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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Statistical diagnostic and correction of a chemistry-transport model for the prediction of total column ozone" *by* S. Guillas et al.

Anonymous Referee #1

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Review of "Statistical diagnostic and correction of a chemistry-transport model for the prediction of total column ozone" by Gullias et al., Atmospheric Physics and Chemistry, 2005.

GENERAL COMMENTS

This paper presents a method for making data based corrections to model output. I am not sure that much is gained by doing this but in principal it can be done. The paper is suitable for publication after the comments and suggestions detailed below have been addressed.

I am guessing that this technique only works well if it is applied in the near future. I am not convinced that if the technique were applied to 'correct' model output e.g. for a run out to 2050 that it would necessarily improve the model results. Feedbacks would likely change the dependence of ozone on the basis functions over a longer period. Perhaps something needs to be said about this?

Section 2 has a lot of detail about the UIUC model. Does the reader really need all of this detail? I felt I could judge the merits of your proposed model correction method without needing to know anything at all about the UIUC model. I think that you could make your paper much shorter and much more accessible to the average reader if you shortened this discussion of the model details to just one short paragraph with pointers to other publications where these details are presented.

There are an awful lot of tables in this paper and its not clear to me how they serve the reader. What is the reader to take away from them?

SPECIFIC COMMENTS

Page 10422, line 25: I am glad to see people are starting to recognize the inability of these models to capture the interhemispheric differences in the response of ozone to Pinatubo.

Page 10423, line 27: I doubt that your statement 'For true future scenarios, say over the 21st century, our method not only improves the seasonal representation (e.g allowing for more accurate predictions for a particular month), but can also examine scenarios for given QBO and AO' is true given that feedbacks, e.g. GHG induced changes to stratospheric temperatures and dynamics, would alter the dependence of ozone on the basis functions. Please provide evidence that this does not constitute a problem for your proposed method.

Page 10427, line 18: Why 'so-called'? They are the monthly zonal means and that's all there is to it.

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Page 10427, line 24: What do you mean by UV's?

Page 10428, lines 1 to 7: It wasn't clear to me why you were discussing the lack of spatial coverage in Vitali Fioletov's global ground-based total column ozone data set when you aren't even using it.

Page 10428, line 9: There is no 'can' about it. The eccentricity of the Earth's orbit does cause changes in extraterrestrial solar irradiance of 7%.

Page 10428: Discussion of the AAO and AO indices: again I think you could shorten the paper by providing references to how the AAO and AO indices are defined. All of this discussion of the application of EOF analyses to geopotential height fields isn't the point of your paper and I feel that it distracts the reader. Unless it is different to the standard approaches used to derive the AAO and AO indices, I would just cite 1 or 2 papers for their source.

Page 10428, lines 26 to 29: It wasn't clear to me exactly how you dealt with the seasonality of the effect of the QBO on ozone. It sounds like you expanded the QBO regression coefficient as A = A_0 + A_1*sin(2*Pi*t/12) + A_2*cos(2*Pi*t/12). If so good, but probably not good enough. I think you need at least one more Fourier pair, i.e. + A_3*sin(4*Pi*t/12) + A_4*cos(4*Pi*t/12) to adequately account for the fact that the QBO is more effective in winter and spring in influencing ozone.

Page 10429, line 7: It is not at all clear to me from Figure 1 that there is a discrepancy in the seasonal cycle in ozone between the measurements and the model. Why not show the mean seasonal cycles from the measurements and model e.g. something along the lines of Figure 2 of Struthers, H., K. Kreher, J. Austin, R. Schofield, G.E. Bodeker, P.V. Johnston, H. Shiona, and A. Thomas, Past and future simulations of NO2 from a coupled chemistry-climate model in comparison with observations, Atmospheric Chemistry and Physics, 4, 2227-2239, 2004. This would show much more clearly the differences between the measurements and the model and more specifically the differences in the mean annual cycle.

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Page 10429, line 21: You state that 'These tests are very useful for the modelers, since they can improve the physical representation of their models'. The tests themselves do not improve the models at all. The models need to be changed based on the results of the tests.

Page 10429, line 22: It is not at all clear to me what you mean by 'The selection of criteria that highlight orthogonal model processes helps understand the model behavior'.

Page 10430, line 24: Monthly indicators of what?

Page 10431, lines 8-23: This description of the regression model is unnecessarily complicated. Just say something like O3 = a + b*t + c*QBO + d*AO +e*SOLAR where the coefficients (a to e) are expanded into fourier components as e.g. a = a_1 + a_2*sin(2*pi*t/12) + a_3*cos(2*pi*t/12) to account for seasonality in the coefficients. In fact it is not clear to me why you deal with seasonality in the trend by applying the model individually to each month (that is effectively what you are doing with your 'monthly indicators') but deal with the seasonal dependence in the QBO by expanding its coefficient into the first two fourier components. Why not just deal with seasonality in the same way for all of the terms including the constant (a above) which would then simply become the stationary component of the annual cycle. Have a look in Randel, W.J., and J.B. Cobb, Coherent variations of monthly mean total ozone and lower stratospheric temperature, Journal of Geophysical Research, 99 (D3), 5433-5447, 1994 for a good clear example of how to describe such a regression model.

What is the scale in Figure 3? dobson units?

Page 10432, line 4: You state that 'The 5 standard errors, which depend on the latitude and the month considered, are usually less than 3DU at the midlatitudes and much less near the equator' but I really couldn't see this in Figure 3.

Page 10432, line 7: It's not clear to me what you mean by 'we accounted for the circulation through the QBO and the AO' and likewise a few lines later.

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Interactive Comment

Page 10437, line 12: I believe you are being overly optimistic thinking that 'This would give model developers some additional knowledge in order to scientifically improve the treatment of chemistry and transport processes'.

GRAMMAR AND TYPOGRAPHICAL CORRECTIONS

Page 10428, line 14: Is the word 'projected' missing here? and again on line 17.

Page 10428, line 23: The abbreviation for milli-bar is mb not Mb and I would anyway prefer to see hPa. Likewise a few lines later.

Page 10436, line 9: Replace 'is could' with 'could'.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 10421, 2005.