

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 #2

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General Comments

This paper analyses trends and variability in tropical lower stratospheric ozone based on a new merged data set of SAGE II and OSIRIS data. The analysis of the new data set sheds new light on the trends in tropical ozone so that the paper would make a good contribution to ACP.

My major critique is that the paper is entirely focused on statistics and that there is very little discussion of the physical and chemical mechanisms leading to a certain trend or variability in tropical ozone. For example, it is known that the QBO impacts verti-

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cal transport in the lower stratosphere (Punge et al., 2009) (and, in turn, that vertical transport then impacts tropical ozone (Randel et al., 2007)), so the strong impact of the QBO on trends is probably not surprising. Further, the increase in carbon dioxide does not directly impact tropical ozone in the lower stratosphere. It is a rather indirect chain of effects where (again) the rate of tropical upwelling is important. Thus the increase in carbon dioxide is rather linear, but the effect on tropical ozone is not guaranteed to be linear. The paper should address these issues and the related mechanisms of changes in tropical lower stratospheric ozone.

Moreover, EESC plays a substantial role as an explanatory variable in the paper. I question whether EESC should be used as an explanatory variable at all in the analysis. As stated in the paper, there is only a slight (bit how ‘slight’?) impact on tropical lower stratospheric ozone – it is argued through the change in column ozone. There is clearly no effect through local chemistry as reactive chlorine is essentially absent in the lower tropical stratosphere. So if there is no mechanism apparent, how EESC might impact tropical lower stratospheric ozone why is it included in the analysis? Arguably, the temporal development of N_2O and CH_4 (which are both greenhouse gases like the CFCs, but also impact ozone loss cycles and thus the ozone column) should be more important variables than EESC.

Finally, as already stated in my ACPD review, the quality of the figures needs to be improved substantially. All figures have very thin axis lines, Figs. 2 and 3 also very thin lines in the figure. Figs. 2-4 have labelling in much too small letters.

In summary, the paper addresses an important interesting subject and should be suitable for publication in ACP. However, I feel that the issues raised in this review need to be addressed carefully in a revised version.

Detailed Comments

- page 16662, l. 2-4: Put the first sentence at the end of the abstract.
- page 16662, l. 9: replace reaching by maximising
- page 16662, l. 13: Citation for older trend studies?
- page 16662, l. 15: 'primarily'? any other reason?
- page 16662, l. 15: 'primarily'? any other reason?
- page 16662, l. 16: The first WMO Report on ozone was actually in 1981 (WMO, 1982)
- page 16663, l. 28: the paper by Daniel is on GWPs, a better citation for EESC in the framework of ozone is e.g. Newman et al. (2007)
- page 16664, l. 1: drop 'fewest maxima', it is well known that EESC has only one peak
- page 16664, l. 22: state what the essence of the Wang filtering is
- page 16665, l. 13: Do you want to be more specific about which Envisat instruments? Only MIPAS?
- page 16666, l. 8: somewhat unclear what is meant by GPS timing issue.
- page 16666, l. 11: Here and elsewhere: make sure that variables in the text (here zmm) are in italics
- page 16667, l. 11: You could consider a latitude band which is varying with season to optimise the selection of tropical data.

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- page 16669, l. 9: The increase in carbon dioxide does not directly impact tropical ozone in the lower stratosphere. It is a rather indirect chain of effects where the rate of tropical upwelling is important. I suggest discussing the physics here in more detail.
- page 16669, l. 25: more explanation is needed here about the concept of orthogonal QBO time series. Here just two names, QBOa and QBOb are stated.
- page 16670, l. 14: What about QBOb? Why is it not mentioned, why is its behaviour different?
- page 16670, Eq. 2: State which the underlying assumptions are for EESC here, Which age of air, which width of the age distribution etc. (Newman et al., 2007)
- page 16670, l. 24: How 'slight'? I question whether EESC should be used as an explanatory variable at all in the analysis.
- page 16671, Eq. 3: Have the authors considered to do the analysis on theta levels as well? This would remove a large fraction of the ozone seasonal variation in the polar lower stratosphere (Randel et al., 2007; Konopka et al., 2010)
- Personally I find the term 'annual harmonic of linear terms' (and similar wording) confusing. Why not call it seasonal variation of the linear trend (as sometimes done later in the paper)
- page 16672, l. 27: the discussion on heterogeneous chemistry here is rather speculative. I suggest either to be more explicit (which het. reactions?) or dropping this part.
- page 16673, l. 14: drop; as stated above it is well known that EESC has only one peak and no short term variability (e.g., Newman et al., 2007)

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- page 16673, l. 16: as stated above, I find the term 'annual harmonic of linear terms'
- page 16675, l. 11: Do not abbreviate confidence interval
- page 16679, l. 21: Discuss what the reasons could be for the discrepancy with the results of Forster et al. (2007).
- page 16680, l. 15: But the effect of the greenhouse gases on ozone is via tropical upwelling. Thus, for this argument to hold one need to show that the trend in tropical upwelling depends also linearly on greenhouse gases, which is not quite as clear. There is some support from models perhaps (Butchart et al., 2010).
- page 16681, l. 19-23: is there a physical explanation for these findings? At least are they consistent with the expected mechanisms how, e.g., QBO and ENSO impact tropical ozone in the stratosphere?
- page 16683, l. 9, 10: Are these statements about the spectrum of the age of are consistent with observations (e.g. Stiller et al., 2008) ?
- page 16685, l. 16: I find the statement about the 'central tendency' which 'can be measured using the mode' (and similar below) confusing. I believe what you mean is the mean age-of-air.
- page 16686, l. 10: I am confused here: is the -3% trend a result of this paper or not? If it is not the provide a citation, if it is there seems to be a contradiction with the sentence following.
- page 16686, l. 14-15: As stated in my initial review, I consider this statement very speculative. Which mechanisms are alluded to here, which chemistry on the clouds, which trends of cloud surface area? Either this sentence needs to be dropped of there needs to be a extensive discussion of the mechanisms in the paper.

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References

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

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