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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Spatial regression analysis on 32 years total column ozone data" *by* J. S. Knibbe et al.

Anonymous Referee #3

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This paper presents a variety of statistical regression analyses on a 32-year gridded data set of total column ozone. The data are assimilated from a variety of satellite-based measurements, with offsets accounted for using ground-based measurements. The authors explore several avenues of analysis, including the use of physical parameters to represent the seasonal cycle in ozone, the use of month by month analysis to characterize the seasonal variations in the various proxy fits, the use of EESC vs. piece-wise linear trend to best describe the long-term changes in the ozone time series, and the use of EESC with different age properties and PWLT trend fits with different recovery periods to describe the long-term changes. This is an interesting analysis and an example of how regression techniques can be used to test the data, but I worry that there is significant over-interpretation or misinterpretation of the regression results, as

it is very difficult to assign causality when so many inter-related variables are in play.

As an example, the variable DAY is used as a physical proxy for ozone production due to exposure to solar radiation. However ozone production occurs primarily in the tropical middle/upper stratosphere, and then is transported to higher latitudes. I do not believe the strong correlation between DAY and high latitude total ozone is a result of ozone production in the polar regions, but rather a reflection of the Brewer-Dobson circulation and ozone transport that varies seasonally, as does DAY. In fact DAY is basically the forcing for all other dynamical variables, so it will be impossible just using a regression model to interpret the myriad of ways DAY effects total ozone. That's not to say one can't use DAY in the regression, but it can only be interpreted as the net effect of seasonal-scale variations on total ozone.

I believe this work can be published, but a number of significant issues must be addressed first. As mentioned above, extreme caution should be used in assigning cause to some of the fits. Also, the error analysis has to be better defined (see comment below). There are some interesting results, but I felt the follow-up discussion of these results got a bit lost or was included as a last thought in a section rather than highlighted. In some cases, a direct comparison of reconstructed time series from different fits would be helpful, rather than trying to compare results from the full regression fit. I outline some examples below.

Specific Comments:

It is not clear how the statistical errors are defined in the regression model. The authors state that the error term E is assumed to be uncorrelated, but we know that ozone time series are correlated in time, and this should be accounted for by some means. This will not change the regression coefficients, but will affect the uncertainty assigned to the coefficients. The autocorrelation must be included, or if it is in the current model, an explanation of how is needed. A thorough and consistent definition of errors is required. I do not like but accept the definition of physical vs. statistical model, when in reality all the models presented here are statistical. The primary difference between the STAT and PHYS models is the ozone seasonal cycle definition. The use of physical variables compared to harmonics to represent the seasonal cycle in ozone is an interesting approach, though I'm not sure how useful it is in the final analysis. To the extent that the 'DAY' variable can be itself defined by Fourier harmonics the regression model will not be able to distinguish between the two. As stated above, because total ozone is already a net effect of altitude-dependent processes, it may not be possible to truly represent the physical processes that control total ozone. I would be very interested to see if DAY is accurately reproduced by harmonics, and if not, what are the differences? Is there some subtle shift in timing that might lead DAY to produce a better fit? It's very hard to tell without a direct comparison. In my experience the seasonal cycle is typically represented by 4 sets of harmonics. Testing if two or four sets better described the DAY variable would also be interesting. If DAY is described by harmonics, than the two approaches can be used interchangeably. I must admit I am a bit confused about the use of the other dynamical variables to represent seasonal cycle. It seems like these represent variations from the seasonal norm (more/less wave motion, stronger/weaker vortex). Do the authors mean the seasonal cycle to be all dynamical variations that repeat year to year but also accounting for year to year variability? Would it make sense to remove the seasonal cycle from GEO, for example? The seasonal variability of GEO should be highly correlated with DAY, but the anomaly from the seasonal cycle might capture more variability? The authors might demonstrate this by adding the GEO and DAY terms together (i.e. adding the resulting ozone signal from each), and comparing to the "Fourier terms."

I think it might help for the authors to carefully consider the motivation for each experiment and the results. For example, what is to be gained from the analysis of the seasonal variation of the proxy fits? Is the goal to increase the degrees of freedom used in the fit? If so, a little more discussion of the degrees of freedom, and how that effects the significance of the results in a 32-year time series would help. The motivation might also be to determine in what conditions a harmonic series cannot be used to represent

the seasonal cycle, such as is found for EESC. This to me was more interesting. In cases where the seasonal fit was harmonic, it seems the P-value approach would accomplish the same result as pre-defining the season variability, but it is good to know that the fits were harmonic. One could also directly compare the seasonal variations from each approach (but in the STAT version use only the harmonics, not harmonics + alternate variable)... this is more direct than trying to interpret a comparison of the full regression results.

I do not understand why the STAT model, with harmonics for each regression term, also includes the "alternate variables" with the pre-defined seasonal term. It seems this is "double-fitting" the seasonal variation and confusing the comparisons.

There is some confusion on the use of equivalent latitude vs. latitude. If I read it correctly, the data are averaged in equivalent latitude to get the seasonal dependence of the proxies, but otherwise analysis is done at grid locations. This should be clarified. Both GEO and PV should account for dynamical variability in ozone.

One of the unique aspects of this work is the analysis of the gridded data. In that regard some additional discussion of the characteristics of the gridded data would be useful. For example, the reason regression analyses are typically done on time and spatially averaged data to that by averaging the data, short time or small spatial scale variations that cannot be captured by the regression model are averaged out. Also, total ozone is also spatially correlated, and the assimilation process likely has a further smoothing effect. For example, the authors might comment on the significance of the spatial variations in the ozone recovery rates (Figure 12) or on the variability in the gridded time series relative to say the zonal mean. Just a little more focus on the statistical properties of the gridded means vs. zonal means.

The authors should consider including two regression terms for the volcanoes, rather than a single AERO term. The effect of aerosols on ozone will vary between eruptions because of the chlorine content in the atmosphere, and interaction with the seasonal cycle. See recent papers by V. Aquila for modeling of Pinatubo and more recent references on aerosol effects on ozone. By including both volcanoes in one term you are forcing the response to be linear to the anomaly, which is likely not the case. A few more details on the Pinatubo results could be added as well. As the authors note, the older papers did not account for dynamical variability. Do the recent quoted regression papers (Brunner/Wohlmann/Kuttippurath) also indicate a significant effect of Pinatubo in the Antarctic even when dynamical variability is taken into account? A short summary of their results will help the reader understand the degree of the discrepancy that the authors point out.

The authors might consider focusing on a few of the results where they get different results than expected or than in previous studies, and try to characterize these results in context with previous studies. There should be several L. Hood references regarding the solar cycle fits (Soukharev and Hood, JGR, 2006 for example). There should also be examples of using regression fits to identify the turn-around point in a piece-wise linear trend analysis for comparison.

When sensitivity to EESC age of air and PWLT turn around points are discussed, the authors should reiterate upfront the reason for the sensitivity tests. Are we testing the data to identify expected physical parameters, or are we using the data to define physical parameters? It is a very subtle difference. For example, the authors note that for physical reasons, the EESC parameters vary as a function of latitude and altitude, and certain parameters are most physically representative in certain regions. But then the authors use the data to determine the best parameters. The authors should reiterate the physical characteristics of EESC, and discuss why they agree or disagree with the data fit results. Do the fit results reflect what we know physically about EESC in the tropics, or at high latitudes? Are the results in Fig. 13 statistically significantly different, or is one age just slightly different from another?

Similarly a little more discussion of what would be expected from the PWLT if the modeled turn around point occurs before or after the actual turn-around point. Based on

the results in Table 7, the turn-around is prior to (or possibly at) 1997 in the maximum ozone recovery rate case (see next point also). The later beyond the true turn-around the date is placed, the greater the recovery rate (the model has less time to get back up to the level of the data, as it continued to decrease while the data stabilized or increased). Placing the turn around at a particularly low or high value will also have a large effect. Again, are the differences in Fig. 13 significant, or is one fit very close to another? A closer look at the Bogata vs higher latitude results might help. Are the time series characters and turn around points truly different?

Is Table 7 the maximum ozone recovery rate or average? The table says maximum but the text says average.

It is not clear what is causing the difference between the PWLT and EESC/EESC_2 results, but the authors should consider looking into this more and commenting in this paper. Do we expect clear recovery in the ozone hole region yet? There is variability but we still have complete loss of ozone in a defined altitude region. Might this be why the significance is low?

There is a section of the conclusions that repeats information presented just prior. Maybe these sections can be combined.

Technical Comments:

L46: The observation of an ozone hole (otherwise implies it first occurred in 1985) L52: Newman et al., (add period) L52: Political action was taken L67: averaged in equivalent latitude coordinates L74: Later, the equivalent. . . L75: EESC chlorine and bromine (not 'ide') throughout L75: was introduced to represent the net effect of chlorine and bromine on ozone L81: and Pinatubo (remove the) L85: often represented by the eddy heat flux L87: Weber et al., (add period) L88: is the eddy/jet stream interaction EP Flux or EP Flux divergence? L91: forms a boundary between polar and mid-latitude stratospheric air and acts to isolate polar stratospheric ozone. L97: seasonal variability of external forcings on ozone (re-word first part of sentence) L106: gain a better understanding L120-121: Remove “Ozone recovery. . . with respect to ozone recovery” sentence. L129: analysis of seasonal ozone L161: chlorine L162: as an explanatory variable L175: compare regression results L190: remove accuracy L203: eruptions dominate L219: ENSO signal affects the dynamics of the lower stratosphere, including the amount of ozone L227 the dynamical features in the stratosphere such as the polar vortex are highly affected L229: measure of the force L233-324: These effects. . . more details here. The effects of what? The strong vortex? Also explain build-up phase. L243: ECMWF L243: HPa not mbar (standard units) L246: pressure levels L259: includes L307: captures/follows/represents (instead of aligns) L280: analysis done along lines of equivalent latitude? L319: November. L375: units; 27 what? L383-384: I remember El Chichon having a larger effect in tropical latitudes. This could be a result of not separating the AERO signal for the two volcanoes. The larger Pinatubo effects swamp the El Chichon effects. L398: notion that the amount L410: DAY and EP L442: model’s performance L475: Results are significant L495: /year (check throughout for /y) L504: missing period. L564-569: are the changes to QBO/ENSO between models related to the introduction of other variables? Is it possible that what is thought to be QBO is correctly apportioned to one of the new variables in STRAT? L592: remove extra comma L871: chlorine/bromine L897: maximum or average? L923: South to -55? Poleward of -55S L931: the figures did not come out well when printed and were difficult to read from the computer. Not sure if you can fix this, or if the editor needs to fix. These plots are very useful, but difficult to compare. Same for figures 8 and 9. L969: White regions

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