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

S. Guillas et al.

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Below is a point-by-point list of changes to the manuscript, following the referees' comments.

Anonymous referee #1

GENERAL COMMENTS

It is possible that if feedbacks were to greatly alter the fundamental dynamics affecting the distribution of stratospheric ozone, then the approach used here would be

inadequate for long term predictions. The approach we used may be useful for multi-decadal analyses, given that using a past learning period worked well for the recent decade, but we really don't have any evidence at this point for how adequate it is or isn't for future long term changes.

Some readers are likely to want to see the longer model description.

Tables: yes, as referees #1 and #2 point out, it is not easy to take away information from them. We followed referee #2's suggestion and use bold numbers to show the relevant information.

SPECIFIC COMMENTS

p. 10422, line 25. Thanks.

p. 10423, line 27: It is possible that if feedbacks were to greatly alter the fundamental dynamics affecting the distribution of stratospheric ozone, then the approach used here would be inadequate for long term predictions. The approach we used may be useful for multi-decadal analyses, given that using a past learning period worked well for the recent decade, but we really don't have any evidence at this point for how adequate it is or isn't for future long term changes.

p. 10427, line 18: removed

p. 10427, line 24: lack of solar ultraviolet radiation

p. 10428, lines 1 to 7: Despite the coarser resolution of the calibrated SBUV-SBUV/2 data set and the lack of coverage for polar latitudes compared to the aforementioned data sets, we used the SBUV-SBUV/2 data set. Indeed, the lack of temporal gaps and the careful calibration are more important for the statistical study of the deficiencies of a CTM.

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Interactive Comment

p. 10428, line 9: Removed

p. 10428: Discussion of the AO/AAO indices removed, and references provided.

p. 10428, lines 26 to 29: Yes, the QBO could be more adequately accounted for by using one more Fourier pair. However, for model parsimony, and thus to get smaller uncertainties in the parameters estimation, we resorted to only one Fourier pair.

p. 10429, line 7: Yes, this could have been done. However, to avoid more figures, Figure 5 (left panel) shows clearly the differences for Northern midlatitudes. A reference to this Figure is added to the text.

p. 10429, line 21: Yes. Text changed accordingly.

p. 10429, line 22: Tests results for specific processes (e.g. associated with transport or chemistry) help understand the model behavior. Text changed accordingly.

p. 10430, line 24: Indicators of each month.

p. 10431, line 8-23: Thanks for the reference. We only accounted for the seasonality in the QBO coefficient, since the other influences do not depend as strongly on the season, and we want to set up a parsimonious model. We dealt with seasonality using indicators of each month, because, as show in Fig. 5, the seasonal cycle in the model is difficult to represent in terms of sines and cosines: There are "kinks" seemingly due to model "swings" in latitudes. Randel and Cobb (1994) proposed another alternative, by removing the seasonal cycle in total column ozone and lower stratospheric temperatures, and then observe the strong association. However, this method, in addition to require twice as many seasonal parameters, can not be used to evaluate seasonal deficiencies of the model. The scale in Fig. 3 is in Dobson Units.

p. 10432, line 4: Yes, it is not shown on Figure 3, but computed, and reported in the text.

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p. 10432, line 7: We accounted for the QBO and AO, so that they are no longer confounding factors that could potentially increase the standard errors of the monthly deficiencies.

p. 10437, line 12: Changed to "this could probably"

GRAMMAR AND TYPOGRAPHICAL CORRECTIONS

p. 10428, line 14: This part is now removed.

p. 10428, line 23: Changed.

p. 10436, line 9: replaced.

Anonymous referee #2

SPECIFIC COMMENTS

1. The main advantage of our method is to combine the detection of deficiencies involving processes not captured by the model (e.g. QBO, AAO, AO,..) and captured by the model (annual cycle, solar cycle,..). The other advantage is that we are able to assume only linearity in the second order: Only in the second step the model inadequacies are assumed to be approximately linearly related to the explanatory variables. The alternative method (removing these effects on the data, and on the model outputs) would leave us with two data sets of residuals, obtained through probably different parameters in linear regression and some differences. We would not only assume linearity in the initial regressions, but we could not exploit as much the results as with our approach since these two data sets may differ in their construction. As for the possibility of associating

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the prediction with different chemical or physical processes, the referee is right. This would be a major advance for modelers. It is actually under investigation, through experimental design techniques, and is beyond the scope of this paper. What is gained through this method is the possibility of detecting deficiencies and improving predictions, not yet attributing deficiencies to specific processes.

2. In Table 6, the numbers c and a are respectively closer to 0 and 1 than in Table 4. Furthermore the RSE is always smaller. Indeed the last two columns of Table 6 report the RSE for the raw model (same as last column of Table 4), and the RSE for the adjusted model.

MINOR COMMENTS

The mean age of greater than 5 years found at high latitudes in the mid- to upper stratosphere agrees much better with available data than the roughly 4 years found in the older version of the model used for the M&M II analyses.

We highlighted the significant entries using boldface characters.

It is somewhat both. It may be that the model is just inadequate for treating the S.H., but if so, it is because of missing physics that is not that well understood, at least in terms of representing processes in a 2-D diabatic driven model.

corrected

Probably both, but we know 2-D represents the polar vortex very poorly.

to estimate deficiencies and modify

Yes, the referee is right.

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