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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.

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Introduction

To begin we would like to thank the anonymous reviewers for their comments. Below, we will reply on the questions and statements from the anonymous reviewers and announce several adjustments to the manuscript in order to clarify several addressed issues in a revised version.

The key points of this paper are threesome. One, it is the first ozone regression study performed in longitude and latitude dimensions on total column ozone data. Two, a more physically oriented model (PHYS) is applied and compared to the conventional 'statistically oriented' regression model (STAT) that uses harmonic functions to param-

eterize seasonal variability in ozone. And three, we estimate the ozone recovery rates, which are currently under large debate, using piecewise linear regression methods and EESC based methods. Though the reviewers acknowledge that the methodology is mathematically sound, clarifications are in place on several issues. The main concerns of anonymous reviewers #1 and #2 are on the robustness of several results and both reviewers address that several results need to be interpreted more extensively. Anonymous reviewer #1 found the results to be a bit disappointing and feels that more emphasis on new results is desirable. The main concern of reviewer #3 is on the seasonal characterization of ozone in the PHYS model and the difficulties in the interpretation of these results, especially considering the variable DAY. All comments have been accompanied with a list of specific and technical comments. Below we will first provide a general reply to elaborate our view on the matter, followed by addressing the specific comments in replies posted for each reviewer individually.

General Reply

One main concern was the robustness of the results. To date, most ozone regression models use either harmonic series to model seasonal variation of ozone or perform the regressions on yearly values. The variation in ozone dependencies throughout the year with explanatory variables is accounted for in similar manner; either by multiplying the explanatory variables by harmonic functions up to several frequencies or taking yearly values. Results of these studies are consistent from a geographical point of view (in particular the latitude dependence). In that sense they are robust. This is not such a surprise, as the applied models are very similar. However, a better understanding of what drives seasonal ozone variations is desired and a complete spatial analysis of total ozone variability is still lacking. Additionally, this may affect the statements that are to be made about long term ozone variations. The reason is that harmonic functions in the STAT model may account for variations that should be attributed to other variables or their seasonal adjusted variables and, contra, long term explanatory variables may account for variations that may be explained by year to year variations or anomalies in related highly seasonal variables. For the long term variables (those of group A) therefore we claim that robustness of the results is exactly what we test with respect to the conventional STAT model. Most results corresponding to these variables, as noted by reviewer #1, were similar to that of previous studies on equivalent latitudes. However, some differences are noted, e.g. the EESC_2 results are largely affected by the harmonic functions in the STAT model and the PHYS model shows more effect of EESC and AERO at northern high latitudes, whereas the STAT model attributes more variation to ENSO and the QBO at northern high latitudes. We will emphasize such results more extensively in the revised version.

The robustness of results and spatial patterns regarding the highly seasonal variables (Group B) in the PHYS model is definitely more problematic. Nevertheless, most of the seasonal ozone variation has been accounted for, as the residuals don't show a pronounced seasonal cycle. Furthermore, we carefully selected and tested various combinations of these explanatory variables in preliminary regression runs. These preliminary regression runs showed reasonable robustness of the obtained spatial patterns. However, the intensity of the DAY, EP (and to a smaller extend GEO) signal may be overestimated in the Northern Hemisphere at high latitudes as a result of the large correlations in the region. Less robustness was found in the Antarctic region, which is why we made a choice for including the EP and PV variables in this region. Nevertheless, we agree that interpreting the results from the highly seasonal variables is not straightforward when large correlations are present and we have mentioned that caution is required in this respect. Implementation of methods towards orthogonal explanatory variables could be very useful indeed, as reviewer #2 suggests. However, the prime scope of the paper is to analyze and present results of an previously unexplored domain (full 2D spatial variability of total ozone). We argue that our results provide sufficient information on how ozone relates to these explanatory variables when correlations are properly taken into account in the interpretations. It is beyond the scope of this paper to explore alternative methods for analyzing total ozone variability.

Additional concerns were on the interpretation of results obtained by the variable DAY by reviewer #3. The referee states that DAY is used to describe the ozone production in tropical latitudes but the obtained corresponding results at mid to high latitudes cannot be interpreted as such because ozone transportation towards higher latitudes generates a time lag in the seasonal signal. As we examined our own results more closely, we believe our interpretations indeed need to be restated. Most of the high values (at high northern latitudes) of the DAY variable are strongly affected by correlation features due to DAY-EP correlations. Taking these correlations into account, the results make more sense as we will point out in the specific reply for reviewer #3.

We discuss the specific comments of each reviewer separately in specifically addressed replies.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 5323, 2014.