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Interactive comment on "Interannual variation patterns of total ozone and temperature in observations and model simulations" *by* W. Steinbrecht et al.

Anonymous Referee #1

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This paper analyzes 20 years of observations of total ozone and temperature in parallel with two 20-year simulations by versions of the ECHAM chemistry-climate model. For the data and the simulations, they do a parallel analysis using a statistical time-series model to do a regression fit.

I have some serious issues with this paper that should be addressed by the authors.

General Comments: One of the great values of parallel analysis of simulations and data is the ability to quantitatively test the response to specific perturbations of the atmosphere. This paper would be improved by showing some quantitative results,

either in tables or in simple line graphs. I thought that Figure 2 showing the percent variance captured by the statistical fits was revealing. One of the models did not do a very good job of capturing what was supposedly put into it. Some time series plots may have reveal what was happening. I am always bothered by seeing lots of results from time-series models without any original time-series plots. Half to two-thirds of the contour plots could be removed and a few time series and zonal mean plots could be added to illustrate particular points. I believe that this would increase the readability of the paper and help the authors better shape some significant conclusions.

1. It was not clear exactly how the regression model was applied to the data or the model simulations. I worry about the multiple "dynamical" surrogates that may not be independent of one another. The authors address this issue by pointing out that they used the most signficicant first and then discarded those that were not significant to avoid the problem of partially redundant terms. But, it was not clear if that was done to each data point independently or to each data set independently. Did all points in one of the graphs come from a fit to the same parameters?

2. The fits to the E39/C model simulation shown in Figure 2 fail to capture what the authors term a signficant amount of the variance. In Figure 3, the trend fit is not significant over a large part of the globe. This is occuring in a model that is being driven by a trend in chlorine and by the other factors included in the fit. Why is the statistical time-series analysis unable to retrieve the terms that were put into the model? Without a plot of at least some time series, the reader is left to guess. This appears to negate the conclusions about that model. I would conclude that this analysis fails to "validate" the E39/C model for long-term trend analyses. An even stronger statement would be that it invalidates the model for this application. This is not surprising with its 10 hPa lid. We are becoming far too uncritical about the failures of our models when compared to data. At a recent CCMval workshop in Boulder, Colorado, these two models and several others were compared to data in a "process-oriented" validation exercise. They both fared quite poorly on some tests like the N2O-NOy correlation. While I realize that

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there is nothing the authors can do in this paper to magically make the models better, I do think that they need to emphasize both the successes and failures of the models.

3. Trend term: Using a linear trend through the end of 1999 is probably not the best surrogate for the chlorine/bromine effect on ozone. The trend term results for total ozone for MAECHAM clearly show a bias with respect to those for the data. It is impossible for the reader to evaluate this from the postage-stamp sized color contour plots. A latitude dependent zonal mean trend from the two, with error bars would be much more revealing. Do they have the same shape with latitude? Do the error bars overlap? Also the result for E39/C is basically not significant over much of the globe. Including the zonal mean on the plot with the data would show the degree of disagreement or agreement and the error bars would help the reader evaluate signficance. The explanation that MAECHAM gives more trend because it has a larger column is not necessarily true. The sensitivity to chlorine may or may not be related to the total amount of ozone depending on the reason for the bias in total column ozone. One can write out partial derivatives to show, for instance, that if the problem is the photolysis of O2, then the sensitivity is not affected. A residual circulation that is too fast may lead to a larger column ozone at high latitudes, but a reduced sensitivity. The balance between strength of residual circulation and mixing processes may lead to a different result.

4. Trend term for temperature: Trends derived from reanalyses are not generally reliable. In recent UNEP/WMO ozone assessments, trends have been independently derived from the original data. So I am very suspicious of any results of trend analyses of the NCEP reanalyses. In this and almost all of the other figures, the results for 400 hPa don't seem to add anything to the results of the paper. I would eliminate them, or reduce them to one or two salient points.

5. QBO term: I had trouble understanding the statement that twice the standard deviation is plotted. Perhaps I show my ignorance, but I thought that the regression model was returning a coefficient for the fit to the particular surrogate with error bars and that the coefficient would be what was plotted with the error bars determining significance.

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By the way, the paper doesn't really specify how the error bars are calculated. I am also not sure how much importance to give to the fact that the models get a reasonable fit to QBO since they were forced to have a QBO in winds that were nudged to observations. Perhaps there is some significance to the temperatures and total ozone responding correctly to the winds.

6. Solar term: Same comment on the NCEP reanalyses. I am not sure that one should believe any solar cycle derived from the reanalysis. One should always remember that a reanalysis is a combination of data and a model. I think that if you examine the error bars, you would find that the zonal asymmetry of the solar coefficients is not signficant.

7. Polar vortex strength: I am not really sure what these mean. A closer examination might be revealing, but the brief overview in this paper didn't do much for me. Also, I would say the "ozone-hole conditions" have never occurred in the Arctic. This is somewhat a matter of definition as to what you mean by such conditions.

8. 400 hPa graphs: In several places the paper says it includes these "for completeness. They are mostly not necessary. The paper already has too many small panels with almost unreadable results.

9. Conclusions: I feel that this evaluation of the models is too uncritical. Models are an attempt to include as much of our knowledge as is possible in a consistent framework. Models are limited by lack of knowledge and lack of computer resources. We build such models to try to help us better understand how the atmosphere works. We can learn about the atmosphere from both the success and the failures of a particular model.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 9207, 2005.

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Interactive Comment