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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Trend and variability in ozone in the tropical lower stratosphere over 2.5 solar cycles observed by SAGE II and OSIRIS" *by* C. E. Sioris et al.

Anonymous Referee #1

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1 Overall rating of the manuscript and major suggestions

The manuscript by Sioris et al. presents a combination of SAGE II (V7.00) und OSIRIS (V5.07) ozone time series for the tropical lower stratosphere. The paper gives a very detailed and thorough multiple linear regression analysis of the resulting data set. Similar to Randel and Thompson (2011), who combined SAGE II (V6.20) and SHADOZ ozonesonde data, Sioris et al. find a significant declining ozone trend in the 18 to 23 km altitude region. This trend seems to continue throughout the 1984 to 2012 period. It is consistent with chemistry climate model simulations, which predict such a trend due to

increasing upwelling in the tropics, caused by increasing greenhouse gases.

Important contributions to the interannual variability also come from QBO, ENSO, and tropical tropopause pressure, as shown in detail by the paper. Solar cycle variations and variations directly correlated with stratopheric chlorine loading, however, are not found to be significant in Sioris et al.'s analysis. As mentioned further below, I feel that some more caution might be required before excluding the solar cycle.

Generally, I find that this is quite a good and thorough paper. The results are very plausible and are generally clearly presented. Overall, the text seems a bit wordy and lengthy, but I have no simple solution how the text could be shortened. In part the length comes from the fact, that the authors present a lot of details on their use and combination of regression parameters. I think it is good to have this information, but maybe the authors find a way to shorten things a bit.

One important thing that is missing in the paper is a discussion how the uncertainty margins / confidence intervals were obtained. This is important, because only (roughly) correct uncertainty margin allow correct assessment of the significance of trends and other results. Did the authors account for AR1 auto-correlation in the residuals? Auto-correlations in the residuals usually mean that the uncertainty estimates from standard linear regression are too small. When auto-correlations in the residuals are accounted for, the uncertainty margins usually increase (e.g. Weatherhead et al., 2000). What about longer auto-correlations in the residuals (see also Vyushin et al. 2010)?

Also, the abstract and several places in the text should include more cautioning words about the significance of the trends. Figure 4 is especially revealing in that aspect: The large negative trend e.g. at 18.5 km over the entire data period very much depends on the high SAGE II values around 1990 (near solar maximum, see also the higher values around 2000 to 2004, the next solar maximum). The continued negative trend (e.g. at 18.5 km) during the 2001 to 2012 OSIRIS period also depends very much on the few high values around 2002 to 2004 (near solar max) in that record. I think this

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needs to be recognized in a few places in the text, and also in the conclusions and in the abstract.

2 Detailed comments

Introduction: I think somewhere in the introduction it should be mentioned why lower stratospheric ozone trends in the tropics are interesting. Connection with modelled changes in Brewer Dobson circulation. Randel and Thompson 2011 paper.

pg 16664, line 19, pg 16665 line 17, pg 16669 line 22: It would be good to give sources / URLs for the used data sets.

pg 16664 lines 23/24: Why has the filtering a large effect? Is not the main point that the SAGE data are heavily contaminated and not usable in periods/ regions with large aerosol loading?

pg 16666 line 6: Maybe add reference Adams et al. 2013?

pg 16666 line 20: "anomaly period" should probably be "overlap period"

pg 16675 line 6: It would be good to give a reference for the used elimination / regression procedure.

pg 16677 line 2: "1/(1/0.5 $\pm$ 12/27)" for the expected periods. The use of years in the first term and months in the second term confused me. Maybe better to use "1/(12/6 $\pm$ 12/27)"

pg 16670, lines 7 to 14: One thing that is not mentioned, and might be important, are possible correlations / similarities between tropopause pressure and ENSO. I would expect that there is something, and the authors should check and comment on that. (relevant also for a few other places in the text).

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pg 16678, line 6: As mentioned above, a discussion of the resulting uncertainties, auto-correlation in the residuals, etc. is missing here.

pg 16679, lines 14 to 24: I think the comparison to the most relevant paper (Randel and Thompson 2011) is missing here! In fact, I think the trend result from Fig. 8c of Randel and Thompson (2011) should also be plotted in Figure 3. Also the results for QBO and ENSO from Table 2 should be compared / put in the context of Randel and Thompson (2011).

Section 3 in general: I think it would be useful to have more structure in this section, e.g. by having sub-sections on linear trend, ENSO, QBO, tropopause pressure and solar cycle. The same might be true for section 2.2.

Figure 2: The axis labels and numbers should be made larger. The lines should be thicker.

Figure 3: The axis labels and numbers should be made larger. All lines should be made thicker. (In my printout they are hardly visible). The trend result from Randel and Thompson (2011) should be added to the plot.

Figure 4: I think it would be good to also plot a line for the derived linear trend - since the linear trend is one of the key topics of the paper. I also feel that it is important to point out the few high data points around 1990 and from 2001 to 2004, all near solar maxima, that are really key drivers for the long-term declining trend.

Caption of Figure 4: El Chichon erupted in 1982/1983, that should be mentioned. The biggest eruption was Pinatubo in 1991, which is in the Figure, but is missing completely in the caption.

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3 References

Adams, C., et al.: Assessment of Odin-OSIRIS ozone measurements from 2001 to the present using MLS, GOMOS, and ozone sondes, Atmos. Meas. Tech. Discuss., 6, 3819-3857, doi:10.5194/amtd-6-3819-2013, 2013.

Randel, W. J., and A. M. Thompson (2011), Interannual variability and trends in tropical ozone derived from SAGE II satellite data and SHADOZ ozonesondes, J. Geophys. Res., 116, D07303, doi:10.1029/2010JD015195.

Vyushin, D. I., T. G. Shepherd, and V. E. Fioletov (2010), On the statistical modeling of persistence in total ozone anomalies, J. Geophys. Res., 115, D16306, doi:10.1029/2009JD013105.

Weatherhead, E.C., et al. (2000) Detecting the recovery of total column ozone, J. Geophys. Res., Vol. 105, 22,201-22,210.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 16661, 2013.