Response to reviewer #2:

We thank referee 2 for a thorough review. The manuscript is certainly improved in many respects thanks to the referee's great comments.

Only the reviewer comments are in italics.

My major critique is that the paper is entirely focused on statistics and that there is very little discussion of the physical and chemical mechanisms leading to a certain trend or variability in tropical ozone.

There is definitely an emphasis on accurately deriving the linear trend and thus on statistics. However, we disagree that the focus is "entirely on statistics" and that there is "little discussion of the mechanisms leading to variability". There are paragraphs (now sub-sections) for each source of variability (QBO, ENSO, solar, tropopause pressure). We agree that there is less discussion of the physical and chemical mechanisms leading to a trend in tropical ozone, so we have addressed this in a substantial way (see below). We now derive decadal trend profile estimates (see below) and write:

Figure 5 shows the agreement between the observed trend from the merged dataset and the calculated trend using either SAGE II or OSIRIS to determine the vertical gradient of ozone. Also illustrated is the dominant contribution to the ozone trend from the trend in tropical upwelling (the first trend term), which can be inferred by differencing the calculated trend including both trend terms and the calculated trend from only the second term of rightmost part of Eq. (9).

…so the strong impact of the QBO on trends is probably not surprising.

QBO does not affect the long-term trend and the original manuscript clearly states this (e.g. p16680, line7):

The linear trend is not sensitive to the QBO pair because of the short period of its cycles.

Further, the increase in carbon dioxide does not directly impact tropical ozone in the lower stratosphere. It is a rather indirect chain of effects where (again) the rate of tropical upwelling is important. Thus the increase in carbon dioxide is rather linear, but the effect on tropical ozone is not guaranteed to be linear. The paper should address these issues and the related mechanisms of changes in tropical lower stratospheric ozone.

We have created a sub-section 4.1 in the Discussion section, where we write the following and have added a new figure (Fig. 5):

The zonal average continuity equation for ozone mixing ratio is written as (Andrews et al., 1987):

$$\frac{\partial \bar{x}}{\partial t} = \nabla \bullet M + P - L - \bar{v} * \bar{x}_y - \bar{w} * \bar{x}_z \qquad (8)$$

where $\bar{x}$ is the zonally-averaged ozone mixing ratio, $\nabla \bullet M$ is the eddy transport, P and L are chemical production and loss, $\bar{v}$ and $\bar{w}$ are the meridional and vertical component of the residual mean circulation, and $\bar{x}_y$ and $\bar{x}_z$ denote the meridional and vertical gradients, where, for example, the latter gradient can be written explicitly as $\partial\bar{x}/\partial z$. The long-term trend in local production is expected to be positive in the tropical lower stratosphere (Lamarque and Solomon, 2010) so this term cannot explain the sign of the observed ozone trend. Trends in eddy diffusion are outside of our realm of knowledge and the latitudinal transport term is expected to be contribute weakly to the trend at the equator in the lower stratosphere because of the small meridional gradients. The final term is thought to be responsible for the ozone trend since the relative trend in $\bar{w}$ has been modelled and is consistent in sign and relative magnitude with the ozone anomaly trend (Lamarque and Solomon, 2010). The continuity equation as written in Eq. (8) is appropriate for any instant in time, but the vertical velocity and the vertical mixing ratio gradient are a function of time, so we write these factors as $\bar{w}(t)$ and $\bar{x}_z(t)$ and their trends are written as $\partial\bar{w}/\partial t$ and $\partial\bar{x}_z/\partial t$. Analogous to Randel et al. (2007), if we divide the continuity equation by the long-term zonal average mixing ratio ($\bar{x}$) to obtain an ozone fractional difference continuity equation, the vertical gradient term is given by $\partial \ln(\bar{x})/\partial z$ and is a local maximum at 18±0.5 km according to OSIRIS and SAGE II with profiles that are very similar to those obtained with SHADOZ ozonesondes data (Randel et al., 2007). Peak values are 0.68 and 0.84 ln(ppm)/km for OSIRIS and SAGE II, respectively. First, we provide support that the trend terms $\partial\bar{w}/\partial t$ and $\partial\bar{x}_z/\partial t$ are different than zero.

According to model simulations by Lamarque and Solomon (2010), the trend in the vertical velocity ($\partial\bar{w}/\partial t$) at 85 hPa over the period 1970-2005 is ~0.23±0.02 km/year/decade (or ~4%/decade) considering all forcings (including N_2O, etc.), whereas the trend due to CO_2 and sea surface temperature increases is 0.17 km/year/decade, and the trend due to halocarbons is only 0.05 km/year/decade. Thus the linear trend in tropical upwelling in the tropical lower stratosphere is primarily due to CO_2 (as sea surface temperature rise is strongly driven by increases in atmospheric CO_2 but with a lag due to the thermal inertia of the oceans) (e.g. Bryan et al., 1982). The tropical upwelling continues to increase with height throughout the lower stratosphere (Lamarque and Solomon, 2010) but the temporal trend in tropical upwelling ($\partial\bar{w}/\partial t$) is largest (in units of km/year/decade) at 17.5 km based on multiplying the relative trend profile by the vertical velocity profile (both panels of Figure 1 of Lamarque and Solomon, 2010).

The trend in the vertical gradient of ozone mixing ratio ($\partial\bar{x}_z/\partial t$) in the tropical lower stratosphere is largely determined by the trend in tropopause pressure. The sensitivity of ozone mixing ratio to tropopause pressure ($\partial\bar{x}/\partial p_{trop}$) at various altitudes can be determined by a simple linear regression. SAGE II and OSIRIS ozone mixing ratios correlate very well (r^2>0.5) with tropopause pressure in the 16.5-19.5 km range but not well outside this narrow range. An overall range of 14.5 to 25.5 km was studied. At 17.5 km, r^2 values are 0.8058 and 0.6921 for the fit of SAGE II and OSIRIS mixing ratios, respectively, to tropopause pressure plus a constant. Then, we use the following equation:

$$\frac{\partial\bar{x}_z}{\partial t} = \frac{d\left(\frac{\partial\bar{x}}{\partial p_{trop}}\frac{\partial p_{trop}}{\partial t}\right)}{\partial z} \tag{9}$$

and take the value of the second factor in the numerator on the right hand side of Eq. (9) from the caption of Fig. 2. The first factor is obtained from the simple linear regression and then the product is numerically differentiated with respect to height. We find that, at the tropopause, a -3.5% and -4.4% decadal trend in ozone is expected from the linear trend in tropopause pressure using SAGE II and OSIRIS, respectively, to determine vertical gradient in ozone.
The relative ozone trend (%/decade) can be determined by differentiating the final term on the right side of Eq. (8):

$$\frac{\partial \overline{x}}{\overline{x}\partial t} = \frac{(\overline{w} + \frac{\partial \overline{w}}{\partial t}\partial t)(\overline{x}_z + \frac{\partial \overline{x}_z}{\partial t}\partial t) - \overline{w}\overline{x}_z}{\overline{w}\overline{x}_z \partial t} = \frac{(\overline{x}_z \frac{\partial \overline{w}}{\partial t} + \overline{w}\frac{\partial \overline{x}_z}{\partial t})}{\overline{w}\overline{x}_z} \tag{10}$$

where $\partial \overline{x}_z / \partial t$ is replaced by Eq. (9) and a minor term is neglected which involves the product of both time-derivatives in Eq. (10). In summary, the $\partial \overline{x}_z / \partial t$ peaks at 18±0.5 km in both satellite datasets and $\partial \overline{w} / \partial t$ peaks in the troposphere, whereas $\overline{w}$ and $\overline{x}_z$ peak above 25 km. Also, note that trends due to the other terms in Eq. (8) have not been included.
Using either satellite dataset, the first trend component ($\overline{x}_z \partial \overline{w} / \partial t$) in Eq. (10) gradually increases with increasing height in the tropical LS. The second theoretical trend component ($\overline{w} \partial \overline{x}_z / \partial t$) also shows a peak near the tropopause (17±0.5 km in both datasets). After summing these two theoretical trend terms, the trend in ozone mixing ratio peaks at 18±0.5 km (using both datasets). Converting to a relative trend (i.e. an anomaly) then amplifies the peak and shifts it downward to 16.5 km (using either dataset). Note also that the relative vertical velocity trend profile $\partial \ln(\overline{w}) / \partial t$ peaks at 17.5 km, whereas the trend in the vertical gradient of fractional ozone $\partial \ln(\overline{x}) / \partial z \partial t$ increases down to the lowest computed-trend altitude (15.5 km). The computed magnitude of the negative trend in ozone reaches a maximum at 16.5 km of 10.25±0.05% where the uncertainty is the difference between trends using SAGE II and OSIRIS vertical gradients. Figure 5 shows the agreement between the observed trend from the merged dataset and the calculated trend using either SAGE II or OSIRIS to determine the vertical gradient of ozone. Also illustrated is the dominant contribution to the ozone trend from the trend in tropical upwelling (the first trend term), which can be inferred by differencing the calculated trend including both trend terms and the calculated trend from only the second term of rightmost part of Eq. (10). Note that the tropical upwelling and its temporal trend were obtained from Lamarque and Solomon (2010) and are appropriate for a wider latitude band (20N°-20°S) and a different time period (1975-2005) and that vertical gradients from OSIRIS and SAGE II are naturally for shorter time periods than the observed merged trend.

Eq. (10) indicates that the relative trend in ozone is equal to the relative trend in tropical upwelling if vertical gradient trends are neglected. Comparing the ozone trend in Lamarque and Solomon (2010) (their Figure 2, right panel) for the ‘All forcings’ case and their vertical velocity trend (their Figure 1, right panel), one can see that the magnitude of the tropical upwelling trend is essentially equal to the magnitude of the ozone trend except at the tropopause, where the ozone trend goes to 0 but the vertical velocity trend is 2%/decade.

One weakness of our simple trend model is that, as well as being a function of latitude, altitude, and time, $\overline{w}$ and $\bar{x}_z$ may be functions of each other, which would complicate the calculus of Eq. (10). In statistical terms, this interdependence is measured by correlation. In atmospheric physics terms, this could be considered coupling or a feedback which could be positive or negative and varying with altitude. The correlation between $\overline{w}$ and $\bar{x}_z$ is weak (r = -0.1 on a 1 km grid between 15.5 and 24.5 km with the aforementioned data sources). However, the correlation could be stronger at the tropopause, where trends in the vertical gradient of ozone can lead to trends in the vertical gradient of temperature since ozone is involved in atmospheric heating and affects the temperature profile,. In turn, this could affect the trend in tropical upwelling. Conversely, the vertical gradient of ozone could also be a function of the tropical upwelling. In any case, Figure 5 illustrates good agreement in the ozone trend profile determined with this simple model with no feedbacks and the merged observational dataset.

What is not stated but we hope is obvious is that the long-term rising of the tropopause decreases ozone observed at a given height because the vertical gradient of ozone is large and positive near the tropical tropopause.

Moreover, EESC plays a substantial role as an explanatory variable in the paper. I question whether EESC should be used as an explanatory variable at all in the analysis. As stated in the paper, there is only a slight (bit how 'slight'?) impact on tropical lower stratospheric ozone – it is argued through the change in column ozone. There is clearly no effect through local chemistry as reactive chlorine is essentially absent in the lower tropical stratosphere. So if there is no mechanism apparent, how EESC might impact tropical lower stratospheric ozone why is it included in the analysis? Arguably, the temporal development of N_2O and CH_4 (which are both greenhouse gases like the CFCs, but also impact ozone loss cycles and thus the ozone colum) should be more important variables than EESC.

EESC does not play a substantial role as an explanatory variable in the paper. It was used in the final analysis only at 17.5 and 18.5 km. The lowest altitude (17.5 km) was omitted because of difficulty merging the datasets. At 18.5 km, EESC was not used in the final regression model as shown in Figure 3 of the revised manuscript. Somehow, this was not stated clearly in the text, so we now write in the 'Conclusions':

The linear trend is a statistically significant basis function at all altitudes in the 18.5-24.5 km range, and statistically insignificant at 25.5 km, while EESC is not significant at any of these altitudes (see Fig. 3).

The impact is so slight that EESC is rejected from our final regression model at 18.5 km. The mechanism for EESC is the one echoed by the reviewer: "through the change in column ozone". This mechanism was deemed to be sufficient for inclusion in the analysis. It is a statistically significant term at 17.5 km, but this altitude was omitted in terms of the merged trend profile for reasons unrelated to EESC. The other greenhouse gases could be important predictor variables, although we did not consider them. In general, we avoided considering all constituents that have a long-term trend of their own and short-term variability because the inferred trend in ozone can be affected by their inclusion. This is what was learned from this study with regard to aerosol extinction (which is related to aerosol number density). The predictor variables include the linear trend, a constant, EESC (which we carefully tested in terms of its impact on the O_3 trend) and the remaining basis functions are essentially oscillations or cyclic in nature (QBO, ENSO, solar, detrended and deseasonalized tropopause pressure, and QBO harmonics).

Finally, as already stated in my ACPD review, the quality of the figures needs to be improved substantially. All figures have very thin axis lines, Figs. 2 and 3 also very thin lines in the figure. Figs. 2-4 have labelling in much too small letters.

The original figures 1-4 were redone following the reviewer's suggestions.

page 16662, l. 2-4: Put the first sentence at the end of the abstract.

The first sentence was placed after the sentence: "Analysis of the merged dataset (1984-2012) shows a statistically significant negative trend at all altitudes in the 18-25 km range including a trend of (-4.6±2.6)% /decade at 19.5 km where the relative standard error is a minimum."

page 16662, l. 9: replace reaching by maximising

This sentence has already been reworded following the review of referee #1, so the suggested rewording is no longer relevant.

page 16662, l. 13: Citation for older trend studies?

In the same paragraph, there are references to 'older' trend studies.

page 16662, l. 15: 'primarily'? any other reason?

We have rewritten as: "…as a result of…". Chlorine is also present in e.g. carbon tetrachloride which is not a CFC. Also, chlorine could be transported from the troposphere at an increasing rate and involve sources that are not anthropogenic.

page 16662, l. 16: The first WMO Report on ozone was actually in 1981 (WMO, 1982)

We have skimmed this report as well but it was not focussed only on ozone.

page 16663, l. 28: the paper by Daniel is on GWPs, a better citation for EESC in the framework of ozone is e.g. Newman et al. (2007)

We are aware of the paper and have switched to this reference.

page 16664, l. 1: drop 'fewest maxima', it is well known that EESC has only one peak

We now write:

…however Equivalent Effective Stratospheric Chlorine (EESC) (Newman et al., 2007) has had only one maximum and…

page 16664, l. 22: state what the essence of the Wang filtering is

We now write in Sect. 2.1.1:

Data filtering according to the Wang et al. (2002) recommendation is applied to the entire time series and is effective at removing ozone measurements with large uncertainties or with significant aerosol-extinction contamination, such as in the post-Pinatubo period.

page 16665, l. 13: Do you want to be more specific about which Envisat instruments? Only MIPAS?

We didn't want to be more specific about the atmospheric chemistry instruments on Envisat. They are none other than GOMOS, SCIAMACHY, and MIPAS.

page 16666, l. 8: somewhat unclear what is meant by GPS timing issue.

We now write, noting that this was the first leap second adjustment of the mission and that subsequent adjustments have been implemented correctly:

The upcoming leap second adjustment between the end of 2005 and the start of 2006 was implemented incorrectly in August 2005, leading to tangent height errors that persisted for three weeks. Therefore, we screen …

page 16666, l. 11: Here and elsewhere: make sure that variables in the text (here zmm) are in italics

This was done, although keep in mind that "EESC" is a concept and "*EESC*" is the variable name.

page 16667, l. 11: You could consider a latitude band which is varying with season to optimise the selection of tropical data.

This is an interesting idea which we never considered, but would require investigation and could only be considered in future work (beyond this manuscript). Although, not mentioned in the paper, we preferred a narrow band at the equator because it is tropical all year round (e.g. within <30° of the sub-solar latitude). Some environmental variables have a lag (e.g. warming of large bodies of water), so the appropriate seasonal variation of the latitude band would be tricky, particularly during solstice. The width of the latitude band is a second issue: while the sub-solar point may be at 23.45°N at boreal summer solstice, if the latitude band width is 18° (i.e. the largest we tested), this would extend out to 32.45°N, which is clearly extratropical.

page 16669, l. 9: The increase in carbon dioxide does not directly impact tropical ozone in the lower stratosphere. It is a rather indirect chain of effects where the rate of tropical upwelling is important. I suggest discussing the physics here in more detail.

We will defer the discussion of the indirect response of O_3 to CO_2 increases to the Discussion section. So we only write:

The indirect relationship between linear trends in CO_2 and tropical lower stratospheric ozone will be elucidated below (Sect. 4.1).

page 16669, l. 25: more explanation is needed here about the concept of orthogonal QBO time series. Here just two names, QBOa and QBOb are stated.

We now write in this paragraph:

Because the QBO signal has an altitude-dependent lag and the number of available QBO pressures is insufficient for instruments with high vertical resolution and sampling (such as OSIRIS and SAGE II), one of two solutions is generally used, either of which uses two fitted quantities. Either a single QBO proxy is fitted along with a fitted lag to make the phase appropriate for the ozone response at the local altitude, or two QBO basis functions are fitted that tend to naturally account for the difference between the local phase and the phases at the pressures of the two QBO time series. In the latter approach, the two basis functions tend to be orthogonal or tend to envelope the local pressure. In this work, the use of two QBO basis functions is preferred over the approach of using a lag, particularly because of the strong altitude dependence of the QBO signature (in addition to the altitude-dependent lag) in the lower stratosphere (discussed in Sect. 3.3).

page 16670, l. 14: What about QBOb? Why is it not mentioned, why is its behaviour different?

QBOb is not mentioned because it is not correlated with tropopause pressure (r=-0.07). Its correlation to tropopause pressure is expected to be less than that of *QBOa*, because these two QBO terms are (nearly) orthogonal.

page 16670, Eq. 2: State which the underlying assumptions are for EESC here, Which age of air, which width of the age distribution etc. (Newman et al., 2007).

The EESC curve used is not from Newman et al. (2007). It is appropriate for the mid-latitude lower stratosphere. It is intended to be appropriate for an age of air that is greater than the local age of air in the tropical lower stratosphere. This point is discussed in the following sentence of the original manuscript:

EESC is not fit with any age of air correction since it is possible that, given the model results by Lamarque and Solomon (2010) and regression fits of observed ozone by Bodeker et al. (2013), that EESC actually has a slightly positive overall response in the tropical lower stratosphere by destroying ozone in the upper stratosphere which stimulates production below and thus the age of air in the upper stratosphere would be more relevant.

The most appropriate age of air and width of the age distribution was not known without detailed ozone photochemistry modelling but could be considered in the future. Given that EESC is not included in the final regression model at any altitude in the reported trend profile, we simply state the curve was for mid-latitude lower stratosphere (which could be used to translate to an age of air and a width of the age distribution):

It is peaks in early 1999 (appropriate for the mid-latitude lower stratosphere) and is expected to be valid until ~2015.

page 16670, l. 24: How 'slight'? I question whether EESC should be used as an explanatory variable at all in the analysis.

Lamarque and Solomon (2010) show a statistically insignificant increase in ozone of up to 1-2%/decade for the 1975-2005 period in the tropical lower stratosphere due to CFC increases (their Figure 6, bottom), which grows in magnitude with decreasing altitude into the tropical tropopause region, where it becomes statistically significant. While this 1-2%/decade increase was statistically insignificant in their analysis of model data, we found that, in our merged observational dataset at 17.5 km, the ozone response to EESC was statistically significant, but statistically insignificant at 18.5 km. See discussion above in this reply. With future improvements to the OSIRIS and SAGE II datasets, it may be possible to obtain a reliable trend over the merged time period at 17.5 km, and we recommend that EESC should be again considered/tested as a predictor variable in the tropical lower stratosphere.

page 16671, Eq. 3: Have the authors considered to do the analysis on theta levels as well? This would remove a large fraction of the ozone seasonal variation in the polar lower stratosphere (Randel et al., 2007; Konopka et al., 2010)

We considered doing the analysis on pressure levels but neither instrument measures temperature or pressure and thus we hesitated to convert altitudes to pressures using auxiliary information. Similarly, we would also hesitate to convert to equivalent temperatures as well. Fortunately, we are not focussed on the polar lower stratosphere. Some of the variation in ozone resulting from the semi-annual variation in tropopause pressure should be captured by having dp_{trop} as a basis function.

Personally I find the term 'annual harmonic of linear terms' (and similar wording) confusing. Why not call it seasonal variation of the linear trend (as sometimes done later in the paper)

We have removed this wording, which we originally thought was instructional and less confusing than "seasonal variation of the linear trend", in three instances. We now write (p16673, line15):

"Also not considered further for regression modelling with the merged data record is the seasonal trend."

and at p16674, line4:

"Thus we also consider the SAO."

and at p16674, line21:

"Semi-annual harmonics of solar and ENSO terms, plus the semi-annual variation of the linear trend were skipped since their annual counterparts were excluded."

But in one instances, we prefer to keep this wording because the alternative, using the language suggested by the reviewer, would entail specifying cosine and sine terms of the seasonal trend and the seasonal cycle and would be a wordier solution. Reviewer 1 has suggested that the wordiness be reduced. Thus we retain the following sentence (p16685, line1):

Using SAGE II data only, we found that the harmonic of the linear trend (seasonal trend) and the same harmonic of a constant (seasonal cycle) are never statistically significant predictors with the same sign at any altitude (even when using a best regression model specifically for SAGE II at 18.5 km).

page 16672, l. 27: the discussion on heterogeneous chemistry here is rather speculative. I suggest either to be more explicit (which het. reactions?) or dropping this part.

We drop this part of the sentence and now leave only:

"…considering the role of aerosols in determining photolytic fluxes."

page 16673, l. 14: drop; as stated above it is well known that EESC has only one peak and no short term variability (e.g., Newman et al., 2007)

We have dropped this sentence.

page 16673, l. 16: as stated above, I find the term 'annual harmonic of linear terms

As mentioned above, this has been changed to the reviewer's suggestion.

page 16675, l. 11: Do not abbreviate confidence interval

The reviewer has pointed out that the acronym is used without it being defined. Since we use CI later in the paper, we now write:

…95% confidence interval (CI).

page 16679, l. 21: Discuss what the reasons could be for the discrepancy with the results of Forster et al. (2007).

We no longer have a discrepancy with Forster when the autocorrelation of the residuals is included in our trend uncertainty. There really was no significant discrepancy with Forster et al. because the value of ~6.5% was simply read off their Figure 1a and the trend could be as small as -6% given the 1% increments in their colour scheme. Thus, we now write:

At 19.5 km, Forster et al. (2007) and Randel and Wu (2007) show decadal trends of ~-6.5% and ~-3.8%, and we find (-3.9 ± 4.0)%/decade, in closer agreement with Randel and Wu (2007).

page 16680, l. 15: But the effect of the greenhouse gases on ozone is via tropical upwelling. Thus, for this argument to hold one need to show that the trend in tropical upwelling depends also linearly on greenhouse gases, which is not quite as clear. There is some support from models perhaps (Butchart et al., 2010).

Thank you for the valuable reference. We defer this discussion to the Discussion section, so we now write here:

The general agreement is expected if the linear trend in ozone has not changed in the last three decades.

We have included the Butchart et al. reference in the Discussion section. We also point out that the trend in tropopause pressure needs to depend linearly (or not at all) on greenhouse gases:

It is clear that both CO_2 has been increasing linearly and ozone in the tropical LS has been decreasing linearly for decades, but in order to demonstrate that the positive linear trend in CO_2 is driving the negative linear trend in ozone in the tropical LS, we establish here that linear increases in CO_2 appear to lead to linear trends in both tropical upwelling (Butchart et al., 2010) and tropopause pressure (Lamarque and Solomon, 2010). Recall that the tropopause pressure trend directly relates to the trend in the vertical gradient of ozone, which is the second physical mechanism responsible for ozone trends based on Eq. (10). Butchart et al. (2010) show that the tropical upwelling trend due to increases in greenhouse gases is expected to be linear over 140 years (1960-2100). Regarding the tropopause pressure trend, the linearity can be inferred from the small uncertainty of the linear trend coefficient when using a simple linear regression of time to the tropopause pressure (Lamarque and Solomon, 2010). The linear trend on tropopause pressure is statistically significant within 10° of the equator (Lamarque and Solomon, 2010). If the tropopause pressure was changing in a non-linear way to increases in CO_2 (and SST), the trend would not be significant using a simple linear regression. Also, the NCEP tropopause pressure linear trend is also statistically significant within 10° of the equator and agrees very well with the linear trend in tropopause pressure from a model simulation where only CO_2 and SST increases are considered and a second model simulation where all forcings are considered. For the latter simulation (20°N-20°S), the linear trend uncertainty in tropopause pressure is <20% and is expected to be smaller for a narrower latitude band centered on the equator.

page 16681, l. 19-23: is there a physical explanation for these findings? At least are they consistent with the expected mechanisms how, e.g., QBO and ENSO impact tropical ozone in the stratosphere?

There is a physical explanation for these findings. The QBO is a lower to middle stratospheric phenomenon. ENSO, tropopause pressure, and linear variations (in tropical upwelling and ozone vertical gradient) are arising from the troposphere, and thus they should decay with height above the tropopause. We now write immediately below these statements on the observed altitude dependence of the ozone response:

This is expected since the QBO is a lower to middle stratospheric phenomenon with strong zonal wind velocities at 10 hPa, whereas ENSO, in essence, is a disturbance to the Walker circulation in the troposphere (and related ocean temperature and dynamical changes). The expected altitude dependence of the response of ozone to linear and tropopause terms is discussed in Sect 4.1.

page 16683, l. 9, 10: Are these statements about the spectrum of the age of are consistent with observations (e.g. Stiller et al., 2008) ?

These statements about the spectrum of the age of air are consistent with observations. The Strahan et el. (2009) reference included in the paper includes observational information on the modal age of air, which agrees with our findings. The Stiller et al. (2008) paper only discusses mean age of air whereas our observed ENSO signal is expected to be a modal age of air. However, we compared the consistency of the (observed and modelled) mean age of air in Strahan et al. (2009) to those in Stiller et al. (2009) at 20 and 25 km and the agreement is excellent, particularly versus the mean age of air from balloon SF_6 observations, suggesting that there is consistency between the three studies.

page 16685, l. 16: I find the statement about the 'central tendency' which 'can be measured using the mode' (and similar below) confusing. I believe what you mean is the mean age-of-air.

The mean is another measure of central tendency (as is the median, although the median is not used as far as we know in age of air estimation).

page 16686, l. 10: I am confused here: is the –3% trend a result of this paper or not? If it is not the provide a citation, if it is there seems to be a contradiction with the sentence following.

This sentence has been changed and Figure 6 has been created. In short, the trend profiles in Figure 6 are a result of this paper.

page 16686, l. 14-15: As stated in my initial review, I consider this statement very speculative. Which mechanisms are alluded to here, which chemistry on the clouds, which trends of cloud surface area? Either this sentence needs to be dropped of there needs to be a extensive discussion of the mechanisms in the paper.

This sentence was dropped.