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Atmospheric Chemistry and Physics
Discussions

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 #3

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This paper evaluates the trend of tropical ozone in the lowermost stratosphere from the latest versions of SAGE II and OSIRIS satellite measurements, merged to provide range resolved time series spanning 28 years. Since various chemistry climate models predict a decrease of stratospheric ozone in the tropics due to the acceleration of the Brewer-Dobson circulation, a prediction that has to be strengthened by observational evidence, this study addresses an important issue. The methodology for deriving trends is classical, based on widely used linear regression models using common predictor variables such as the solar flux, QBO, ENSO, EESC. Considering the importance of the issue, I suggest publication in ACP, provided that important comments are taken

into account.

Main comments and suggestions

1. The style of the article is quite dense, without clear-cut messages. The writing should be revised in order to provide a clearer explanation of the main results.

2. Introduction (page 16663 l16-20) : More discussion and reference to recent trend studies on tropical ozone based e.g. on SAGE II and SHADOZ data (Randel and Thompson, 2011) should be provided.

3. New version of SAGE II data (section 2.1.1) : the authors use version 7 of SAGE II. Some detail is given on the data filtering in order to avoid contamination by volcanic aerosol due to the Pinatubo Eruption and more generally improve data quality, but no indication is given on the improvement gained with respect to the earlier data version, especially in the tropics. Also, what is the effect of clouds on satellite ozone data retrieval at the tropical tropopause and above?

4. Comparison between SAGE II and OSIRIS data (p16668, l1-4): The difference between anomalies seem to be small (figure 1) but yet the difference between monthly climatologies needs to be taken into account in the construction of the merged time series. Can the authors better justify the scaling of the monthly climatologies? The use of standard error instead of standard deviation would provide a better estimation of the significance of the bias between both measurement types in the overlap period in figure 1.

5. Construction of the merged time series (section 2.1.3): The construction of the merged time series needs to be clarified. In particular, there seems to be a typo in the following sentence: The denominator in Eq. (1) represents the inter-sensor mean ozone over the anomaly period. How are merged the SAGE II and OSIRIS monthly mean data in the overlap period: are they averaged? A figure could be included representing the merged time series as a function of time and altitude.

6. Construction of the regression model (section 2.2):

a. My main concern here is that the reasons for model optimization are purely statistical (reduce linear trend uncertainty) without sufficient consideration of physical mechanisms behind the effect. For example, the ENSO lag seems quite noisy as a function of altitude (table 2).

b. What is the reason for representing the deseasonalized tropopause pressure in figure 2? The figure is of poor quality, as are the other figures of the article (the lines are generally not visible). If a predictor variable is to be presented, the other predictor variables (ENSO, Solar flux…) should also be shown.

c. Reasons for including variables or excluding others seem sometimes far stretched. For example, aerosols are excluded from the analysis although there has been a trend in aerosol extinction in the tropical stratosphere as well as increased variability due to small volcanic eruptions (see for instance Vernier et al., 2011). In contrast EESC is included (together with the linear trend) although the response is not significant. One of the reasons given (it was done in Bodeker et al., 2013) is not convincing. Likewise, the reasons for not considering solar harmonics and seasonal or monthly linear trends are dubious.

7. Discussion of the results of the regression:

a. The response of ozone to the various predictors should be defined (using an equation) as soon as Table 2 is introduced and indication of the 95% CI error bars of the response should be given. A figure could also very well represent the main responses as a function of altitude. To what correspond the values in the column "C"?

b. The discussion of the QBO response in lengthy and cumbersome. The authors should reduce it and provide the main message there.

c. Figure 4 (or an additional figure) should include a representation of the residuals. Is autocorrelation taken into account in the derivation of the uncertainties?

d. If one looks attentively at figure 4, one can see that SAGE II monthly data are much noisier than the OSIRIS ones at the lowermost altitude. At this altitude, a decrease in ozone is seen only up to 2005, followed by an increase. Can the authors comment on that behavior? To what extent the trend is mostly influenced by the rather noisy SAGE II data in the eighties?

e. Figure 3 should include a comparison with model results as shown in Lamarque and Solomon (2010). In this paper, the negative trend peaks around 70 hPa and decreases below, while in the present study, the trend is negative down to 18 km. Can the authors comment on that? Since a negative trend of around 3% per decade can be attributed to a decrease in tropopause pressure, how much is the trend that can be attributed to increases in greenhouse gases?

References

Vernier et al., Major influence of tropical volcanic eruptions on the stratospheric aerosol layer during the last decade, Geophys. Res. Lett., 2011, DOI: 10.1029/2011GL047563

Randel and Thompson, Interannual variability and trends in tropical ozone derived from SAGE II satellite data and SHADOZ ozonesondes, J. Geophys. Res., 2011, DOI: 10.1029/2010JD015195

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