Atmos. Chem. Phys. Discuss., 14, C5604–C5612, 2014
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Reevaluation of stratospheric ozone trends from SAGE II data using a simultaneous temporal and spatial analysis" *by* R. P. Damadeo et al.

Anonymous Referee #3

Received and published: 5 August 2014

GENERAL COMMENTS

This is a very good paper that presents what is possibly the state-of-the-art for regression analysis of the SAGE II database. It is certainly suitable for publication in ACP and will be of interest to a wide range of the ACP readership. I am very pleased that this paper takes the time and care to point out some of the perils and pitfalls with the application of regression analysis. I do have a few concerns, however, that will need to be addressed before the paper is suitable for publication. These are detailed below.

SPECIFIC COMMENTS

Page 2, Line 7: account for any potential diurnal variation on what? Ozone presumably? or ozone trend?

Page 2, Line 9: What is 'the recovery period'? First this presupposes that ozone is actually recovering from the effects of ODSs - something that your paper would need to prove first - which is also something that cannot be done only with regression analysis. I think that it would be clearer for the reader if you just stated the period here.

Page 2, Line 9: Will readers unfamiliar with the field know what is meant by 'variable turnaround time'?

Page 2, Line 10: A hemispheric asymmetry in what? Ozone trends?

Page 2, Line 13: I find this difficult to believe. If you have a data set that runs e.g. from 1984 to 2000 and a second data set that is biased 20% low that runs from say 1998 to 2014, and you apply your 'global regression' to an uncorrected merge of those two data sets, I can't believe that the trends would not be biased negative? I think that you need to say more here about how this would work.

Page 2, Line 20: Is 1% the typical 1 sigma random uncertainty on each ozone measurement? If so, perhaps you should say so.

Page 2, Line 21: Do you mean accurate measurements or precise measurements? Precision matters more for trend analysis than accuracy.

Page 3, line 15: It wasn't completely clear to me what you did here. I didn't understand what you meant by 'from multiple component data sets'. For example, consider the solar cycle basis function as 10.7 cm solar flux. How is this a 'multiple component data set'? Couldn't this be made much simpler by using Gram-Schmidt to orthogonalize the basis functions?

Page 3, line 19: As done in Section 3 of Austin, J., K. Tourpali, E. Rozanov, H. Akiyoshi, S. Bekki, G. E. Bodeker, C. Brühl, N. Butchart, M. Chipperfield, M. Deushi, V. I. Fomichev, M. A. Giorgetta, L. Gray, K. Kodera, F. Lott, E. Manzini, D. Marsh, K.

ACPD

14, C5604–C5612, 2014

Matthes, T. Nagashima, K. Shibata, R. S. Stolarski, H. Struthers, and W. Tian (2008), Coupled chemistry climate model simulations of the solar cycle in ozone and temperature, J. Geophys. Res., 113, D11306, doi:11310.11029/12007JD009391 which you should therefore cite.

Page 3, line 19: But this will not create a basis function that is orthogonal to all other basis functions. It will only create a basis function that is orthogonal to the one it is being shifted with respect to. While your approach does allow for phase shifts in any periodic basis function, it does not ensure that all basis functions being used in the regression are orthogonal. For that you need Gram-Schmidt orthogonalization.

Page 4, line 1: Account for over 99% of the variance in what? It certainly can't be in ozone.

Page 4, line 4: This is novel. As far as I am aware, all previous regression studies have assumed that there is no delay in solar cycle effect on ozone and therefore did not consider any phase shift in the solar cycle basis function. Do you have any physical basis for expecting there to be a phase shift between change in solar output and ozone response?

Page 4, lines 10-12: This is worded quite confusingly. Isn't it simply the case that you have one linear trend basis function that covers the whole period, and then a second linear trend basis function that is everywhere zero before 1997 which quantifies the change in trend after 1997?

Page 4, line 17: Account for over 99% of the total variance in what?

Page 4, line 19: I think that you could avoid this 'pathological' behavior by orthogonalizing all of your basis functions. Assuming that the trend basis function comes before the EESC basis function in the Gram-Schmidt orthogonalization, this would effectively remove any trend from the EESC basis function.

Page 4, lines 25-26: I disagree that 'A seasonal cycle in a predictor variable would

ACPD
14, C5604–C5612, 2014

interfere with the seasonal cycle in ozone'. In this case I suspect that you would find that e.g. expanding the volcanic aerosol regression model fit coefficient in Fourier pairs to account for seasonality would, if the mean of the signal is also subtracted, result in time series that are quite orthogonal to the mean annual cycle terms. You should at least do that test so that you can state this more categorically.

Page 5, line 9: I think that you need to be a bit more specific in what you mean by 'cross-term'.

Page 5, line 15: No, the use of orthogonal basis functions does not allow for a change in amplitude with time. The net amplitude of two orthogonal functions is given by sqrt(sqr(A)+sqr(B)) where A and B are the fit coefficients for each of the basis functions. There is no time dependence in the amplitude. Really, including an orthogonal version of a periodic basis function has just one advantage i.e. it allows for the phase shift to be non-zero.

Page 5, line 20: Yes this is, in essence, a cross-term, but really this is nothing more than accounting for the QBO fit coefficient to be seasonally variable which can be accommodated by expending the QBO regression model fit coefficient in a Fourier expansion in season.

Page 6, line 11: I think that it would be good to have a Figure here to show an example of some data swaths.

Page 6, line 16: You need to explain what a 'non-dropped event' is.

Page 7, equation 2: I have a big problem with the use of equation (2). This equation should only be used if you are measuring *the same quantity* N times. You are not measuring the same quantity N times. To make my point clear, consider the figure I have provided with this review (Example1.png). The upper and lower panels both show synthetic measurements of total column ozone on one day in one latitude zone. Both data sets have the same mean (300 DU) and both have the same sigma_Ybar (0.9081

ACPD
14, C5604–C5612, 2014

DU) as calculated using your equations (1) and (2). And yet I would argue that I can have much higher confidence in my daily mean zonal mean for the upper panel since the noise level is lower - essentially I could fit a couple of Fourier pairs to remove the known structure i.e. the clear wave 1 pattern and then, using your equation (2), would derive a much smaller sigma_Ybar. Equation (2) is inappropriate for use in this case because it takes Ybar to be the best estimator for the true value at every longitude. For the upper panel Ybar is not the best estimator for the true value at every longitude. A fit of some sort (as a described above) would be better. This is an important issue that the authors need to deal with more comprehensively.

Page 8, line 20: This is very reminiscent of the Bodeker et al. (2013) approach.

Page 9, line 17: For use in the correction of what? You haven't stated anything about a correction being required.

Page 9, lines 17-18: I didn't understand what you have done here. Specifically I didn't understand why any iteration is required to derive the autocorrelation coefficient. I think that you need to explain in more detail why autocorrelation matters and how you deal with autocorrelation in a data set that is multi-dimensional. It is not clear to me that it can be easily reduced to the equivalent of auto-correlation in a one dimensional data set. Or perhaps this is explained in detail in Appendix B?

Page 9, line 19: Again it is not clear to me why an iterative correction is required.

Page 10, line 19: But isn't it possible that a seasonally dependent coefficient, such as that for the QBO, could be statistically significantly different from zero during one part of the year but not during another? What do you do in that case?

Page 11, line 8: What do you mean by 'the total residuals'? Do you mean the total of all of the residuals? A few lines later you say 'The total residuals are a combination of the correlated and uncorrelated residuals' but you don't say exactly how they are combined.

ACPD
14, C5604–C5612, 2014

Page 12, line 20: But this would only be true of the aerosol basis function had non-zero values over the full period though right? If there was a period of 5 years when the aerosol basis function was identically zero, I believe that the regression model would assign all of the 'seasonality variance' to the annual cycle basis function.

Page 13, line 19: Just to be clear I think that you should say 'at altitudes above where the proxy is available'.

Page 13, line 22: I think that you should say 'is larger around the time of the Pinatubo volcanic eruption' just to be clear.

Page 14, line 16: I would guess that it is the latter but that would be a guess. A key question to be addressed is: do you have any physically-based mechanism in mind that would cause ozone to respond two years later to a change in solar output? If no such mechanism can be envisaged, then it is probably safer to have only one solar cycle basis function in a regression.

Page 14, line 27: I don't understand what a 'regressive response' is.

Page 15, line 11: Given your Figure 8, I wonder if you would care to comment on the findings from previous studies that have reported on strong responses of ozone to Pinatubo over northern midlatitudes but only very weak responses over southern midlatitudes. See, for example, Shepherd et al. "Reconciliation of halogen-induced ozone loss with the total-column ozone record" Nature Geoscience, DOI: 10.1038/NGEO2155.

Page 16, line 21: 'piecewise linear trend' means 'joined at some particular time' and so there is some redundancy in this sentence.

I have concerns related to the EESC-derived trends plotted in Figure 13. Please conduct the following test: take your SAGE II data and replace all 1998 data with the data from 1997. Do the same for 1999, 2000, ... 2005 i.e. make it that ozone stays in 1998 to 2005 exactly as it was in 1997 by repeating 1997 for each year after 1997. The

ACPD
14, C5604–C5612, 2014

ozone trend from 1998 to 2005 is then, by design, zero. I contend that you will derive positive trends in ozone from your EESC-based analyses (but not from your piecewise trend analyses). I think that this is a very easy test to perform and I would like to encourage you to do this test. I believe that it will show that your EESC-based analysis of the ozone trends from 1998 to 2005 is not to be trusted.

Page 25, line 9: Always assumes a constant what?

Page 26, line 17: You either need to write proper acknowledgments or remove this section.

GRAMMAR AND TYPOGRAPHICAL ERRORS

Page 3, line 22: Replace 'variables are created' with 'variables is created'.

Page 3, line 27: Replace 'resulting seven' with 'resulting in seven'.

Page 4, line 13: Replace 'effective equivalent' with 'equivalent effective'.

Page 4, line 14: Replace 'amount of halogen compound loading on the stratosphere' with 'chlorine and bromine loading in the stratosphere'.

Page 5, line 6: Replace 'off of' with 'on'. And likewise elsewhere.

Page 6, line 14: Replace 'that data is' with 'those data are'.

Page 6, line 17: Replace 'are applied' with 'is applied'.

Page 6, line 20: Replace 'exclusion of of any' with 'exclusion of any'.

Page 7, line 3: Replace 'henceforth' with 'hereafter'. Likewise on line 8.

Page 7, line 4: Replace 'latitude bin' with 'latitude zone'.

Page 9, line 22: Replace 'this data' with 'these data'. The word 'data' is plural. It's singular form is datum.

Page 9, line 26: The word 'criteria' is plural so either 'filtering criteria' or 'a filtering

ACPD
14, C5604–C5612, 2014

criterion'.

Figure 3 caption: Replace 'insufficient data exists' with 'insufficient data exist'.

Page 12, line 8: Replace 'data itself' with 'data themselves'.

Page 12, line 17: Replace 'data was used' with 'data were used'.

Page 15, line 6: I think that the word 'extraneously' has been used incorrectly here. I would have gone with 'anomalously'.

Figure 11 caption: Replace 'data does not exist' with 'data do not exist'.

Page 16, line 14: Replace 'exists' with 'exist'.

Page 21, line 29: Replace 'An insufficient amount of data is' with 'Too few data are'.

Page 22, line 9: Replace 'regressed to, where' with 'regressed to where'.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 17681, 2014.

ACPD
14, C5604–C5612, 2014

Interactive Comment

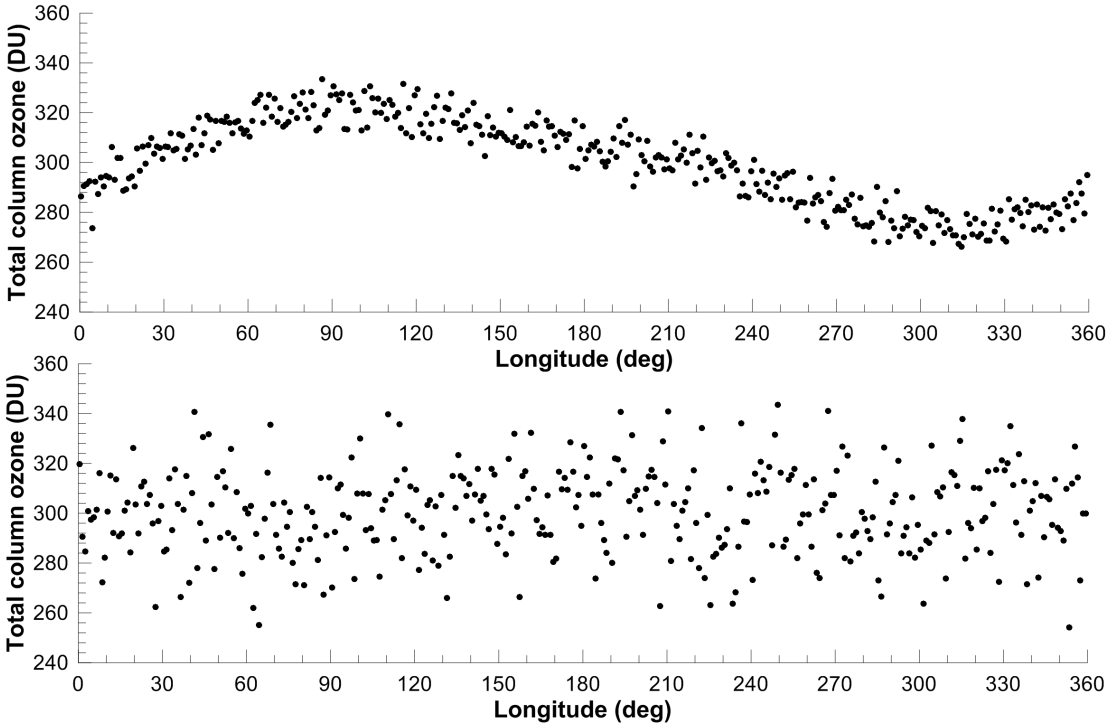

Fig. 1.

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