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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Attribution of stratospheric ozone trends to chemistry and transport: a modelling study" *by* G. Kiesewetter et al.

Anonymous Referee #3

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GENERAL COMMENTS

This is an interesting paper that presents some valuable results and insights into the drivers of ozone changes over the past few decades. I do have a few concerns related to how the analysis was done and these are detailed below. I am also worried that some of the more interesting results are 'hidden' in amongst results which are already well known and which have been reported on in many previous studies. I would encourage the authors to highlight the novel results from their analysis.

SPECIFIC COMMENTS

Page 17492, line 9: Why was 1999 chosen as the breakpoint in your trend analysis

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given that ODS concentrations in the stratosphere peaked long before then? Is it an issue that your selected breakpoint is 5 years later than in the Hadjinicolaou study cited on line 12 of page 17493?

Page 17493, line 11: Is it the polar chemistry or the ozone that's exported to lower latitudes?

Page 17493, line 12: No mention here of changes in stratospheric aerosols, especially sulfate aerosols? The Mt. Pinatubo eruption caused a far bigger decline in total column ozone over northern midlatitudes than ODSs.

Page 17494, line 9: True but they ignore the huge chemical effects that the volcanoes had on ozone. To what extent does ignoring e.g. Pinatubo, which significantly suppressed ozone close to the time of maximum ODS concentrations, affect your conclusions?

Page 17495, line 2: What are the differences between these two ERA data sets and how did you deal with these differences during their overlap period (1989-1999)?

Page 17495, line 25: But EESC didn't peak in 2000.

Page 17496, line 22: These EESC parameters are appropriate for the Antarctic stratosphere, but what did you use for other regions? If you used the same parameters for the whole global, your EESC will peak too late outside of the Antarctic stratosphere and I suspect that this will affect your attribution. Maybe you are saved by the fact that the attribution happens inside the same model that ran from the prescribed EESC, but I think that this point needs to be clarified in the manuscript text.

Figure 1: I have serious concerns about how the data are plotted in Figure 1. It is stated in the figure caption that the right hand axes are shifted with respect to the axis in the left to offset the differences between the measured and modelled ozone. But having the E4 and EI axes also shifted with respect to each other hides the huge differences in ozone between the two simulations that result from the temperature biases between

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the ERA-40 and ERA-Interim reanalyses. The difference in northern midlatitudes is 21 DU. That's almost as large as the long-term decline. This will leave readers feeling very unsettled.

Page 17498, line 27: Is it just the wind fields or also the temperature fields that drive these large differences? Also on the same line I would suggest replacing 'Ozone variability' with 'Ozone interannual variability'.

Page 17499, line 3: Is the aim here to quantify the ability of the model to reproduce inter-annual variability, or to quantify the ability of the model to reproduce the ozone changes in general? You have done the latter but in some ways imply the former. The high R squared values that you are getting result in large part from the long-term secular trends in ozone driven by changes in EESC. So this is not a test of the model's ability to track the year-to-year changes in ozone. For that you would first need to regress both the observations and the modelled total column ozone against EESC, remove the EESC signal, and then calculate the R squared correlation between the anomalies. That would be a more representative test of the model's ability to track the observed interannual variability.

Page 17500, line 21: But that's no reason not to apply a linear least squares regression model. You can perfectly adequately fit the regression model to the data over the full period and then use the EESC fit coefficient to say what the effective linear trend would have been over e.g. 1979-1999. It is simply a way of reporting the sensitivity of ozone to EESC in a manner that people are more familiar with i.e. given that sensitivity, what trend in ozone would be been induced by EESC over the period 1979-1999.

Page 17500, line 23: You can use the whole 32 year record in one piece. Just include a basis function of the form A*ERADiff where ERADiff is set to zero whenever you are using a model value from a run using ERA-40 and set to 1.0 whenever you are using a model value from a run using ERA-Interim. The value of the A coefficient will be the mean offset between the ozone from E4 and EI. Better still you can even then apply

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the regression model to ALL of your model output i.e. it doesn't matter if you feed into the model two values for the same year, one for EI and one for E4. Try it. It will work and it will make your analysis more robust and much cleaner.

Page 17500, line 27: There is one problem with this approach - the linear fits in the two different period are likely to be 'misaligned' at the pivot point (1999/2000 in your case). If that is indeed what you have done, then you have not made full use of the information available to you. In that case I would strongly suggest that you follow the methodology outlined in Reinsel, G., E.C. Weatherhead, G.C. Tiao, A.J. Miller, R. Nagatani, D.J. Wuebbles, and L.E. Flynn, On detection of turnaround and recovery in trend for ozone. J. Geophys. Res., 2002, 107, (D10), 10.1029/2001JD000500. It really is quite easy. You do a single regression model fit to the whole time series but you include two 'trend' basis function i.e. O3= A*Trend1 + B*Trend2 where say Trend 1 is the year number after 1979 and Trend 2 is the year number after 2000 but (and this is important) is set to zero before 2000. The A coefficient then gives you the trend up to 2000 and A+B gives you the trend after 2000, and the two parts are *forced* to join at the transition year.

Page 17501, line 7: Do you mean residuals? And it's not clear to me how you obtained the uncertainties on the fit coefficients from the residuals. Usually the fit coefficient uncertainties are obtained from the diagonal elements of the covariance matrix after the regression has been run. You need to give more details on how these uncertainties were calculated. I don't believe they have been calculated correctly. See e.g. Tiao, G.C., G.C. Reinsel, D. Xu, J.H. Pedrick, X. Zhu, A.J. Miller, J.J. DeLuisi, C.L. Mateer, and D.J. Wuebbles, Effects of autocorrelation and temporal sampling schemes on estimates of trend and spatial correlation. J. Geophys. Res., 1990, 95D12, 20507-20517. and Weatherhead, E.C., G.C. Reinsel, G.C. Tiao, X.-L. Meng, D. Choi, W.-K. Cheang, T. Keller, J. DeLuisi, D.J. Wuebbles, J.B. Kerr, A.J. Miller, S.J. Oltmans, and J.E. Frederick, Factors affecting the detection of trends: Statistical considerations and applications to environmental data. J. Geophys. Res., 1998, 103, (D14), 17149-17161.

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Page 17501, line 12: No, the agreement of trends does not in any way indicate that your model captures the pattern of variability. The pattern of variability could be completely different but still have the same trend as the observations.

Figure 6: Something is very wrong with this figure. There is absolutely no way that the SON total column ozone trends over Antarctica from 2000-2009 are negative and even more unlikely that they are larger than the trends from 1979-1999 shown in Figure 5. This just absolutely does not make sense to me. At best (worst) ozone over Antarctica has been about constant since 2000 but more likely has increased slightly. Plot the data and look. I can only assume that this figure is for 1979-1999 and not for 2000-2009 as is stated in the figure caption. If that is the case then its also not clear to me how/why this figure would be substantially different from Figure 5.

Page 17502, line 4: True but your analysis is still showing statistically significant negative trends over the Antarctic from 2000-2009 even though EESC over the period was declining.

Page 17502, line 8: You see I think this problem will go away when you follow the approach I have listed above where you include two linear trends terms in the regression. This 'pins' the trend line early in the period and doesn't allow it to 'float' as is currently the case. Please try the Reinsel approach - I think it will solve a lot of your problems.

Figure 7: I wonder how different this figure would look if the analysis was also done using ozone number density rather than ozone mixing ratio? This would be more relevant for interpretation of the source of the observed/modelled trends in total column ozone.

Page 17503, line 7-9: How does this conclusion mesh with the findings of Engel, A., T. Möbius, H. Bönisch, U. Schmidt, R. Heinz, I. Levin, E. Atlas, S. Aoki, T. Nakazawa, S. Sugawara, F. Moore, D. Hurst, J. Elkins, S. Schauffler, A. Andrews, and K. Boering, Age of stratospheric air unchanged within uncertainties over the past 30 years. Nature Geoscience, 2009, 2, 28-31.

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Page 17503, line 10: How do you know that its the influence of polar ozone depletion and not just increasing halogen driven gas phase ozone depletion?

GRAMMAR AND TYPOGRAPHICAL ERRORS

Page 17492, line 2: Replace 'ODS' with 'ODSs' and likewise throughout. The convention is for ODS to be 'ozone depleting substance' and ODSs to be 'ozone depleting substances'. I would suggest that you simply follow the convention. Right now you sometimes use ODS a singular e.g. on line 9 of page 17492 and sometime as plural e.g. on line 12 of page 17492.

Page 17492, lines 4-5: Replace 'in chemical composition' with 'in the chemical composition of the stratosphere'.

Page 17493, line 8: Replace 'understanding about a possible' with 'understanding of a possible'.

Page 17493, line 9: I would say that its more than desirable. It is necessary.

Page 17496, line 2: Replace 'on temperature' with 'to temperature'.

Page 17496, line 14: Replace 'in form' with 'in the form'.

Page 17499, line 3: 'time series' as two words - make the same change elsewhere.

Page 17499, line 23: Replace 'in dependence of' with 'as a function of'.

Page 17499, line 29: Replace 'in parts' with 'in part'.

Page 17502, lines 15-18: This sentence makes no sense to me. Please rewrite it and improve clarity.

Page 17502, line 25: Replace 'good agreement to' with 'good agreement with'.

Page 17502, line 27: Replace 'trend in in the' with 'trend in the'.

Page 17504, line 18: Replace 'air mases' with 'air masses'.

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