Interactive comment on “Spatial regression analysis on 32 years total column ozone data” by 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 parameterize 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 issues.

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 emphasise 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 analyse 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 analysing 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.

specific reply

We discuss the specific comments in order of appearance: first the comments of reviewer #2, followed by those of reviewer #1 and to finish with those of reviewer #3. The reply finishes with notes on the technical comments in the same order.

Reply on specific comments by reviewer #2

- Reviewer: "*One of the most puzzling results has to do with the differences noted (see lines 522 and 523) for the EESC versus the PWLT results and the ozone recovery rates in the ozone hole region.*"

The differences in obtained PWLT and EESC based ozone recovery rate estimates are due to the different breakpoints in the linear segments and the shape of the EESC curve respectively. Breakpoints situated later in time will increase the slope of the linear segment in the recovery period to fit the nominal ozone values in this timeframe. For the EESC based estimates the complete EESC curve is fitted to the ozone timeseries. For larger age of air parameters the slope of this curve is less steep in the recovery period, which results in smaller recovery rate estimates. The fundamental difference is that the PWLT estimates are based on 2 individually fitted linear segments, whereas the EESC based estimates are based on fitting the complete curve. The large range in the obtained estimates and their standard deviation is something to worry about. It shows that quantification of ozone recovery rates is extremely sensitive to the applied trend estimation method and on model assumptions. Based on the location, however, it still can be argued which model is more appropriate. For example, the age of stratospheric air increases by increasing latitudes. This is accompanied with an increasing range of possible recovery rates, because the breakpoint at high latitudes is considered to have occurred later in time than around the tropics.

At the end of section 2.4 we added "The piecewise linear trend (PWLT) characterization for long term ozone variation has the advantage that the slope in ozone recovery and ozone depletion periods can be estimated separately, whereas these slopes are proportionally fixed in the EESC curves. On the other hand the EESC parameterization yields a smooth curves instead of the ad-hoc turn around point in the PWLT characterization.". Additionally, we now elaborate on these results in the discussion section.

- Reviewer: *"The results in Table 7 for the SH region are also puzzling in the same way."*

For table 7, we found that the numbers got mixed up. Figures 11 and 12 show the trends on this matter correctly.

This is adjusted in the revised paper, and the description 'maximum' will be changed to 'average' since the numbers are averages.

-Reviewer*: "Line 149 (in MSR ozone section), is the "standard errors" really what you want to mention here, rather than the "standard deviations"."*

We have changed 'standard errors' to 'standard deviations'.

- Reviewer: *"could you clarify why plots do not specify equivalent latitude or was this coordinate system used as a partial analysis which was then redone on geographic latitudes?"*

We have used geographical coordinates for the seasonal analysis. We have changed 'equivalent' to 'geographical' in the first line of section 2.3 in the revised manuscript.

Reviewer: *"L486, why did you not consider a piecewise linear model with two adjacent time periods (such as 1979 to 1997 and 1997-2010)?"*

- We have applied a piecewise linear function in these analyses. So we have modelled the decreasing trend in ozone prior to the breakpoint, but we focussed on presenting the recovery rates because these are currently more interesting.

Technical corrections

- We have corrected all textual comments.

- L381, Reviewer: *"it would be good to find and cite past references (if any) that may have looked at interhemispheric differences for the solar influence.".*

we have looked for publications that report hemispheric asymmetry in solar cycle response on ozone, and now found references with similar results (Hood and Soukharev, 2006).

-L387, Reviewer: *"do you mean that for both pressures, the correlation for total column is positive?".*

essentially we mean that including the QBO of both pressure levels in these regressions with positive regression coefficient yields the best fit. This indeed is due to positive correlation with ozone along the equator, especially at 30 hPa.

- L416/417, Reviewer: *"the statement is not very convincing if not backed up by the reference cited (is it?) or by this manuscript's work.".*

In the revised manuscript we backup this statement with references (Harris et al., (2008); Kiesewetter et al., (2010) and Rieder et al., (2010))

- L426, Reviewer: *"I strongly suggest that the definition of R squared be provided here, to ensure that this is clear to all readers.".*

Although we believe the definition of R^2 is basic statistical knowledge, we provide a proper definition in the revised version.

- Reviewer: *"I can understand the motivation for studying a site over Antarctica, but if you have a motivation for the other two sites, please indicate this (one in the tropics and one at high northern latitudes, is it as simple as that?).".*

The sites are selected almost as easy as you have stated, with the note that Bogota is chosen to properly show the effect of ENSO at the equator. We will include a line about this in the revised version.

Reply on specific comments by reviewer #1

- Reviewer: "It is unclear to me why the choice of explanatory variables is restricted to PV and EP in the SH high latitudes, when EP, GEO and DAY show even stronger correlations in the NH."

Not restricting our analysis to fewer explanatory variables in the PHYS model in the Northern Hemisphere is explained in the general reply above. To summarize: Preliminary regression runs showed reasonable robustness of the obtained spatial patterns. Although 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, 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. 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.

We clarify this issue in the revised paper by inserting the following at the end of section 2.2: "Despite these high correlations at the Northern Hemisphere, preliminary regressions with both of these variables included and either one of them included separately showed reasonable robustness of the obtained results up to approximately 50˚N, whereas at higher latitudes we account for this correlation feature in the interpretations of regression results. For this reason we choose to include both EP and DAY for regressions performed at the Northern Hemisphere."

- Reviewer: *"Is the separation of seasonal ozone variations, described by the physical variables EP, GEO, PV, and DAY (or the harmonics in STAT) from the seasonal response to the non-seasonal explanatory variables of group A unambiguous?"*

The alternative variables (within group A) do not interfere much with the seasonal variables (group B). Multiplication of a variable with an harmonic function does not yield a variable which is dominated by seasonality in the degree that variables of group B are dominated by seasonality. An exception of this is the EESC_2 variable because the short term variability in EESC is extremely low. EESC_2, however, has a very specific seasonal behaviour and a trend within this seasonality unlike any other included explanatory variable. It has been common practice in previous regression studies to multiply explanatory variables with much more harmonic functions, often without noting this subtle detail regarding interference with the Fourier series that conventionally account for seasonal ozone variations.
We added the following at the end of section 2.3 to clarify this issue: "Remark that these alternative variable are not necessarily dominated by the multiplied seasonal function. This is only the case for EESC_2, due to the extremely low short term variations in EESC. EESC_2 shows a very specific trend in this seasonality which is very different from the highly seasonal variables in group B. Therefore, the alternative variables hardly interfere with the parameterization of seasonal ozone variations in the regression models that are defined in the next section."

- p 5335 l 3 ff and Fig 3 top left, also Fig 4 top left: Reviewer: *"I find it surprising that the EESC response is so low in Arctic spring (and in the Arctic in general)."*

We agree that more attention should be given to the fact that we do not identify Arctic catalytic ozone depletion in our results as clearly as we do at the Antarctic in the seasonal analysis. This is due to the Arctic ozone hole occurring more irregular than the Antarctic hole. This is the reason that we do not apply such explicit treatment for the Arctic ozone hole as we do in the Antarctic ozone hole.

We now state in the discussion that the Arctic ozone hole is not clearly detected due to the Arctic ozone hole occurring more irregular than the Antarctic hole and in section 2.3 we state that we do not define an alternative variable for describing the Arctic ozone for this reason.

- p 5337 l 25ff, Figures 4 and 6: Reviewer: "*The zonal asymmetry of the EESC2 (and EESC) regression coefficient is striking, particularly in the STAT model. Is this robust?"*

Yes we find that these patterns are robust. However, there is some model dependence for the EESC_2 results when you compare these figures more closely.

We described this in results section 3.2 and in the discussion section.

- p 5338 l 5ff: 1) Reviewer: *"Why is the solar signal so weak in the area of strongest insolation i.e. the tropics?"*

The solar signal is not very strong in amplitude in (tropical) total ozone (Wohltmann et al.). Solar effects are stronger higher up in the tropical stratosphere, even though in absolute sense the lower stratosphere contributes more (Hood et al., 1997). Solar effects are thus better identified in vertically resolved ozone than in total ozone, and correlations between tropical total ozone and the solar cycle are generally rather weak (Brunner et al., 2006; Wohltmann et al., 2007).

- p 5338 l 5ff: 2) Reviewer: *"The strong and zonally asymmetric solar signal at high southern latitudes is indeed surprising. ... I wonder if there is a compensating mis-attribution between EESC2 and SOLAR?"*

The correlation value between SOLAR and EESC_2 is -0.0069. There is no mis-attribution between these variables. It is possible, however, that in- or excluding EESC_2 in a regression de- or increases the residual ozone variation, so that SOLAR can be found significant more easily. But in fact, this feature is more common, as Soukharev and Hood (2006) found similar patterns in vertically resolved ozone dependencies with the solar cycle.

We added "A similar hemispheric asymmetry, with larger ozone influences at the Southern Hemisphere, is found in the effect induced by the solar cycle, with positive regression coefficients at low- and mid- latitudes for both hemispheres and barely significant features at the equator itself. This spatially persistent but weak solar signal is consisted with results of e.q. Soukharev and Hood (2006) on the solar cycle variation in ozone and Wohltmann et al. (2007)." in the discussion of the revised document.

- p 5338 l 25ff, and p 5345 l 18ff. Reviewer: *"Given the strong correlation between EP and DAY, the attribution to DAY is not so clear to me, especially at high latitudes."*

This point is a continuation of the first specific comment. The regression model does not distinguish well between in situ ozone production and the ozone transport. Results for DAY and EP both at high latitudes, north of approximately 50 ˚N, have been overestimated. Though we made a general statement to be careful with such interpretations, we can address this issue more specifically for these results. Preliminary regression experiments have shown that the contribution for the DAY variable is more persistence at latitudes up to 50 ˚N and the EP contributes more above this boundary. This has been examined by performing regression runs while in- and excluding either of these explanatory variables in turn. Precise quantification of these contributions is very difficult though.

We added “In the interpretations of these results, we must account for the high correlation values between EP and DAY at the Northern Hemisphere. Up to around 50˚N, the positive effect of DAY on ozone is likely due to in situ ozone production driven by exposure to solar radiation. Towards higher latitudes DAY’s regression coefficients are increasingly affected by correlation features with the EP variable complicating direct physical interpretations due to overestimation of regression coefficients” in the discussion section.

- p 5340 l 21 ff., and p 5344 l 6 ff: Reviewer: *“How come this low explanatory power of the PHYS model at high northern latitudes?”*

The lower performance of the STAT and especially the PHYS model at high northern latitudes shows that ozone variations in total ozone variation are less well understood. These statistical models do not appear to be well suited for describing total ozone variations because the persistent seasonal ozone cycle in the northern polar region this seasonal component can be accounted almost any set of explanatory variables that vary on seasonal timescales. Any choice in this set will affect short term and long term ozone variations differently and, subsequently, affect results for non-seasonal variables of group A in these models. In passing we note that the STAT model shows effects of the QBO variables up to high latitudes, whereas the PHYS model shows a more persistent pattern in the results for EESC, AERO and (to a smaller extend) SOLAR in the Northern Hemisphere.

In the discussion section we added “The higher performance of the STAT model as compared to the PHYS model north of 70˚N may be caused by extreme domination of stable seasonal variations in the ozone timeseries, which are better parameterized by the orthogonal harmonics in the STAT model.”

- pages 5344|21ff: Reviewer: *“It is not clear to me why the age of air that fits best should decrease again from the mid-latitudes to polar latitudes – while at the same time the PWLT method gives results more in line with increasing age of air towards the poles, as one would expect.”*

Figure 13 shows an unexpected trend with better fits for decreasing air ages towards the poles for the PWLT results, whereas the EESC results yields a less pronounced trend of better fits for increasing air ages towards the poles (actually mid latitudes) that fits the expectations a little bit better, but indeed still not really as expected. The question on how to interpret these results is a valid one. The fundamental difference between these PWLT and EESC curves must be kept in mind: the PWLT has the advantage that the slopes of the two linear segments can be set independently from each other, whereas the EESC curve has the advantage of being a consistent smooth curve. In regions with low air ages (more instantaneous response to changes) and low latitudes we would expect a more sudden breakpoint in the ozone trend, whereas in regions with large air ages and high latitudes the ozone trend changes more smoothly (more delayed response to changes). This is likely what causes the EESC curve to better fit the data in polar regions. The reason for the better fit of 3 year air age EESC instead of EESC with higher air age parameters is probably related to the difference in ozone response rate on increasing ozone depleting substances and the currently decreasing amount of ozone depleting substances. This may be better represented by the 3 year air age EESC instead of an EESC variable with higher air age parameter. However, given the freedom in choice for the age of air and the EESC shape one should not over-interpret these results. Nevertheless, it remains unclear why the PWLT results in Figure 13 (middle plot) generate a trend of an earlier ozone turn-around point towards high latitudes.

At the end of section 2.4 we added "The piecewise linear trend (PWLT) characterization for long term ozone variation has the advantage that the slope in ozone recovery and ozone depletion periods can be estimated separately, whereas these slopes are proportionally fixed in the EESC curves. On the other hand the EESC parameterization yields a smooth curves instead of the ad-hoc turn around point in the PWLT characterization.". Additionally, we now elaborate more on these results in the discussion section.

- pages 5344ff, Sections 4 and 5: Reviewer: *"I would suggest re-organising the discussion and conclusion sections a bit., ..., I miss some clear conclusions – take home messages – from the current work."*

On reviewers advice we have revised the results, discussion and conclusion section. The most important changes are:
- Moving "The reduced explanatory power At 55˚S is related to the vortex edge itself. Regression studies focusing on the Antarctic ozone hole typically use either a dynamical definition like the equivalent latitude to define the vortex area, or stay sufficiently far away from the vortex edge (south of 70ºS; *e.g.* Kuttipurath et al. (2013)). Hassler et al. (2011) have shown that the shape of the Antarctic vortex has changed somewhat during the last 30 years which has consequences for analyzing Antarctic ozone. However, given that this study focuses on the global patterns of ozone variability, use of a spatially variable definition of the vortex edge is not possible." from the conclusion section to the discussion section.
- Deleting "Three regions show reduced explanatory power in both models: the Antarctic vortex edge region, a tropical belt around 10ºS and a smaller band over the northern edge of Africa extending into central Asia. The band with reduced explanatory power over the tropics and the smaller band over North Africa extending into Central Asia are due to a large component of white noise in the ozone time series." from the conclusion section, because these findings have been sufficiently addressed in the discussion section.

- Adding "As for post peak-EESC ozone trends, the results of our regressions indicate that standard methods for determining trend uncertainties likely underestimate the true uncertainties in the ozone trends that can be attributed to decreasing EESC. Hence, great care has to be taken with discussing the statistical significance of these trends." in the conclusion section and "Based on these observations we conclude that ozone is recovering globally at a rate between 0.2 and 1.7 DU/year and between 0.9 and 3.1 DU/year for the Antarctic ozone hole period specifically. However, given the uncertainties discussed above it is not possible to determine an appropriate trend uncertainty level, hence no statistical significance of the recovery rates can be determined." at the end of the discussion section as one of our take home messages.
- Adding "This first spatial regression study yields pronounced patterns in longitude/latitude dimensions of ozone-regressor dependencies. The effect of ENSO on ozone is mainly identified at the Pacific. We don't find clear indications of aerosol effects on ozone at the Antarctic. The effect of the 11-year solar cycle appears to be more important in the Southern Hemisphere, especially between -50º and 100º in longitudes, which is currently unexplained. And the effect of the southern polar

vortex, clearly identified north of Antarctica, is large on total ozone columns." as second paragraph in the conclusions section as take home message.

- Technical corrections are all applied accordingly.

Reply on specific comments by reviewer #3

- Reviewer: *"The autocorrelation must be included, or if it is in the current model, an explanation of how is needed."*

Though we agree that autocorrelation is present in the ozone time series, it is of no concern for the main objective in our study, which is the spatial patterns that arise in the regression coefficients. The P-value approach is applied here and suffices in our opinion for this purpose since autocorrelation only affects the uncertainty assigned to the regression coefficients. This only needs to be properly evaluated regarding the recovery rates where we make specific statements about significance in ozone recovery rates. Although the uncertainties in these estimates are much more dependent on the age of air parameter for the EESC variable and the modelled turn-around point in ozone for the PWLT estimates than on the autocorrelation, we agree to incorporate autocorrelation in the error handling for these trend estimates in the revised version.

We implement the method described in Press et al (1989) to account for autocorrelations of lag 1 in the error term. This has changed the standard errors of the recovery estimates. A description of this method is included in the revised manuscript, section 2.4, first paragraph.

- Reviewer: "*I do not like but accept the definition of physical vs. statistical model, when in reality all the models presented here are statistical."*

The regression models have been given the labels 'statistically oriented' and 'physically oriented' to emphasize their main difference. We are fully aware that they are both considered statistical regression models.

- Reviewer: *"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."*

The explanatory variable DAY approximates an harmonic function at low to mid latitudes but inhibits truncation of its seasonal pattern towards the poles, since a polar day lasts six months. At low to mid latitudes it is true that DAY can be well resembled by harmonics but so can any function with a strong cycle. The main purpose of these physically meaningful variables instead of harmonic functions is not to achieve a higher explanatory power, but to assign meaning to the specific phase and period in seasonal oscillation by attributing such variation to variables such as DAY, EP and GEO. For this reason we do not find the experiment of comparing DAY and GEO parameterized ozone variability to results characterized by the harmonics a useful suggestion in the scope of this study. We do agree that the PHYS model has the drawback that these highly seasonal variables may explain additional seasonal variation driven by other processes as well and we encourage more investigation to resolve this issue. This study gives our view on how far the current understanding has advanced. For example: the obtained results that correspond to DAY may seem unsatisfactory because we may not expect in situ ozone production at high northern latitudes in the amounts that these results imply. For a large part this is a feature of high correlations with the EP variable. However, we note that the DAY variable explains ozone variability from the tropics polewards, whereas for the EP flux this occurs north of 40 ˚N. The EP flux coefficients also increase towards the southern polar vortex in the Southern Hemisphere. For both hemispheres this is consistent with stratospheric ozone production occurring in the tropics and subsequent ozone transport towards higher latitudes as inferred from the EP dependence. In the revised version of the manuscript we have included this examination of results (discussion section on EP and DAY results).

- Reviewer:*"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."*

The alternative variables (within group A) do not interfere much with the seasonal variables (group B). Multiplication of a variable with an harmonic function does not yield a variable which is dominated by seasonality in the degree that variables of group B are dominated by seasonality. An exception of this is the EESC_2 variable because the short term variability in EESC is extremely low. EESC_2, however, has a very specific seasonal behaviour and a trend within this seasonality unlike any other included explanatory variable. It has been common practice in previous regression studies to multiply explanatory variables with much more harmonic functions, often without noting this subtle detail regarding interference with the Fourier series that conventionally account for seasonal ozone variations.
We added the following at the end of section 2.3 to clarify this issue: "Remark that these alternative variable are not necessarily dominated by the multiplied seasonal function. This is only the case for EESC_2, due to the extremely low short term variations in EESC. EESC_2 shows a very specific trend in this seasonality which is very different from the highly seasonal variables in group B. Therefore, the alternative variables do not interfere much with the parameterization of seasonal ozone variations in the regression models that are defined in the next section."

- Reviewer: *"There is some confusion on the use of equivalent latitude vs. latitude."*

We have used geographical latitudes throughout this study. We have changed 'equivalent' to 'geographical' in the first line of section 2.3 in the revised manuscript.

- Reviewer: *"One of the unique aspects of this work is the analysis of the gridded data."*

We will include some extra emphasis on the spatial variations of the regression results in the revised manuscript. We have included the following text as second paragraph in the conclusion section: "This first spatial regression study yields pronounced patterns in longitude/latitude dimensions of ozone-regressor dependencies. The effect of ENSO on ozone is mainly identified at the Pacific. We don't find clear indications of aerosol effects on ozone at the Antarctic. The effect of the 11-year solar cycle appears to be more important in the Southern Hemisphere, especially between -50º and 100º in longitudes, which is currently unexplained. And the effect of the southern polar vortex, clearly identified north of Antarctica, is large on total ozone columns.".

- Reviewer: *"The authors should consider including two regression terms for the volcanoes, rather than a single AERO term."*

The AERO term represents the amount of stratospheric aerosols at 7 degree latitude bands. Therefore, it accounts for the spatial differences between the Pinatubo eruption and the El Chicon eruption. To account for different chlorine contents, we could use two variables instead of one for each eruption. But this would only matter where both eruptions affect the amount of stratospheric aerosols. Furthermore, including only one variable enables us to take other aerosol sources into account in similar degree. The studies referred to by the referee indicate aerosol effects even after dynamical effects into account, although the signal is not spatially consistent (ozone increase over Antarctica during Austral autumn). Furthermore, only at high (polar) latitudes there appears to be a strong aerosol signal [Brunner et al.]. It is also well known that large tropical volcanic eruptions affect poleward transport (Brewer Dobson circulation), which further complicates the inclusion of aerosols in a multi-variate regression. Additional research (submitted) reveals that due to these complexities, regression results are very sensitive for the exact choice of the aerosol proxy. Given the large uncertainties and freedom in parameter choice we would rather prefer to stick with what has been the standard methodology to include stratospheric aerosols in regression studies.

- Reviewer: *"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."*

The advice on focusing on the results that differ from previous studies is appreciated and taken into account in the revision. For instance, in the discussion section we added: "Interestingly, as the STAT model attributes more ozone variation to QBO and ENSO variables at higher northern latitudes as compared to PHYS model results, the PHYS results show a more persistent pattern of EESC and AERO ozone effects at high northern latitudes. The different characterization of seasonal variation in ozone in these models causes these small differences. Another difference is found in the EESC_2 results over Antarctica where a large part of ozone variations that could be interpreted as EESC driven according to the PHYS model (Figure 9) is accounted for by harmonic variables in the STAT model"

Also, in the conclusion section we added: "This first spatial regression study yields pronounced patterns in longitude/latitude dimensions of ozone-regressor dependencies. The effect of ENSO on ozone is mainly identified at the Pacific. We don't find clear indications of aerosol effects on ozone at the Antarctic. The effect of the 11-year solar cycle appears to be more important in the Southern Hemisphere, especially between -50º and 100º in longitudes, which is currently unexplained. And the effect of the southern polar vortex, clearly identified north of Antarctica, is large on total ozone columns." as second paragraph.

- Reviewer: *"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."*

The sensitivity to the age of air and PWLT turn-around points are examined to see if they fit the expected trend of large air ages and later turn-around points towards the poles. The results are somewhat surprising though, especially for the PWLT results. A statement on significant difference is very difficult, since you have to take multiple testing into account. But differences in R^2 values reaches up to 2% for the different regression fits, which is significant in our opinion. Adding this to some of the robust patterns we obtained in Figure 13 we feel that interpretation of these results is needed. We will clarify some of these issues in the revision. One possible interpretation is that the long term ozone prefers to be modelled by a smooth curve at the poles instead of an ad hoc turn around point, due to the high age of air in this region. The choice for the 3 year air age instead of the 5.5 in most of the EESC regressions may be due to the deviating proportion in ozone recovery rate versus ozone depletion rate in the EESC variables. The ozone feedback may be better explained by the 3 year version though the 5.5 year version may fit the turn-around point better. We do not conclude that these result properly show the air ages in the corresponding regions. Note that we have submitted a paper that in detail discusses the effects of different kinds of uncertainties in multi-variate regressions of total ozone, which has large consequences for both statistical significance of trends as well as the robustness of trend results derived from multi-variate analyses.

- Reviewer: "Is Table 7 the maximum ozone recovery rate or average?"

They are average recovery rates. Also, some numbers have been mixed up in table 7. This is corrected in the revised manuscript.

Reviewer: *"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."*

At the end of section 2.4 we added "The piecewise linear trend (PWLT) characterization for long term ozone variation has the advantage that the slope in ozone recovery and ozone depletion periods can be estimated separately, whereas these slopes are proportionally fixed in the EESC curves. On the other hand the EESC parameterization yields a smooth curves instead of the ad-hoc turn around point in the PWLT characterization.". Additionally, we now elaborate on these results in the discussion section.

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

On reviewers advice we have revised the results, discussion and conclusion section. The most important changes are:
- Moving "The reduced explanatory power At 55˚S is related to the vortex edge itself. Regression studies focusing on the Antarctic ozone hole typically use either a dynamical definition like the equivalent latitude to define the vortex area, or stay sufficiently far away from the vortex edge (south of 70ºS; *e.g.* Kuttipurath et al. (2013)). Hassler et al. (2011) have shown that the shape of the Antarctic vortex has changed somewhat during the last 30 years which has consequences for analyzing Antarctic ozone. However, given that this study focuses on the global patterns of ozone variability, use of a spatially variable definition of the vortex edge is not possible." from the conclusion section to the discussion section.
- Deleting "Three regions show reduced explanatory power in both models: the Antarctic vortex edge region, a tropical belt around 10ºS and a smaller band over the northern edge of Africa

extending into central Asia. The band with reduced explanatory power over the tropics and the smaller band over North Africa extending into Central Asia are due to a large component of white noise in the ozone time series.” from the conclusion section, because these findings have been sufficiently addressed in the discussion section.

- adding “As for post peak-EESC ozone trends, the results of our regressions indicate that standard methods for determining trend uncertainties likely underestimate the true uncertainties in the ozone trends that can be attributed to decreasing EESC. Hence, great care has to be taken with discussing the statistical significance of these trends.” in the conclusion section and “Based on these observations we conclude that ozone is recovering globally at a rate between 0.2 and 1.7 DU/year and between 0.9 and 3.1 DU/year for the Antarctic ozone hole period specifically. However, given the uncertainties discussed above it is not possible to determine an appropriate trend uncertainty level, hence no statistical significance of the recovery rates can be determined.” at the end of the discussion section as one of our take home messages.

- Technical corrections are all applied accordingly.