-to-month ozone variability. We have removed the comparison with this work to simplify the discussion on our own results.

p.24637, lines 18-20. I cannot see any large signal in VPSC data in 1998 and 1999. Is that what you are trying to attribute to observing larger ozone values as compared to other time periods? And it is in the contrast to 2005 and 2011 when VPSC signal is large, while tropospheric ozone is low, correct? May be this section should to be re-written to make it more clear for the reader to see how VPSC might be influencing the tropospheric ozone variability at Ny-Alesund. It seems that comparison to other paper makes it difficult to understand your results.

Indeed, the larger ozone values in 1998 and 1999 were related to the VPSC, not in the sense that VPSC shows large values those years, but in the sense that it shows "non-large" values, i.e. there was less decrease of ozone those years (due to warm winters). However, in addition to the effect of "no-decrease due to VPSC" (about +3E16 molec.cm-2 in Fig. 5), the QBO signal also contributes to higher values in 1998 and 1999. We will focus in the new manuscript to the 2005 and 2011 years to clarify the main message.

p.24638 line 1-3. I do not see large departure in VPSC in 2003. Moreover, the model did not capture low ozone point in 2003 (middle panels of Figure 5). May be something else can explain this variability, QBO or Equivalent latitude?

Indeed, the VPSC in 2003 is not as large as in 1996, 2000, or 2005. It is larger than 1998, 1999, or 2004, and similar to e.g. 2010. However the model gives similar ozone LowS values in 2000 and in 2003, instead of twice larger values in 2000. This is due to the TP signal (lower panel of Fig. 5) which enhances ozone values in 2000 while it decreases, in addition to VPSC, the ozone in 1996, 2005, and also in 2003, but for one single point (therefore maybe less clear in the plot). The low ozone value obtained by the model in 2003 (similar values than 2000) is due for about 1.5E17 molec.cm-2 to the VPSC signal and for about 4.5E17 to the TP signal. Since the TP signal dominates the

ozone decrease in 2003, we will remove from the text the reference to the 2003 year when discussing the influence of VPSC. The measured lowest value in 2003 is indeed not captured by the model, which is only represented by VPSC and TP (Fig. 4), which means that the other proxies (including QBO and EL) were found non- significant. We have tested to run the model forcing the QBO and the EL to be included, but as might be expected (otherwise probably the proxy able to describe such a outlier event would have been significant) they do not explained the lowest value. The EL signal goes on the other direction (increases ozone at that date), while the QBO signal is too small at that date to impact significantly the ozone values. This measured low ozone value remains unexplained at present.

p.24640, lines16-17. It is hard to believe that the Lauder dataset could contain a significant contribution from Solar cycle, especially in the troposphere. As authors point out – the record is too short to be analyzed for a Solar cycle signal. I would not use the Solar cycle in analysis, or would adapt the fit from another middle latitude station (i.e. Wollongong?). You can try to analyze a shortened 2001-2012 record from Wollongong to check if you get similar artifact from fitting data with the Solar cycle proxy.

We have made the analysis at Wollongong for the 2001-2012 period, and the time-series (therefore the solar cycle signal) do not show the same behavior as in Lauder. To stay coherent for all stations, we prefer to keep the model the same for all stations, but we have highlighted even more than in the previous manuscript that the solar signal should be taken with care at Lauder: we give the trends without the solar cycle in Table 6. The impact is not large in MidS and Total columns, but the 1.1 %/decade decrease on UppS trend is sufficient to make the trend non-significant (+1.7+/-2.4 compared to +2.8 +/-2.4).

p.24641, line20-24. Can the choice of 470 K reanalysis for calculation of the ELL for Wollongong be a problem or is it a sequential model fit that determines which proxy to keep? Why was 470K chosen to represent the low stratospheric layer at

Wollongong?

For each layer, the initial model starts with only the seasonal cycle and the trend parameters. Then the choice of the proxies to be added in the model is made by a stepwise regression procedure as described in Sect. 3. The model tests all given proxies (solar, QBO, TP,...). For the EL proxy we test in the model only the EL proxy that corresponds to the altitude of the approximate middle of the layer. Therefore, it is not possible to include the ELM or ELU proxies in the model for the LowS columns. For the total columns, the three ELL, ELU and ELM proxies are tested. Concerning the choice of 460 K for ELL at Wollongong and Izana: we make an approximation with the Equivalent Latitude used for the different layers. We are dealing with thick partial columns: 12-20 km for high and mid latitude stations; 15-23 km for subtropical stations, to avoid to be too close to the tropopause region which could include tropospheric signal in the lower stratospheric layer. So, we have chosen to use the Equivalent Latitude which would correspond to approximately the altitude of the middle of the layer (about 16 km and 19 km, for mid/high and subtropical latitudes, respectively). Indeed, for Wollongong the choice of 460 K has an impact on the trend of LowS: if we use the 370 K proxy, there is no trend anymore in the proxy itself and the Wollongong LowS trend becomes +2.5+/-2.8 %/decade. The impact of using the 370/550/950 K series instead of the 460/700/1040 K is negligible at Izana and at Wollongong for the other layers. We prefer therefore to stay coherent with the way we have chosen the EL proxy, and provide to the reader the information about the trend in the EL proxy at 460 K, and the obtained trend at Wollongong without this proxy (+2.4+/-2.8 %/decade), in case one would like to remove this impact for comparisons. Probably, the way of dealing with the EL proxies could be improved in the future.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 24623, 2014.