Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-575-RC2, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Impact of Non-uniform Sampling on Stratospheric Ozone Trends Derived from Occultation Instruments" *by* Robert P. Damadeo et al.

Anonymous Referee #2

Received and published: 4 October 2017

The manuscript "The Impact of Non-uniform Sampling on Stratospheric Ozone Trends Derived from Occultation Instruments" by Damadeo et al. describes an application of a 2D regression model to estimate the main components of the ozone variability (QBO, solar, etc.) as well as long-term trends and instrumental drifts using data from several satellite instruments. This approach is reasonable and produces better results than just a simple linear regression by latitudinal belts. I have two major comments, but otherwise, the paper can be published after relatively minor revisions.

Major comments:

1. It is stated that the method is described in the previous paper by Damadeo et

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al., 2014. It is not really the case since this paper deals with multiple instrument. The authors should add an Appendix or Supplement with the method description. In particular:

Damadeo et al., 2014 analyzed data from just SAGE II. How exactly was the analysis of six satellite instruments (or just three? SAGEII, HALOE and ACE- FTS) done? Was any instrument-specific weighting applied? The authors stated in many places that they try to use orthogonal functions. But the functions should be orthogonal on the dataset of available observations. Damadeo et al., 2014 mentioned that seven Legendre polynomials of latitude were used for the fit. However Legendre polynomials are orthogonal on 90S-90N, not on the 60S-60N interval where almost all measurements were taken. How was this handled? Also, it seems that seven polynomials are too many. The authors should provide some justification.

2. The authors compare their STS regression with the MZM method. I suggest the author reduce the part related to MZM and focus only on their STS results.

The MZM method is used in the paper in a very peculiar way: the authors just average all data within 10-deg latitudinal belts and assigned the value to the middle of the belt at the middle of the month. Most of the ozone variability is coming from the annual cycle. The annual cycle can be estimated, for example, by the same approach as discussed in the paper: by fitting all SAGE II data by a set of spherical (for latitude and, if necessary, longitude) and sin/cos functions (for time). Then the MZM method could be applied to the deviations from the annual cycle. The annual cycle is indeed orthogonal to the other proxies, so it should not affect their estimates. This step would largely remove most of the sampling problems and will likely produce results similar to STS.

Specific comments:

P.4, l. 12. What data were used for this conversion? See box 2-1 from Ozone Assessment 2014 and comment on potential conversion errors.

P.4, l. 22. ENSO is mentioned here, but no result was shown. Is it necessary to include it?

P.4, l. 22. The shape of the EESC function depends of latitude and altitude. What exactly was used? The authors used 2 “orthogonal” EESC functions and show the trend results. But how does the resulting EESC signal look like? What is the “phase”/delay? If the authors want to have an additional delay for EESC, it is more logical to introduce an unknown time lag.

P. 8. Solar cycle. It is difficult to get the 11-year solar cycle from SAGE data. Is the estimated solar signal statistically significant? Are the differences in the solar signal at different latitudes significant?

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