Response to Referee #2:

We are grateful to the referee for her/his very careful reading of the manuscript and for her/his constructive comments and suggestions. Responses to each individual comment that has been quoted […] are given here below.

General comments

1/ *[This manuscript is largely an update of Wespes et al., 2016 but includes 4 more years of data.]*

This manuscript is indeed built on previous IASI studies, but we hope that the referee will appreciate that it is actually more than an update of Wespes et al., 2016, insofar as the regression model is more complex and here adapted to stratospheric studies with the inclusion of specific proxies (accounting for the aerosols, the volume of PSC and the Eliassen-Palm flux), and as the analysis is now performed at the global scale, not on a zonal basis, which allows us to better demonstrate the added value of the IASI dataset.

2/ [The manuscript is well written, though there are occasions where the wording is confusing, likely due to language issues]

We are grateful to the referee for suggesting a series of English style corrections in her/his technical comments below. They have all been included in the revised paper.

Major comments

1/ *[My primary comment concerns the analysis and conclusion that the ozone response to CFCs is changing in time. The authors base this conclusion on a series of linear fits over varying time periods, which show sharper trends (both positive and negative) in the most recent data relative to trends in the record from earlier start points. The series of trends is computed after the sources of natural variability, as fit over the full IASI time period to the most relevant proxies, are removed. Nevertheless there will still be variability in the time series that has not been perfectly captured by the regression model. If that variability has autocorrelation on a longer scale (months), a tendency for the data to be high or low at the beginning or end of the record, which might actually be due to uncaptured noise, will disproportionately affect the trend. If this is the case, such a variation at the end of the record will have successively more influence as the fit period gets shorter, as the end point of each fit is the same.]*

The referee is right; the uncaptured variability might disproportionately affect the estimated trends when calculated over varying time periods, but, so might be the calculation of the associated uncertainty. This is specifically addressed in Fig.12 of the paper that illustrates the time evolution of both trends and associated uncertainties over varying time periods.
We agree that the comparison of trends calculated over different lengths of time is not straightforward and that considering successive time segments of same length would make the statistical error more comparable across the fits. Nevertheless, there are limitations in using successive identical segments as discussed below in response to the referee's suggestions. We note finally that the uncaptured variability might also induce different systematic errors between segments (of same or different lengths), e.g. in case of "trend-like" noise over a specific segment.
In order to address this issue, we now consider the autocorrelation in the noise residuals in the uncertainty estimation illustrated in Fig.12.

As discussed below in response to the two next referee's comments, we believe that the results shown in the revised Fig.12 of the paper are the best way to represent the time evolution of the trends over the 10-

years IASI period. In addition to modifying the figure, we have taken care to better balance the findings through the revised manuscript, especially in the title, the abstract and the conclusions (see our responses here below).
Finally, we have also found a bug in the calculation of the estimated trends through the manuscript. We apologize for this. The overall conclusions remain unchanged but the Figures 8 to 12, and the numbers given in the text have been corrected accordingly.

2/ *[If I understand correctly, the associated uncertainty plots in Fig. 12 tell us that each trend is different from zero trend at the 95% level, but that does not mean that the trend fit over the last 2 years is different from the trend fit over the last three years or last 4 years at the 95% level. For example in the SH high latitude LST the initial trend is _ 1 DU/yr with uncertainty of say 0.25 DU/yr (difficult to tell exact numbers from the contour plots) and the final trend is approaching 2.5 DU/yr with an uncertainty of close to 1.5 DU/yr, meaning the initial and final trends are not statistically significantly different or only barely so, depending on the exact numbers.]*

We would like to point out that the exact numbers are given in Section 4.4 of the manuscript, specifically for the SH high latitude LSt: “In the LSt, a clear speeding up in the southern polar O_3 recovery is observed with amplitude ranging from ~1.5±0.4 DU/yr over 2008-2017 to ~5.5±2.5 DU/yr over 2015-2017 on latitudinal averages.” Hence, the reader could appreciate that the initial and the final trends are statistically different from each other, despite the larger amplitude of the uncertainty over the shorter periods. This is further illustrated in Figure 1 here below which represents the lowest amplitude of the estimated trend, by subtracting, from the absolute value of the linear trend, the associated uncertainty that includes the autocorrelation in the noise residual.

The colorscale in the revised Fig.12 has been modified to avoid the saturation in order to address the comment on the lack of clarity. In addition, the uncertainty now accounts for the autocorrelation in the noise residuals and, hence, the uncertainty values are corrected accordingly throughout the manuscript.

3/ *[I believe a more appropriate approach would be to fit trend segments over the same length of time, with varying start and end points. The authors could compare the time evolution of trends over 2-yr segments, 3-yr segments, 4-yr segments and longer. The 2-yr segments would be the trend fit from 2008-2009, 2009-2010, 2010-2011,… 2015-2017. 3-yr segments would be 2008-2010, 2009-2011,…, 2014-2017 and 4-yr 2008-2011, 2009-2012, …, 2014-2017, and so on. In this way both the start and end point will vary, and each fit has the same length, such that the uncertainty is similar across the fits. If the results show consistent changes in time in the fit trends that are greater than the inherent uncertainty, this would indicate a change may be taking place. As the segments get longer (4-yr +) the change in trend will be less from segment to segment, but so will the uncertainty threshold that must be met to show significant change. So the authors can check for consistency in the trends within each segment length vs. time and consistency between 2-yr, 3-yr, 4-yr etc… segment results to determine if there is a shift in the ozone change rate.]*

We are grateful to the referee for this interesting suggestion. However, there are some limitations in using that approach:

- By fitting long segments, we would compare trends that are estimated over similar periods; i.e. for instance, 8-yr segments would imply comparing trends over 2008-2015 *vs* 2010-2017, which would smooth a progressive acceleration in the ozone change rate over the 10-year IASI period.
- By fitting short segments, we would induce a large uncertainty on the trend estimate (because of few data points and a hardly detectable trend from the noise) and, hence, less-conclusive results.
- The jump that occurs in September 2010 in the IASI dataset could over-represents disproportionally the estimated trends when they are calculated over short segments that encompass the jump period.

To follow the referee's suggestion, we have therefore investigated if the change rate in IASI O_3 could be inferred from segments that are long enough to lead low uncertainty and limit the jump effect. This is illustrated on Figure 2 here below that shows the trend evolution over 6-year, 7-year and 8-years segments in the LSt. Despite the smoothing of the trends over long periods, the progressive acceleration remains observed, especially in the Southern mid-latitudes. The results are also quite consistent with the revised Fig. 12, which gives more confidence in the speeding up observed in IASI LSt O_3.

Given the limitations discussed above, we believe that Fig.12 of the paper is the best way to represent the progressive acceleration in the O_3 recovery. Nevertheless, we agree that the IASI period is still relatively short to compare trends over successive segments of same length that are long enough to reduce the uncertainty. In addition, we calculate that the largest trend amplitudes derived over the last years of the IASI measurements would actually require a longer detection length than the covered time segments. Therefore, as suggested by the referee#1, we use, in the revised version, a more careful wording about the speeding up of the O_3 trends through the manuscript, especially in the abstract, in Section 4.4 and in the conclusions.

For example, one can read now at the end of the abstract: "Additional years of IASI measurements would, however, be required to confirm the O_3 change rates observed in the stratospheric layers over the last years" and at the end of Section 4.4: "Nevertheless, we calculated that additional years of IASI measurements would help in confirming the changes in O_3 recovery and decline over the IASI period (e.g. ~ 4 additional years are required to verify the trends calculated over the 2015-2017 segment in the highest latitudes in LSt). In addition, a longer measurement period would be useful to derive trends over successive segments of same length that are long enough to reduce the uncertainty, in order to make the trend and its associated uncertainty more comparable across the fit."

The title of the manuscript has also been changed accordingly to: "Is the recovery of stratospheric O_3 speeding up in the Southern Hemisphere? An evaluation from the first IASI decadal record".
An alternative to that title would be: "First signs of a speeding up in stratospheric O_3 recovery in the Southern Hemisphere, contrasting with a decline in the Northern Hemisphere, as seen from IASI".

4/ *[I also believe showing some example time series of the data being fit, after the other variations have been removed, would be very useful in this particular analysis.]*

We thank the referee for that suggestion. Some typical examples of gridded daily time series in the S.H. mid-latitudes in the LSt, after the fitted natural variations have been removed, are provided in the Figure 3 here below. The residuals clearly show positive trends. The fitted significant trends over varying periods ending in 2017 are superimposed. The trend values and associated uncertainties are also indicated for a conclusive evaluation of the significant O_3 change rate in stratosphere. While the speeding up is significant from the zonally averaged trends (see the revised Fig. 12 and the Figure 1 here below), it is more hardly but still detectable over individual gridded time series. Examples have been added in the revised Supplement.

5/ *[Finally, when doing this analysis, is the VPSC term also removed, or is this term considered part of the ozone response to CFCs and thus left in the time series? Similarly, in reference to the jump in the data in September 2010, although this may be small relative to the full trend, does this jump influence the results of the time dependent trend analysis shown in Fig. 12, or has it's effects been removed before fitting these trends?]*

All the adjusted proxies, including the VPSC term, have been kept fixed (or removed) in the trend analysis over varying time periods, so that any changes in the adjusted O_3 drivers (including in VPSCxEESC) over

time do not influence the trend estimation. It is now clearly mentioned in the revised manuscript that VPSC is removed as well:
"…the ozone response to each natural driver *(including VPSC)* taken from their adjustment over the whole IASI period (2008-2017; Section 3, Fig.5) is kept fixed."

On the contrary, for consistency with Chapter 4, the jump found in the IASI data in September 2010 has not been removed from the trend analysis shown in Fig.12 and, hence, it could influence the trends calculated over the periods starting before the jump only (i.e. 2008-2017, 2009-2017 and 2010-2017). However, the jump is of positive sign and, hence, it does not contribute at all to the acceleration observed in the IASI O_3 change rates over the 10-year period. It would even mask it when comparing the trends estimated over periods starting before *vs* after the jump. This has been added in Section 4.4:
"The jump found in the IASI O_3 records on September 2010 (see Section 2.1) is not taken into account in the regression; hence, it might over-represent the trend estimated over periods that start before the jump only (i.e. 2008-2017, 2009-2017, 2010-2017)."

Minor comments

1/ *[Can the authors say more about the difference between fitting a daily record and a monthly mean record? I know this was addressed in the 2016 paper, but I am particularly interested in the error analysis. Is the daily autocorrelation similar to the monthly autocorrelation? For long-term trends, the uncertainty is more impacted by correlations in the residual on longer time scales rather than day to day variations. Is the lag-1 autocorrelation term used to scale the uncertainty similar when considering daily data and monthly data?]*

The autocorrelation coefficients at various lags corresponding to a daily mean record *vs* a monthly mean record were examined for the 2 stratospheric layers (cfr Figure 4 here below for the latitudinal distributions of the lag-1 to -4 autocorrelation terms in daily *vs* monthly data fitting in the MUSt). As expected, the lag-1 autocorrelation term appears to be the most important in all cases (daily and monthly) and is found to be much larger in the daily than the monthly mean records. This means that the correction of the uncertainty estimate, by the autocorrelations in the noise residual, is larger when adjusting daily data, i.e. the uncertainty associated to the fitted trend is much more impacted by the autocorrelation when fitting a daily record, but, as shown in the 2016 paper, it is compensated by a better quality adjustment, which, hence, reduces the amplitude of the uncertainty in daily *vs* monthly data records.

2/ *[Although I appreciate not wanting to add too much to the paper, I think it would help the reader to repeat the basic equations defining the multivariate model in this paper. At different times three different papers are referenced for equations concerning the model. I think it would be easier to just include all relevant equations in this paper, including the normalization equation.]*

The MLR and the normalization equations are now included in the revised paper at the start of Section 2.2.

3/ *[Very little is said about the seasonal cycle, though the model description includes terms for the annual and 6-month harmonics (pg 5.). Can the authors comment on the seasonal cycle, and particularly do they see the seasonal cycle interacting with EPF and VPSC, which are both also correlated and look very seasonal in nature. Similarly on the interaction between EPF and VPSC, in Fig. 7a in the NH high latitudes the ozone variability explained by the proxies for EPF and VPSC are similar and well above the variability of the actual IASI ozone. Is this another way of showing that the two terms falsely depict variability that isn't in the actual data, but that variability cancels when the terms are added? Have the authors tried fitting to one or the other of the terms, rather than both terms? Particularly in the Austral Spring, where the authors believe the VPCS signal is real, is the amplitude of that signal sensitive to whether or not EPF and/or the seasonal cycle are fit?]*

Correlation between the annual cycle and EPF is of course expected. In several previous papers, the harmonic terms are even used to adjust the effects of the Brewer-Dobson circulation in addition to the seasonal cycle of insolation, but then the interannual variability is not captured. However, the EPF and VPSC proxies show sufficiently year-to-year variations to limit the compensation effect between each other and with the 1-yr harmonic term.

In order to verify this, as suggested by the referee, an annual MLR without including EPF has been performed to better evaluate the possible discrimination between the EPF, VPSC and 1-yr harmonic terms. This is illustrated for LSt in Figure 5 here below that represents the global distributions of the adjusted coefficients for the 1-yr harmonic ($\sqrt{a_1^2 + b_1^2}$) and the VPSC regression coefficients from the annual MLR without EPF *vs* the reference one. We show that the global distributions of the VPSC regression coefficients between the two MLRs are similar, which indicates a good discrimination between the two parameters on an annual basis. For the 1-yr coefficients, the overall global distributions look similar with, however, some expected but small differences relative to the EPF contribution, especially over the high latitudes where the EPF contribution is the largest. In addition, it is worth noting that the likely correlation between the VPSC, EPF and 1-yr terms is taken into account in their associated uncertainties.

Some words of caution have been added in the revised Section 3 about a possible correlation between the annual harmonic term and the EPF proxy:
"Furthermore, given the annual oscillations in EPF, compensation by the 1-yr harmonic term (eq. 1, Section 2) is found (data not shown), but it remains weaker than the EPF contribution, in particular at high latitudes where the EPF contribution is the largest."

We would like to point out that the likely correlation between VPSC and EPF was already mentioned in the paper in Sections 2.2 and 3 which describe the proxies and their adjustment: "Correlations between VPSC and EPF are possible since the same method is used to build these cumulative proxies". They can indeed compensate each other by construction given the opposite sign of their regression coefficients. However, we highlight the physical meaning behind the sign of their regression coefficients and the differences between the spatial distributions of their regression coefficients (see Fig.5 of the manuscript), which indicate a discrimination between these two variables.

On a seasonal basis, the austral spring is the period when VPSC is the largest and dominates over EPF in the S.H.; this is consistent with the role of PSCs on the polar O_3 depletion chemistry and the smallest EP influence due the formation of the O_3 hole, in comparison with the N.H. However, a compensation effect might indeed explain the large similar VPSC and EPF variability in the N.H. high latitudes in fall, as it was already mentioned in the paper: "The strong VPSC influence found at high northern latitudes in fall (Fig. 7a) are likely due to compensation effects with EPF as pointed out above."

The good discrimination in austral spring and the compensation effect in the N.H. fall are verified in the Figure 6 here below that compares the latitudinal distribution of the 2σ O_3 variability in VPSC, from the seasonal MLR with or without including EPF. The amplitude of the variation explained by VPSC are similar between the two seasonal MLRs in the Austral spring, while, not in the N.H. fall. The results in Figures 5 and 6 here demonstrate a good discrimination between the two covariates yearly and in the Austral spring.

In the revised version, we now mention:
"The strong VPSC influence found at high northern latitudes in fall (Fig. 7a) are due to compensation effects with EPF as pointed out above and verified from sensitivity tests (data not shown)."

Finally, we believe that it does not make sense to remove both the 1-yr harmonic term and EPF from the MLR model; the annual cycle that is caused by solar insolation which is the main driver of the observed O_3 variability will no longer be represented, which will lead to erroneous results.

4/ *[Can the authors discuss comparisons between IASI total ozone and other sources of satellite total ozone measurements? It is difficult to compare trend values presented here with previous studies (Weber et al for example) because of the different time periods fit, and zonal mean vs high spatial resolution gridded trends. Have IASI total ozone trends been directly compared to trends from any of the other total ozone satellite records? It would be very useful to also see how the data themselves compare in total ozone, either through reference to previous work or in a comparison plot in this manuscript.]*

Performing comparisons between O_3 trends derived from IASI *vs* other satellite instruments would be of course interesting for evaluating the inferred trends and the relevance of the current datasets to carry out trend studies. However, it is a significant endeavour that is beyond the scope of the present study. Actually, this will be specifically addressed in the frame of the recently started Ozone_CCI+ program where the IASI O_3 trends will be compared to those estimated from GOME-2 (both onboard the Metop platforms) over exactly the same time period and using the same MLR model/method. In that way, the bias resulting from different time periods, spatial/temporal samplings and trend calculations will be excluded.

5/ *[Can the authors address how the seasonal averages are constructed? In particular, the authors specifically investigate the JJA trends over the South Pole and Antarctica, but it appears from Fig. 4a there is very little is any coverage in the deep winter at polar latitudes, but that coverage increases with latitude towards the equator. Are the JJA averages for each grid point made with any available data, or is a threshold set, and does the coverage vary with latitude in the polar regions in Figure 10 and 11?]*

The distributions of seasonal trends provided in Fig. 10a and 11a of the paper do not correspond to averages; instead they represent the adjusted seasonal trend parameters for each grid cell (see our response to the technical comment [*L270-272*] below). It is true, however, that the coverage vary with latitude in the polar regions since only the daytime measurements are used in the paper (as mentioned in Section 2.1). This explains the gap (grey cells) over the polar regions during both austral and northern winters in Fig. 10a and 11a of the paper, in comparison with the other periods (Fig. 10b and 11b) and the annual trend distributions (Fig. 8 of the paper).

Technical comments

1/ [*The use of the absolute value signs around the trend values was a bit confusing. I can see this when talking about the amount of time needed to detect a trend of |x| DU yr-1 because this can be a positive or negative trend, but in other cases the authors state the trend is positive or negative, and in that case it is unclear why the absolute value designation is needed. For example on page 15, the absolute value bars are not needed in lines 561 and 564. In line 591, is this a positive trend of 1.5 DU/yr or do you mean positive or negative? If the authors do not mean to say this value can be positive or negative, I would suggest removing the absolute value bars and just stating positive or negative (such as in line 594, positive is stated so the bars can be removed, to me at least the bars imply positive or negative).*]

The consistency in using the absolute value bars has been checked through the manuscript. The absolute bars are now only used when discussing the detectability of a specified trend (i.e. when the trend can be of both positive and negative values); in other cases, the sign is specified.

2/ [*L12 should this be > 25hPa or < 25hPa? Since the units are in hPa I suggest it is < as in 25 hPa and lower pressures. L34 in a lesser -> to a lesser. L41 introduce O3 after ozone. L43 gas. In the stratosphere*

L45 for regulating -> to regulate. L45 introduce chlorofluorocarbons here, at first use of CFCs. L47-48 suggest These latter are the origin of the massive -> CFCs cause. L46-54: In general, I don't think the timing is correct is this introduction to the phase out of the CFCs. At the time the Vienna Convention was ratified, and the MP for that matter, it was not yet proven that CFCs were the cause. The Vienna Convention was ratified based on the theory that CFCs could cause ozone destruction; I don't believe the Farman paper was even released yet. All this to say, even though this is just an introductory paragraph I think it is important to be precise on the history, the implication in the wording is that the ozone hole was discovered first and everything else was a reaction to that discovery. L56 Suggest removing first phrase, and start sentence as A recovery from… L59 This is decline of CFCs in the stratosphere, correct? L61 confirmed -> identified. L67 polar region -> polar regions. L68 No reliable estimates of long-term trend -> Statistically significant long-term recovery in total O3 column on a global scale has not yet been observed, likely because… L71 low -> lower. L75 I believe there are other references here as well. Check Wargan, K., C. et al. Recent decline in lower stratospheric ozone attributed to circulation changes. Geophys. Res. Lett., 45, no. 10, 5166-5176, doi:10.1029/2018GL077406. L81 controversy -> uncertainty. L82 sensitive -> difficult. L109 applied on -> applied to. L110 remove 'of'. L172 and contrasts with -> rather than]

Thank you for these corrections. The text has been revised as suggested. Note in particular the following points:

- O_3 was already introduced in the abstract.
- The timing in the introduction has been corrected in the revised version. The Farman et al. paper was accepted (28 March 1985) just after the Vienna Convention (22 March 1985).
- Wargan et al. (2018) has been added in the introduction.

3/ [L*178-180 the effect of the jump is found small enough to explain the trend? I'm not sure what the authors mean here.]*

Changed to:
"The estimated amplitude of the jump is found to be relatively small in comparison to that of the decadal trends derived in Section 4, hence, it cannot explain the tendency in the IASI dataset. Therefore, the jump is not taken into account in the MLR."

4/ [*L192 In order to unambiguously -> In an effort to unambiguously (we try to separate unambiguously, but it is never perfect). L209 of the mixing*]

Done as suggested.

5/ [*L270-272 I'm not sure what the authors are trying to say here. Including the equations would help here. There is already a seasonal cycle in the original model, so it is not clear how the seasonal terms are added. Is this the equivalent of 4 separate runs, one for each season? Equations would also clarify how the seasonal MLR is used after the annual MLR is run.]*

As it is stated in the paper, the seasonal MLR replace the annual functions with 4 seasonal functions, i.e. by adjusting 4 coefficients (one for each seasonal functions for the main proxies, instead of only one coefficient per annual function in the annual MLR). Hence, in the seasonal MLR, the explanatory variables are split into four seasonal functions ($x_{spr} X_{norm,spr} + x_{sum} X_{norm,sum} + x_{fall} X_{norm,fall} + x_{wint} X_{norm,wint}$) that are simultaneously and independently adjusted. There is only one run (as for the annual MLR) with 4 adjusted parameters per proxy. Note that this is not to be confused with the seasonal cycle (harmonic terms) which is treated exactly the same way in both the annual and seasonal formulation of the MLR model (only one annual coefficient is adjusted for each harmonic function). Hence, the seasonal MLR is not equivalent to 4 separate runs. The seasonal MLR takes into account the different influence of the geophysical processes onto O_3 across the seasons, while the annual model is more constrained by the adjustment of year-round proxies which, hence, induces larger systematic errors.

The sentence has been rewritten in the revised version to:
"In the seasonal formulation of the MLR model, the main proxies ($x_j X_{norm,j}$; with x_j, the regression coefficient and $X_{norm,j}$ the normalized proxy) are split into four seasonal functions ($x_{spr} X_{norm,spr} + x_{sum} X_{norm,sum} + x_{fall} X_{norm,fall} + x_{wint} X_{norm,wint}$) that are independently and simultaneously adjusted for each grid cell."

6/ [*L285-288 suggest for clarity not switching the order of the reported results, in L288 LSt goes first and in 291 MUSt is reported first. L302 counteracted -> counteracting (this may occur in other places as well in the text). L 321 suggest adjusted signal of the proxies -> reconstructed proxies. L333 shows up as a typical… L347 MUSt, (remove 'n'). L360 records -> values. L392 deployment ->formation. L414 remove 'have'. L460 in the case of prolonged…*]

Done as suggested.

7/ [*L555 I do not see polar trends reaching 2.5 DU/yr in the MUSt? The trends are positive in the NH pole but negative over Antarctica, and the scale only goes to 2 DU/yr. L560 The authors call out the similarity between the MUSt and LSt with both showing high positive trends at southern polar latitudes, but again at the pole the MUSt trend appears negative, though the trends at southern high latitudes are positive. This description seems a bit confusing and doesn't seem to match Figure 8.*]

Some cells were indeed characterized by trends of 2.5 DU/yr even if the color scale is saturated at 2 DU/yr for clarity. From Fig.8a of the manuscript, one can see that the trends in MUSt are positive almost everywhere, except over Antarctica, with the largest values over the northern polar region and around Antarctica for the S.H.
"(except over Antarctica)" has been added in the revised text to exclude this from the discussion.
Note that the Fig. 8 and the corresponding values given in the text have been revised to correct a bug, as mentioned above.

8/ [*L596 an additional _ 7 years. L599 suggest The longer required measurement periods at high latitudes is due to the larger residuals in the regression fits (i.e. largest sigma e) at these latitudes (see Fig 4 a and b). L613 is there a reason the authors occasionally switch to DU per decade? If not, I suggest keeping DU per year. At first I could not understand why such a large value of 15 was used, then I saw it was DU per decade. L623-624 again it seems the increase in total ozone at high southern latitudes is dominated by the LSt result over the pole though both layers contribute in the latitude bands surrounding Antarctica, comparing to the results in Fig. 8.*]

Done as suggested.

9/ [*L652 summer -> austral winter. L674 over Antarctica (remove 'the'). L696 Salomon -> Solomon*]

Done as suggested.

10/ [*L686 what makes the negative trends here unrealistic?, It seems that the large positive trends off the coast of Antarctica have a similar detection length. I see that there is a bit more uncertainty in the fit in the negative trend region, but to say they are unrealistic requires more specific evidence, such as a time series showing the failure of the fit. I suggest the authors either provide more evidence or simply note that the* *area of higher negative trends is associated with a higher residual from the model. Could it also be something that is happening in the troposphere that is affecting the total ozone trend.*]

“unrealistic” has been replaced by “higher”; The large positive trends around Antarctica have a shorter detection length.

11/ [*L705 This is just a suggestion, but to make the interpretation for the reader easier, could the authors provide the relevant IASI mean ozone values (or climatological values) so the readers can translate between DU/yr and % per dec when comparing results from other studies.*]

The trend in IASI TOC is now given in %/dec as well.

12/ [*L766 suggest However, a longer period of IASI measurements is needed to unequivocally demonstrate a positive trend in the IASI record. L775 additional measurements for the trend to be unequivocal. L781 suggest These results verify the efficacy of the ban on ozone depleting substances imposed by the Montreal Protocol and it’s amendments throughout the stratosphere… L788 and it likely* -> *which likely. L807 in the near future. L809 extent* -> *extend*]

Done as suggested.

Figures

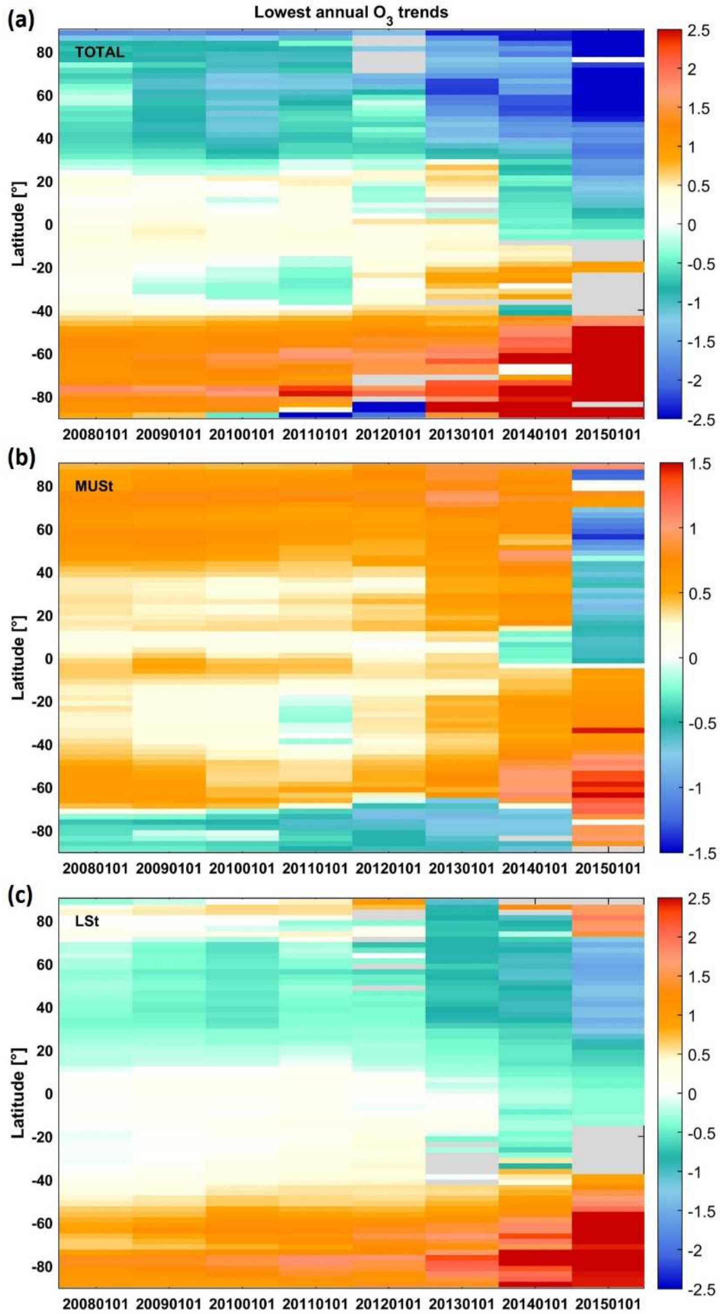

Figure 1: Evolution of estimated linear trend (DU/yr) minus the associated uncertainty accounting for the autocorrelation in the noise residual (DU/yr; in the 95% confidence level) in (a) the total, (b) the MUSt and (c) the LSt O_3 columns, as a function of the covered IASI measurement period ending in December 2017, with all natural contributions estimated from the whole IASI period (2008-2017).

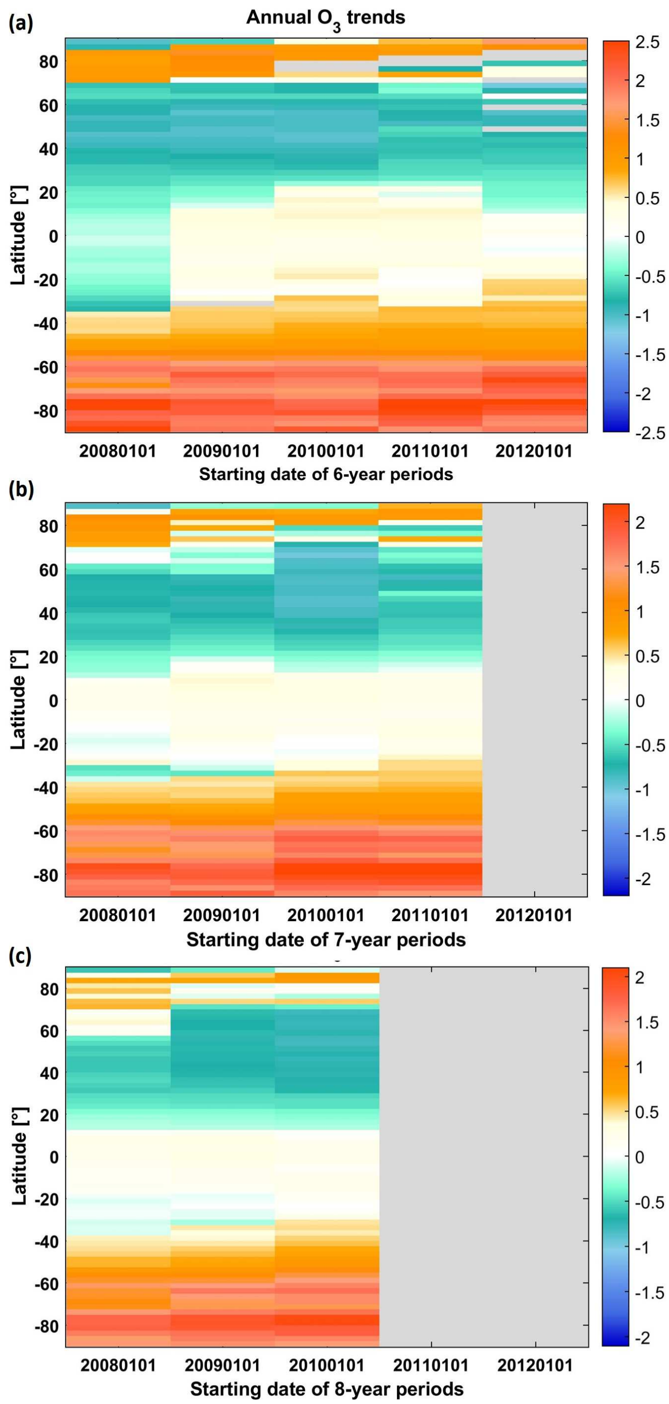

Figure 2: Evolution of estimated linear trend (DU/yr) in the LSt O_3 columns, over (a) 6-year, (b) 7-year and (c) 8 years segments of IASI measurements, with all natural contributions estimated from the whole IASI period (2008-2017).

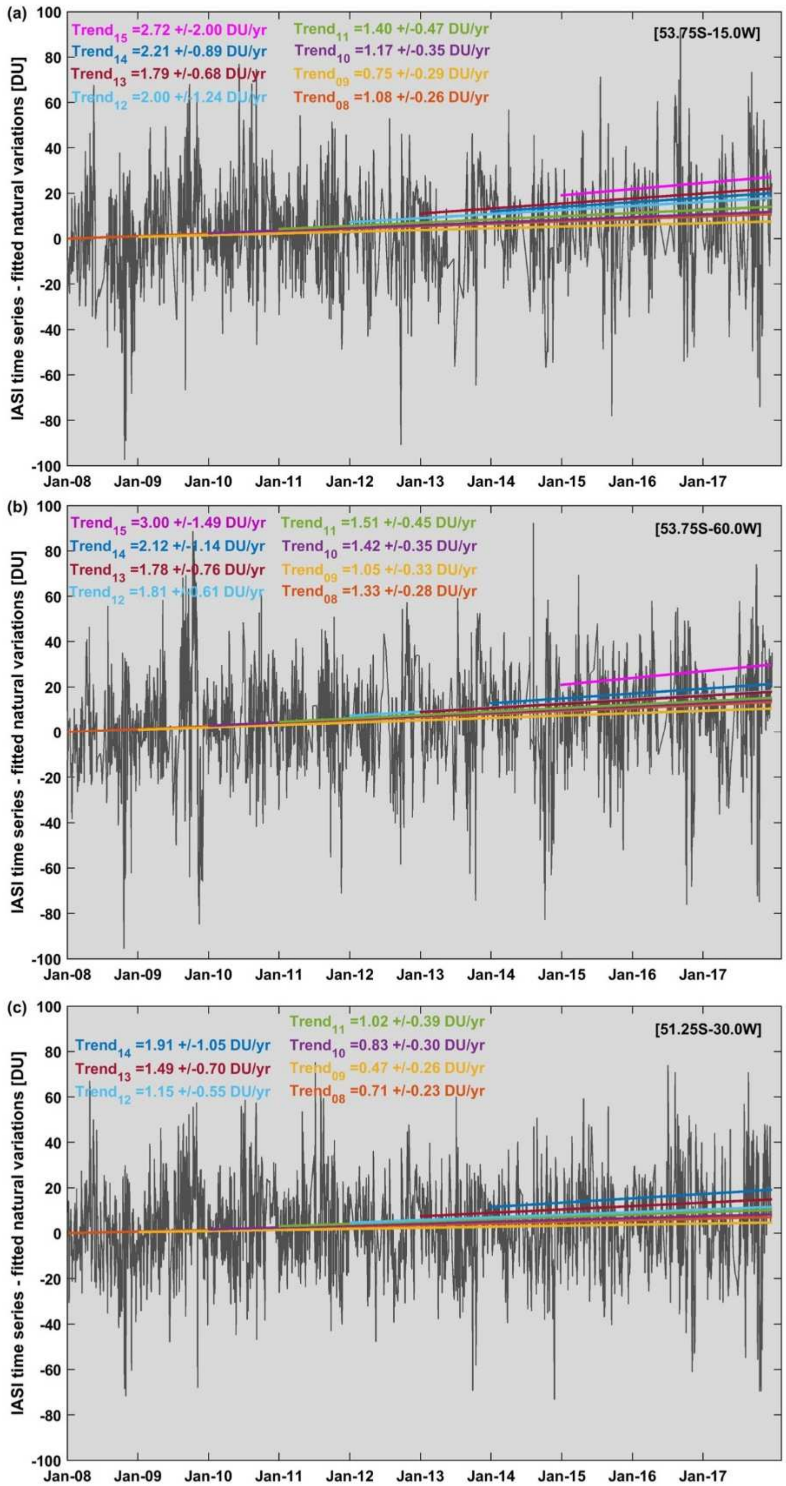

Figure 3: Example of gridded daily time series of O_3 measured by IASI in the LSt over the period 2008-2017 with all the contributions to O_3 variations adjusted from MLR over the full IASI period removed, except for the trend (in DU). The averaged significant fitted trends calculated over varying time periods from a single linear regression are superimposed. The trend values with associated uncertainty (in the 95% confidence level; in DU/yr) are indicated.

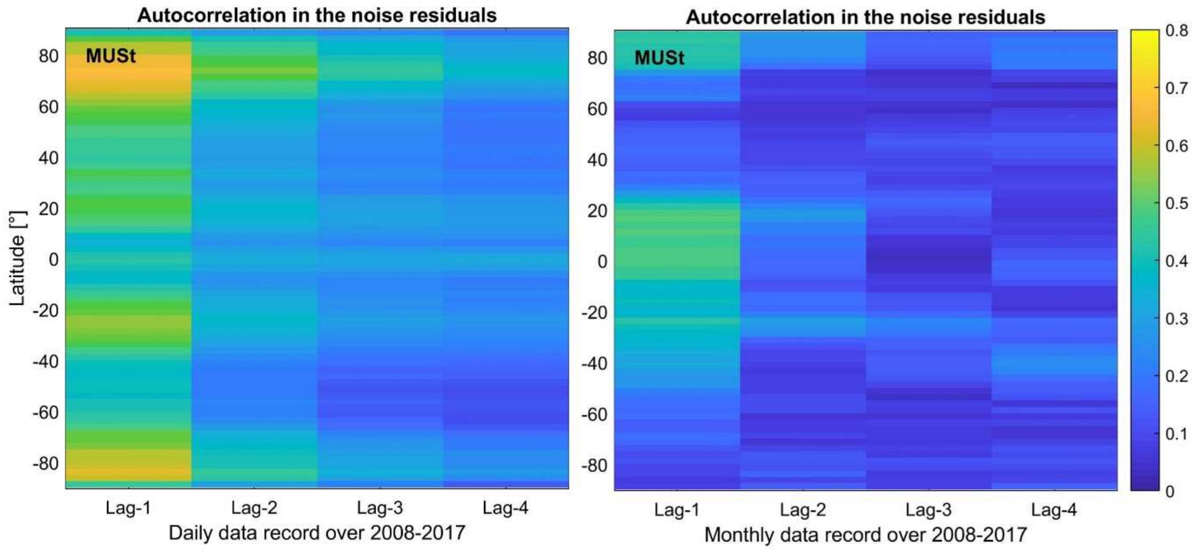

Figure 4: Latitudinal distribution of the lag-1 to -4 autocorrelation terms in the noise residuals when fitting a daily mean (left panel) *vs* a monthly mean (right panel) record in the MUSt over 2008-2017.

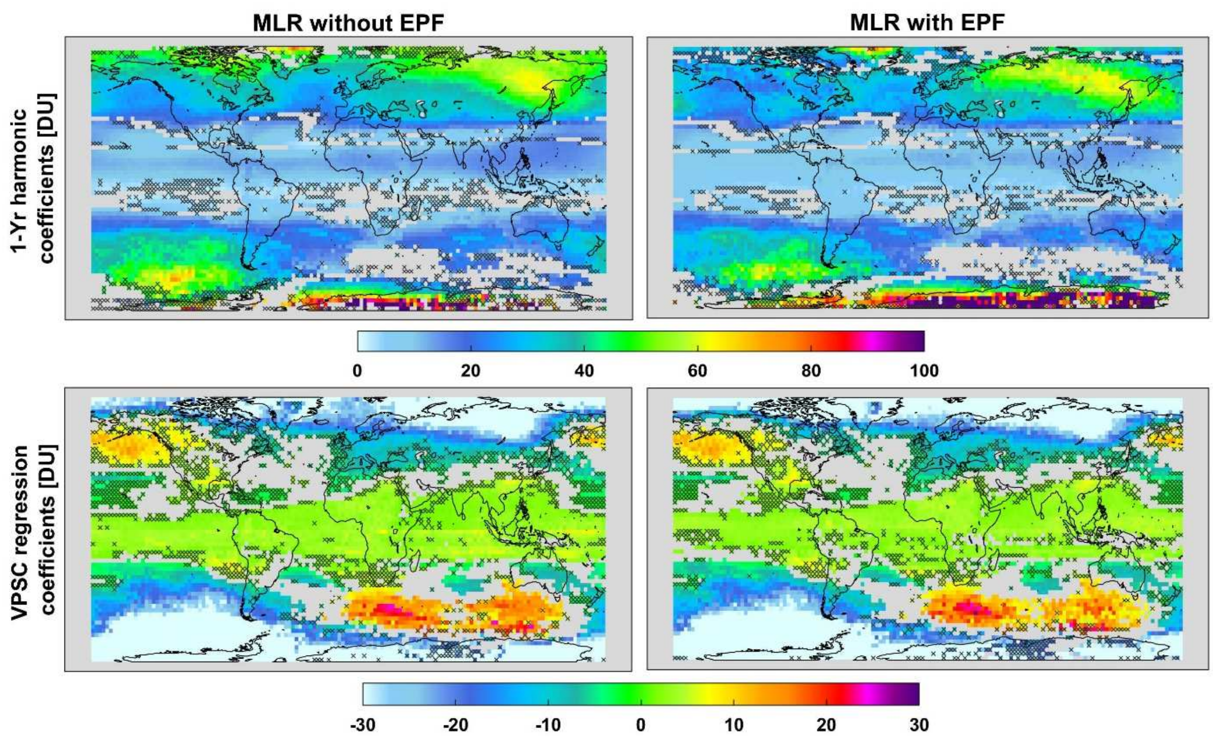

Figure 5: Global distribution of the annual regression coefficient estimates ($\sqrt{a_1^2 + b_1^2}$, in DU) for the 1-yr harmonic term (top panels) and for the VPSC proxy in LSt obtain from the annual MLR without or with EPF (left and right panels).

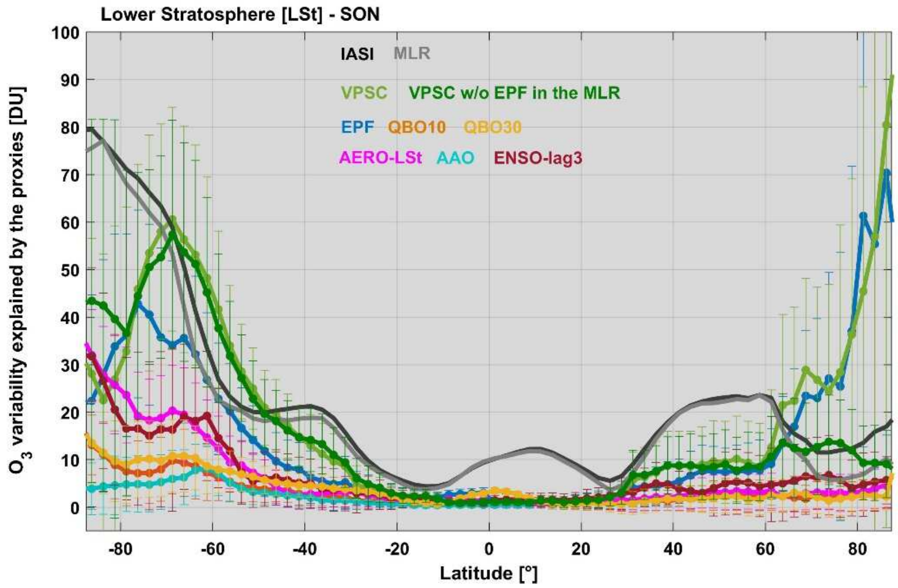

Figure 6: Same as Fig.7 of the paper for the austral spring periods (SON) in LSt, with, superimposed, the 2σ O_3 variability due to variations in VPSC from the seasonal MLR without EPF (dark green).