Response to reviewer #3

We thank referee 3 for a thorough review. The manuscript is certainly improved in many respects thanks to the referee's comments.

Reviewer comments are italicized:

1. The style of the article is quite dense, without clear-cut messages. The writing should be revised in order to provide a clearer explanation of the main results.

Based on comments by the other reviewers, the revised draft will contain some clear-cut messages. In particular, a new sub-section is introduced to describe the mechanisms by which CO_2 increases leads to decreases in tropical lower stratospheric ozone and the observed trend is compared with calculated trends. Also, sub-sections were created in the Results section, each dedicated to a predictor variable: linear, QBO, etc.

Here are some other clear-cut messages that are present in the original manuscript, along with any relevant revisions to these statements:

1) Merging with OSIRIS allows trends to be significant to a lower altitude (18.5 km) versus SAGE II alone (21.5 km). This is found on p.16680, lines 12-13:

The merging allows for the detection of a statistically significant trend at 18.5-24.5 km, not found with SAGE II alone (except at 21.5-22.5 km).

A related message was added to the current abstract:

Uncertainties are smaller on the merged trend than the SAGE II trend at all altitudes.

2) Trends from SAGE II alone are not significant below 19 km. With the effect of auto-correlation of the residuals included in the linear trend uncertainty calculation, SAGE II trends are now statistically insignificant below 21 km. See excerpt above.

3) Difficulty merging at 17.5 km because of sparse SAGE II during the overlap period and because of seasonally cycle differences between SAGE II and OSIRIS. This was conveyed on p.16679, lines 1-3:

This discrepancy in trends at 17.5 km likely results from the small sample size of available months of overlap (N=12), relative to N=24 at most altitudes.

To which, we now follow with:

This is mostly due to the sparseness of the SAGE II dataset after filtering for aerosol contamination.

And secondly, the original manuscript mentioned (p. 16679, lines 8-10):

This test points to an artificial step between OSIRIS and SAGE II time series, likely due to an imprecise anomaly bias correction, which likely stems partly from the seasonal biases in OSIRIS.

Messages cannot always be clear-cut and there is some element of speculation because there is only one SAGE II dataset and one OSIRIS dataset with which to work. For example, we did/could not try to adjust the seasonal cycle of the OSIRIS ozone data so it is difficult to determine what the impact of the seasonal cycle differences is on the results and conclusions when there is no 'alternate world' where these differences are smaller. The revised manuscript also notes that the difficulty in merging manifests itself at 18.5 km with autocorrelation higher than in individual satellite datasets:

…we find that there is no obvious altitude dependence to the autocorrelation, except for higher autocorrelation at 18.5 km (ϕ=0.777), whereas the other altitudes are in the ϕ=0.56±0.05 range for AR1. (…) If the inter-sensor bias between SAGE II and OSIRIS at 18.5 km is not equal to the bias during the overlap period (e.g. due to instrument degradation), a high autocorrelation will result in the merged dataset. At 18.5 km, the merged dataset has a much higher AR1 auto-correlation than for the individual SAGE II and OSIRIS datasets (0.26 and 0.65, respectively). At no other altitude is the auto-correlation in the merged dataset so much larger than in the individual datasets.

4) The altitude dependence of the QBO needs to be considered to improve trend detection capability. This strong and clear-cut message is found in the original manuscript (p16682, L15):

In fact, the correlation between 10 and 30 hPa, and between 30 and 70 hPa is weaker than the correlation between the two orthogonalized QBO basis functions. (…) This method of accounting for the altitude dependence of the QBO in vertically-resolved ozone time series analysis allows for r^2=0.74 using only SAGE II data at 18.5 km in contrast to ≤0.4 found by Randel and Wu (2007) (see also Table 1 for altitude-dependence of r^2 using the merged dataset) and improves the linear trend uncertainty.

We have added to the words "altitude-dependent" to the following sentence in the current abstract:

Underlying strong fluctuations in ozone anomaly due to El Nino Southern Oscillation, the altitude-dependent Quasi-Biennial Oscillation, and tropopause height need to be taken into account to properly determine the trend.

5) Ozone anomaly biases between OSIRIS and SAGE II in the 2001-2005 overlap period are small. This clear-cut, important message is in the opening sentence of the Conclusions:

We have shown that anomaly biases between OSIRIS and SAGE II in the overlap period (2001-2005) are small (<2%) when each dataset is deseasonalized separately.

6) All signs point to a linear trend continuing over the past 3 decades at the equator in the lower stratosphere. This clear cut message is delivered at various points in the paper, for example, in the Conclusions (p16686, line4):

Given that the linear trend is a statistically significant basis function at all altitudes, while EESC tends to not be, our results are consistent with model results that show that the driving forces behind the decadal changes in ozone in the tropical lower stratosphere are increases in greenhouse gases and sea surface temperature (Lamarque and Solomon, 2010).

And in the abstract, we have the following clear-cut message, similar to the one in the original manuscript:

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.

2. Introduction (page 16663 l16-20) : More discussion and reference to recent trend studies on tropical ozone based e.g. on SAGE II and SHADOZ data (Randel and Thompson, 2011) should be provided.

Reviewer 1 made a similar suggestion, and we now write there:

Trends in the tropical lower stratosphere are of interest given modelled changes in the Brewer-Dobson circulation (e.g. Bunzel and Schmidt, 2013), and with the significant negative trend in this region observed over the last quarter century using a combination of SAGE II and ozonesonde data (Randel and Thompson, 2011).

3. New version of SAGE II data (section 2.1.1) : the authors use version 7 of SAGE II. Some detail is given on the data filtering in order to avoid contamination by volcanic aerosol due to the Pinatubo Eruption and more generally improve data quality, but no indication is given on the improvement gained with respect to the earlier data version, especially in the tropics. Also, what is the effect of clouds on satellite ozone data retrieval at the tropical tropopause and above?

The data quality improvement has been determined specifically for the tropics since this is the region studied in this paper. The following statement appears in the original manuscript (p16665, line2):

(…) the improved quality of the version 7.0 SAGE II data (Damadeo et al., this issue) was immediately obvious upon switching to the latter as uncertainties were reduced in linear trends at all studied altitudes.

There are very few clouds above the tropical tropopause based on our experience with several satellite-borne instruments including the Imager onboard the Atmospheric Chemistry Experiment. We write in section 2.1.1:

Clouds at the tropical tropopause will be filtered effectively from the SAGE II and SAGE II is not sensitive to clouds below the field of view (FOV) because it uses the solar occultation technique.

However, for OSIRIS, which uses limb scattering, we write in Sect. 2.1.2:

Clouds below the FOV can affect the retrieved ozone at and above the tropical tropopause (Degenstein et al., 2009). Clouds in the FOV are a worst case scenario, but in the vertical direction, the FOV is only 1 km at the tangent point (Llewellyn et al., 2004) so the tropopause would need to be located at 17.5 km for tropopause clouds to contaminate ozone data in the 18.0-19.0 km range (which is reported at 18.5 km). However, there are several reasons why clouds should be a minor source of error on the retrieved ozone trend:

1) Clouds would need to have a trend of their own to affect ozone trends. Otherwise, the bias correction between SAGE II and OSIRIS should largely remove any bias.

2) Ozone data is rejected when bright clouds are detected during the scan (Adams et al., 2013).

3) The solar zenith angles (SZAs) are very large with OSIRIS, particularly in the tropics, so the upwelling radiation off of cloud decks is reduced (Haley et al., 2004).

4) Effective albedo is simultaneously determined during the ozone retrieval and could compensate for the lack of clouds in the forward modelling, particularly for low clouds (Degenstein et al., 2009).

5) The sensitivity of clouds, particularly those at low altitude, is reduced by normalization with a high-altitude reference spectrum (von Savigny et al., 2002). Errors at 20 km are typically <1% when effective albedo is simultaneously retrieved (Flittner et al., 2000).

6) Based on the above arguments, clouds in the FOV are the most likely to have a strong effect, however at the tropical tropopause, they tend to be optically thin.

Comparison between SAGE II and OSIRIS data (p16668, l1-4): The difference between anomalies seem to be small (figure 1) but yet the difference between monthly climatologies needs to be taken into account in the construction of the merged time series. Can the authors better justify the scaling of the monthly climatologies? The use of standard error instead of standard deviation would provide a better estimation of the significance of the bias between both measurement types in the overlap period in figure 1.

The magnitude of the difference between the ozone anomalies is 0.6% at 20.5 km, which is the maximum in the 18.5-25.5 km range and should be subtracted to avoid any bias since the trends are also very small and very sensitive to time series discontinuities introduced by merging data from two different sensors. The scaling of the SAGE II and OSIRIS climatologies is necessary because we are trying to compare anomalies from two instruments during the overlap period. If there is a decreasing trend, the SAGE climatological ozone (i.e. long-term average) at a given altitude will be higher than the OSIRIS climatological ozone. If the decreasing trend was ignored when calculating the ozone anomaly for each instrument, the SAGE II ozone anomaly would tend to be negative during the overlap period because the overlap period occurred at the end of the mission, whereas the OSIRIS ozone anomalies would tend to be positive in the overlap period because this corresponds to the start of its mission. We now write:

However, since the climatologies for the two instruments cover different periods and a temporal trend may exist, we scale the climatology to make it appropriate for the overlap period by multiplying by the ratio in the numerator. This step is necessary to correctly determine any bias between the ozone anomalies of the two instruments in the overlap period (see below).

5. Construction of the merged time series (section 2.1.3): The construction of the merged time series needs to be clarified. In particular, there seems to be a typo in the following sentence: The denominator in Eq. (1) represents the inter-sensor mean ozone over the anomaly period. How are merged the SAGE II and OSIRIS monthly mean data in the overlap period: are they averaged? A figure could be included representing the merged time series as a function of time and altitude.

Hopefully, the construction of the merged time series was clarified in our response to the previous comment. There is not a typo in the quoted sentence. The monthly ozone anomalies for each instrument are averaged during the overlap period. This is described in the original manuscript (p16667, line27):

During the overlap period (2001–2005), the number of measurements from the two sensors is not summed. If only one sensor has ≥10 measurements in a given month and altitude, then only data from that sensor is retained in the merged data record. If both sensors have ≥10 measurements in a given month and altitude, then the inter-sensor monthly mean is used. During the overlap period, for months where both instruments have sufficient data, biases between SAGE II and OSIRIS ozone anomalies are small (< 1 %) but show an altitude dependence (Fig. 1). Thus, at each altitude, there must also be more than two months during the overlap period for which both sensors measured ozone in order for the inter-sensor anomaly bias to be adequately corrected. This bias (averaged over the overlap period) is used to adjust the entire OSIRIS anomaly time series.

6. Construction of the regression model (section 2.2):
a. My main concern here is that the reasons for model optimization are purely statistical (reduce linear trend uncertainty) without sufficient consideration of physical mechanisms behind the effect. For example, the ENSO lag seems quite noisy as a function of altitude (table 2).

Based on this comment, we present some evidence from the literature of the response of ozone (or other trace gases) to the various predictor variables in this section. The variables which are optimized are the ENSO lag, and the QBO pair. More generally, the regression model is optimized at each altitude with respect to which predictor variables are included. The reviewer may also be questioning which harmonics are chosen. Regarding the ENSO lag, it becomes noisy at higher altitudes, where the ENSO signal is weaker and the age of air spectrum is broader. At the lowest five altitudes (17.5-21.5 km), the signal is relatively robust and can reduce the linear trend uncertainty even relative to a 0.5 month lag change. If the latitude band is reduced to 5°, the noise is reduced to ~0.5 months up to 25.5 km. To introduce the role of ENSO in lower stratospheric ozone, we now insert (at p16669, line21):

ENSO affects tropical upwelling which, in turn, leads to fluctuations in temperature and ozone in the tropical lower stratosphere (Randel et al., 2009 and references therein).

To provide some physical background for the use of an altitude-dependent ENSO lag, we insert (at p16669, line 22):

The ENSO signal in the upper troposphere lags the one at the surface, and the stratospheric signal lags further behind according to the age of air.

We moved the following sentences from the Results section to Sect. 2.2 to provide some justification for the use of tropopause pressure as a predictor variable.

Total ozone is well known to be correlated with tropopause pressure, even over large spatial scales, particularly near 30°S in austral summer, whereas at the equator, the correlation is much weaker (Schubert and Munteanu, 1988). Stratospheric ozone mixing ratio also has been shown to correlate with tropopause height at southern mid-latitudes (Bodeker et al., 1998).

The physical basis for inclusion of EESC as a predictor variable is already discussed in the original Sect. 2.2:

…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 physical basis for the use of a constant in the regression is already discussed in the original Sect. 2.2:

…but includes a constant (c) since our merged ozone anomaly does not average over time to nil.

The inclusion of a solar proxy was originally justified simply because it was conventional to do so; however we now add the following to provide physical mechanisms to support its inclusion:

Hood et al. (2010) review the various physical mechanisms that could lead to a lower stratospheric ozone response to solar cycle variations.

The use of harmonics for the QBO and how many harmonic orders to include should be justified. We did so in the original manuscript regarding QBO harmonics (p16674, line 3):

We note that Ray et al. (1994) also found interannual temperature variability in the semi-annual cycle and partly attributed it to the QBO, albeit on a very short data record. Wallace et al. (1993) found the semi-annual cycle in the QBO has comparable statistical significance to the annual cycle, although they included pressures as low as 10 mb.

Finally, the QBO pair is optimized based on statistics, but in the Results section, we discuss the physical plausibility of the resulting pair as a function of altitude:

In the 18-26 km range, QBO is the key predictor of ozone variability (see Fig. 4d,f). The best single QBO pressure at an altitude tends to correspond approximately to the pressure at that altitude (see Table 1). For example, at 17.5 km (~85 hPa), the single best pressure is naturally 70 hPa, with 30 hPa being a nearly orthogonal complement. A QBO time series at 90 hPa might be useful but is not available except for the radiosonde station at Singapore. The complementary QBO term between 21.5 and 23.5 km tends to also be at a lower pressure which is orthogonal to the QBO time series at the local pressure. Above 22 km, there is also a tendency for the single best QBO pressure to be slightly lower than the local pressure (i.e. higher altitude). These tendencies toward lower pressures likely arise from the shape of the age of air spectrum being more skewed to older air with increasing altitude (National Aeronautics and Space Administration, 1999). The 'orthogonal' complementary QBO pressures tend to have a lag of ¼ of the QBO period relative to the single best QBO pressure and thus provide maximum independent information and also account for any lag in the ozone response to the local QBO signal (Witte et al., 2008). These pairs of QBO basis functions act similarly to the orthogonalized QBO basis functions of Randel and Wu (2007).

We have also added the following sentence (p16671, line 8):

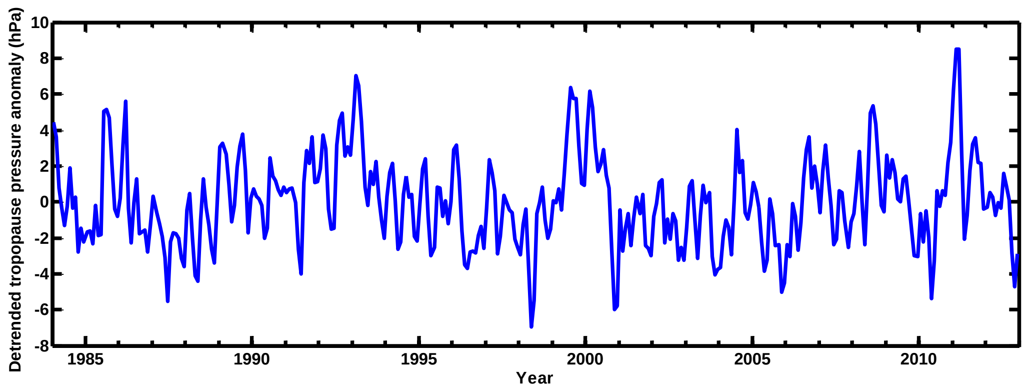
This maximizes the independent information contained in the two QBO basis functions.

We also clarified the modelling of QBO signal in terms of the physical need for two QBO fitting parameters, writing at p16669, line26:

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).

b. What is the reason for representing the deseasonalized tropopause pressure in figure 2? The figure is of poor quality, as are the other figures of the article (the lines are generally not visible). If a predictor variable is to be presented, the other predictor variables (ENSO, Solar flux: : :) should also be shown.

The reason is that detrending and deseasonalization of the tropopause pressure is performed, whereas for the other basis functions (ENSO, solar, QBO, constant, linear), no processing is required. We have improved the quality of the figure (see below). We don't feel the other variables should be shown for this reason. We feel that there is value in illustrating that the deseasonalization and detrending was successful. The semi-annual variation of tropopause pressure is also apparent and relevant to the paper. Furthermore, reviewer 1 seems to find value in the figure or he would not be suggesting cosmetic changes to it.



c. Reasons for including variables or excluding others seem sometimes far stretched. For example, aerosols are excluded from the analysis although there has been a trend in aerosol extinction in the tropical stratosphere as well as increased variability due to small volcanic eruptions (see for instance Vernier et al., 2011). In contrast EESC is included (together with the linear trend) although the response is not significant. One of the reasons given (it was done in Bodeker et al., 2013) is not convincing. Likewise, the reasons for not considering solar harmonics and seasonal or monthly linear trends are dubious.

The reviewer has missed the point that because aerosol extinction has a trend of its own, plus short-term variability to which ozone might respond, this predictor can bias the ozone trend. We learned this lesson the 'hard way' because aerosol extinction was originally included as a basis function until we realized that it can affect the trend in a major, yet possibly artificial way because observed ozone from both instruments may have aerosol artifacts, particularly from OSIRIS, since the data are not as highly filtered. Others have had the same experience with this predictor variable (Solomon et al., 2012). These points are in the original paragraph about aerosol extinction.
EESC was included after regression-model testing showed that it did not affect the linear trend. Besides EESC, none of the predictor variables have a trend, they are simply oscillations or cyclic or a constant. The reviewer is also misquoting the reason for citing Bodeker et al. (2013). We did not include EESC to simply follow Bodeker et al. (2013), but we included EESC because the observed positive response of ozone to EESC in the tropical lower stratosphere demonstrated by Bodeker et al. (2013) confirms the model results that ozone responds positively to EESC in the tropical lower stratosphere (and probably in the upper troposphere as well) due to the self-healing process (i.e. a trend in ozone destruction in the upper stratosphere causes a positive trend in ozone production near the tropopause).
The inclusion of solar annual harmonics did not improve the linear trend uncertainty. They also explained very little additional variance. This means that these harmonic terms should be excluded from the final regression model according to standard statistical practices. For the seasonal trend, the following similar justification was provided (p16673, line16):

This decision is based on the fact that the seasonal trend was not a statistically significant term based on regression model tests using only SAGE II data above 17 km.

Even without the simultaneous inclusion of the annual cycle term, the seasonal trend could account for a shift in the seasonality of ozone between the two data records. Thus it was best to test this predictor variable on a time series from only one of the instruments. We now add the following sentence immediately after the previous excerpt to elaborate on why the test was performed only on SAGE II data:

Testing with SAGE II alone ensures that the seasonal trend is not simply serving to account for ozone seasonality differences between the two instruments.

7. Discussion of the results of the regression:
a. The response of ozone to the various predictors should be defined (using an equation) as soon as Table 2 is introduced and indication of the 95% CI error bars of the response should be given. A figure could also very well represent the main responses as a function of altitude. To what correspond the values in the column "C"?

We add the following at p16679, line 11:

The ozone response and its uncertainty are calculated as the standard deviation of the basis function multiplied by its fitting coefficient or fitting coefficient 95% CI, respectively. Similarly, for the QBO, we combine the ozone response to each associated, retained basis function (including harmonics) in the following generalized form:

$$response_{QBO} = sd\left(\sum_{n=1}^{2} c_{aQBO_n} QBO_n(t) + \sum_{n=1}^{2}\sum_{x=1}^{2}\left(c_{bQBO_n}\cos(x\pi t) + c_{cQBO_n}\sin(x\pi t)\right)QBO_n(t)\right) \quad (7)$$

where *response* is a time-integrated quantity and *sd* is the standard deviation of all of the monthly points in the time series (e.g. 1984-2012). The overall response to QBO is different from the other responses because it will always result in a positive number when calculated using Eq. (7). The uncertainty in the response to QBO is calculated following Eq. (7), but the fit coefficients are replaced with their respective 95% CIs.

The figure below will essentially replace Table 2. An extra column was added to Table 1 relating to QBO harmonics. The values in the column titled '*c*' in Table 2 of the original manuscript are for the constant term (see Eq. (3)).

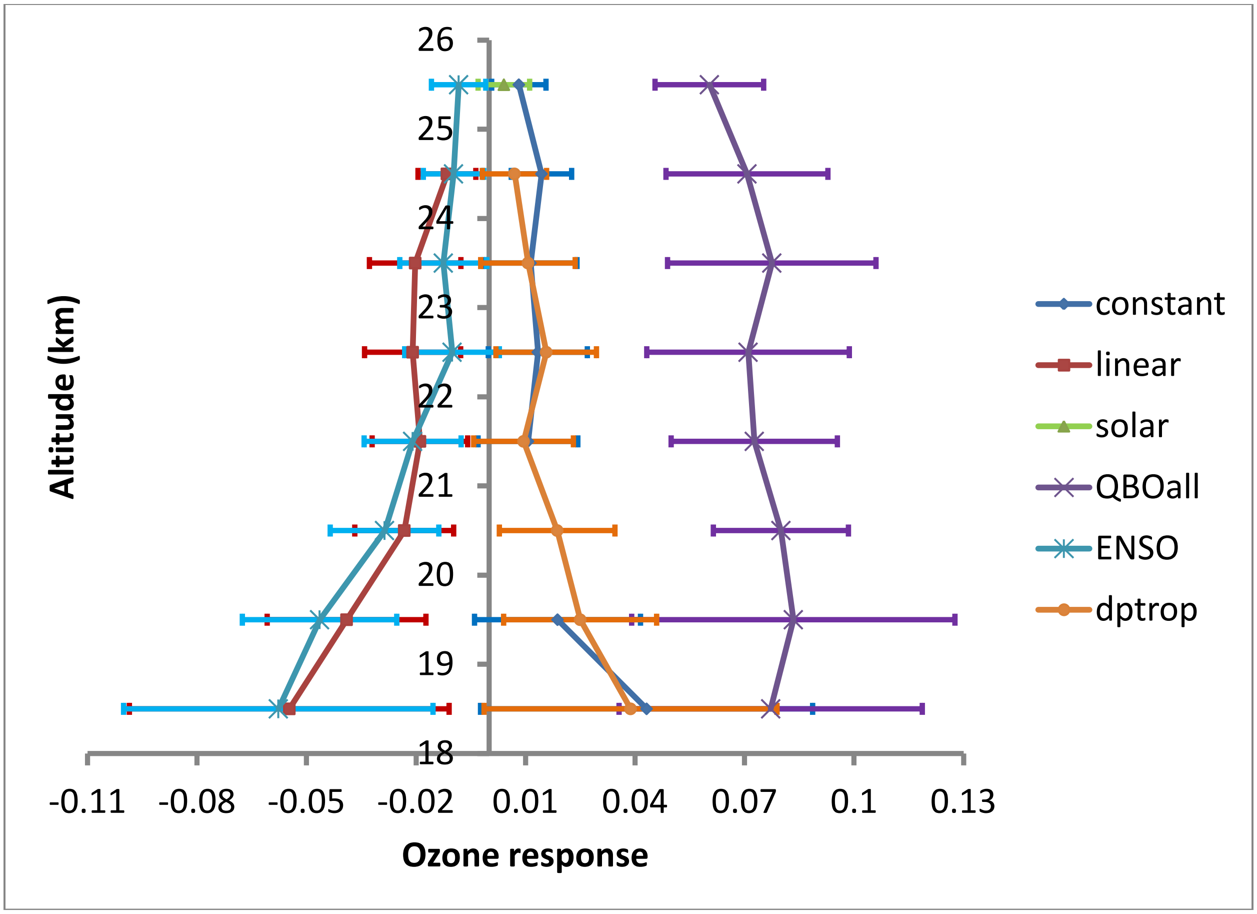


Figure 3. Ozone response and uncertainty (95% CI, accounting for autocorrelation of the residuals) to various predictors (see Eq. 7 and text for formula for response and its uncertainty, except for constant, whose response and its uncertainty are the fit coefficient and its 95% CI). The linear response is proportional but not equal to the decadal trend. QBOall is the combined ozone response to all QBO terms (including harmonics).

b. The discussion of the QBO response in lengthy and cumbersome. The authors should reduce it and provide the main message there.

We deleted three sentences from the QBO response sub-section. The main message is already present as indicated in a previous response:

Figures 5d and f also illustrate that the QBO signature is altitude-dependent and any attempt to fit the QBO signal with time series at a single inappropriate pressure (even with a lag) (e.g. Cunnold et al., 2000; Bodeker et al., 2013) will fail to capture the altitude dependence of the QBO signal. For example, the QBO signal in ozone exhibits sharp temporal changes at the times of extreme amplitude at 24.5 km whereas at 22.5 km, it has much more of a square-wave character (see also Dunkerton and Delisi, 1985). This is particularly evident during the two QBO cycles in the 1998 to 2003 time frame. (…) Thus, it is not surprising that in the 17.5-23.5 km range, fitting the best pair of pressures considerably improves the r^2 relative to fitting two orthogonalized QBO basis functions (Randel and Wu, 2007) derived from all seven pressures, as echoed by Kirgis et al. (2013). In fact, in some cases, a single QBO pressure (e.g. 50 hPa at 19.5

km) explains much more variance than the two orthogonalized QBO basis functions. This method of accounting for the altitude dependence of the QBO in vertically-resolved ozone time series analysis allows for r^2=0.74 using only SAGE II data at 18.5 km in contrast to ≤0.4 found by Randel and Wu (2007) (see also Table 1 for altitude-dependence of r^2 using the merged dataset) and improves the linear trend uncertainty.

Figure 4 (or an additional figure) should include a representation of the residuals. Is autocorrelation taken into account in the derivation of the uncertainties?

In the revised manuscript and in this response, autocorrelation is taken into account in the derivation of the uncertainties. We show the residuals at a sample altitude in the figure replacing the original Figure 4.

d. If one looks attentively at figure 4, one can see that SAGE II monthly data are much noisier than the OSIRIS ones at the lowermost altitude. At this altitude, a decrease in ozone is seen only up to 2005, followed by an increase. Can the authors comment on that behavior? To what extent the trend is mostly influenced by the rather noisy SAGE II data in the eighties?

The evidence we have is that the SAGE II data are less noisy than OSIRIS data at 18.5 km. The reviewer's comment could stem from an illusion related to the sparseness of the SAGE II. The fact that the trend uncertainty is smaller for SAGE II than for OSIRIS (with and without considering the effect of autocorrelation) supports this, especially considering that OSIRIS time series has more data points than the SAGE II one (particularly at 18.5 km). This is more impressive when one considers the following: the amplitude of the observed ozone anomaly is larger in SAGE II data and we do not believe this is related to noise or to the trend. The reviewer is attempting to see noise when the variations are not dominated by noise, but rather real variability due to QBO and other predictors. Obviously the data in the 1980s affects the trend, but we see no reason to exclude it if it is not noisier.

e. Figure 3 should include a comparison with model results as shown in Lamarque and Solomon (2010). In this paper, the negative trend peaks around 70 hPa and decreases below, while in the present study, the trend is negative down to 18 km. Can the authors comment on that? Since a negative trend of around 3% per decade can be attributed to a decrease in tropopause pressure, how much is the trend that can be attributed to increases in greenhouse gases?

We will insert the model trend profile of Lamarque and Solomon (2010) in Figure 6 (below), which is an addition to the original manuscript. We feel it is more appropriately placed there since our calculated trend is also there. The trend in greenhouse gases appears to be almost entirely responsible for the trend in tropopause pressure according to the results of Lamarque and Solomon (2010), if one accepts that SST increases are driven by greenhouse gas increases on decadal timescales. According to our simple model, the trend that can be attributed to increases in greenhouse gases is shown in Figure 6 (either curve beginning with 'both'). The Lamarque and Solomon model (2010) may account for feedbacks which could be very important in the tropopause region. However, the reviewer is likely referring to Figure 2 of Lamarque and Solomon (2010), which also shows a higher peak height than the observed trend by Randel and Wu (2007). Furthermore, their Figure 6 (top) shows a maximum trend magnitude near 70 hPa particularly at the southern end of their much wider latitude band (20°N-20°S), whereas in a band similar to ours (4°N-4°S), the trend shows no sign of decreasing all the way down to the tropopause.

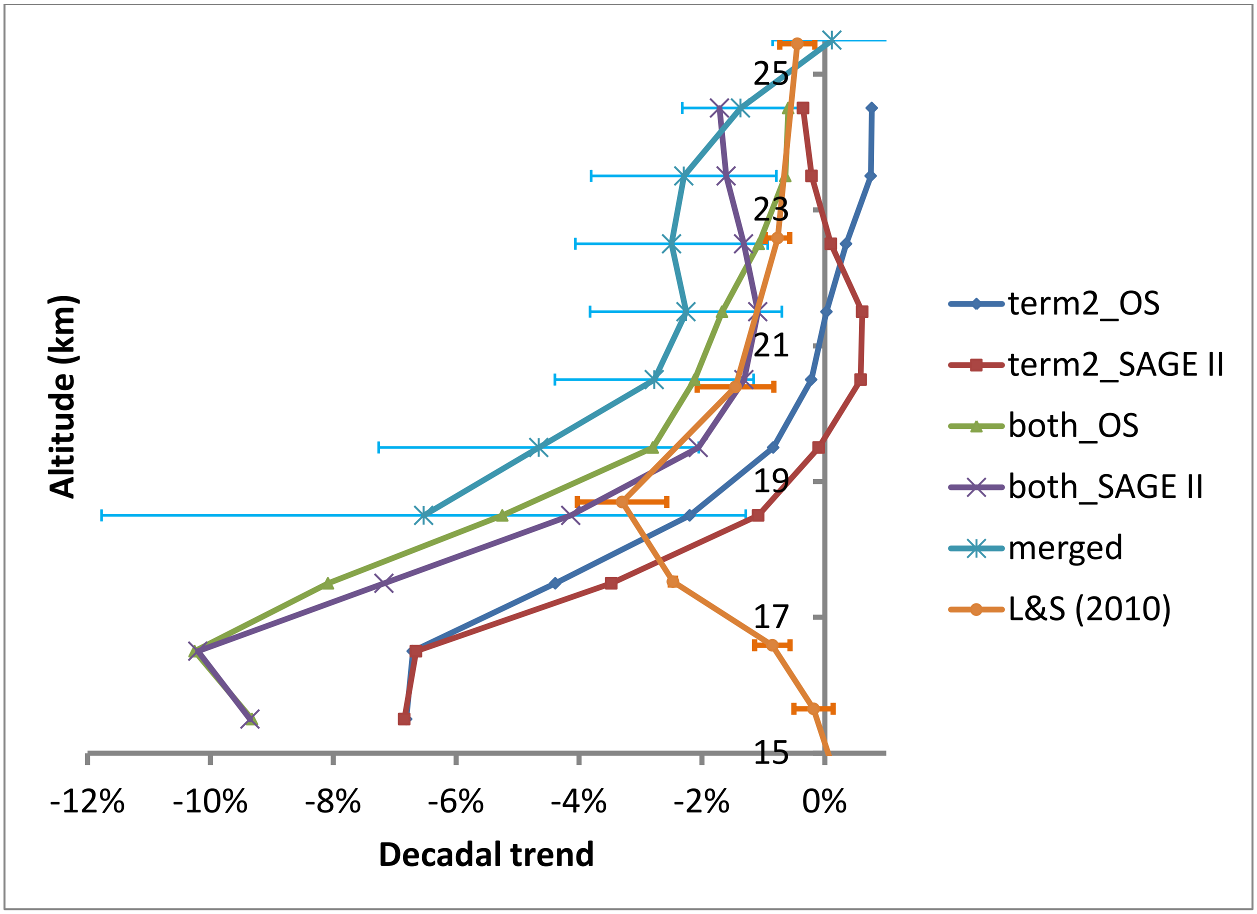


Figure 6. Observed and calculated ozone trends for 1984-2005. The observed trend is labelled 'merged' and is the trend shown in Figure 4 for the merged dataset. The trend profiles labelled 'both_OS' and 'both_SAGE II' are calculated trends including contributions from the trend in tropical upwelling and the trend in the vertical gradient of ozone, with the vertical gradient of ozone supplied by OSIRIS or SAGE II, respectively. The profiles labelled term2 consider only the contribution from the trend in the vertical gradient of ozone (see Sect. 4.1 for details). L&S (2010) is the modelled trend from Lamarque and Solomon (2010) for 1979-2005 for 20°S-20°N.