Response to Editor:

We thank the Editor for his reading of the manuscript and his suggestions. Our responses are given here below.

Major comment:

Line 753-765: Estimating trends for just TWO years is not a good ozone recovery analysis. This would not yield any good or meaningful results in the recovery context. Both LT and MLR are very sensitive to number of years considered for the trend analysis. I have also done some sensitivity tests on the impact of number of years on the trend detection. Therefore, I would suggest you to remove the two-year trend analyses from this work to make your other analysis (10-year IASI data) more robust and appealing. I would like to remind you that one of the referees also had the same opinion/comment on this. Thank you.

It is true that estimating O3 trends on short timescales could lead to non-significant results. However, we would like to recall that all the trend analysis done here rely on the full 10-year dataset of IASI (2008-2017) and that only the speeding up is based on shorter periods (the shortest being 3-years; over 2015-2017). Our analysis has been done carefully by removing the natural variability that has been adjusted on the 10-years of IASI; this is important in particular to avoid the effect of short trend-like segments in natural variations on the trend determination.

Furthermore, we have examined as thoroughly as possible the robustness of this identified speeding up in our replies to the reviewer's comments. This has been done by examining the uncertainty associated to the estimated trends calculated over different lengths of time (accounting for the autocorrelation in the noise residuals), by evaluating the need of additional measurements for confirming the trend and also by testing another approach (suggested by referee 2) that considers successive time segments of same length (over 6-year, 7-year and 8-years segments). These additional analyses have comforted our findings and this has been acknowledged by the referees. Note that we have specifically mentioned, at the end of Section 4.4, the need of additional measurements for verifying the trend derived over 2015-2017.

Please note also that in order to not oversell the manuscript or mislead the reader, we have decided to change the title of the manuscript to a question instead of a strong statement. Overall we believe that with these changes, which were suggested by the referees, we end with a careful and more balanced manuscript. Unless you object, we therefore would like to keep the paper as it is (the two referees also recommend to publish the paper "as is").

Minor comments:

Line 1-2: please indicate the time period in the title, "the first IASI decade (2008-2018) record"
Done as suggested: "Is the recovery of stratospheric O₃ speeding up in the Southern Hemisphere? An evaluation from the first IASI decadal record (2008-2017)"

Line 32: What is meant by "decline is not categorical in to total o3"?

By "not categorical", we mean "not unequivocal", according to the formalism of Tiao et al. (1990) and Weatherhead et al. (1998), as discussed in Section 4.

Line 32: "freezing the regression coefficient". What is this freezing?
We meant here that the regression coefficient were kept fixed, as explained in Section 4.4.

Line 36: "trends over the year"? Usually it's over a time period. Not for a year!
The term "trends over the year" means "annual trends", not "trends over one year".

Line 37: over the last years? How many years?
Over 2013-2017 as shown in Fig. 12 in Section 4.4. We believe there is no need to mention that in the abstract.

Line 39-40: it sounds that only IASI measurements are required to confirm the o3 change.
Our study presents the first detection of a speeding up of O_3 recovery process in the stratosphere over the whole year and estimates the number of additional measurements to assess a specified trend, based on the IASI dataset only.

Line 83: statistical approaches also use measurements or model results
The purpose of this sentence is to indicate that, similarly to our study, other studies already reported O_3 recovery from observational dataset using regression approach.

Line 97-101: The sentence is too hard to understand. Please split and rephrase it.
The sentence has been rephrased.

Line 103: what is high density here? Also join this small paragraph with the previous one.
This term is typically used for describing dataset with high spatio-temporal coverage and without major gap in the time series (e.g. as measured by IASI).
The paragraph has been joined to the previous one.

Line 110: Usually we use the acronym LS for the lower stratosphere, not LSt
We used that acronym to stay consistent with the previous IASI studies.

Line 119: "two years" is too short to estimate trends.
There is no two-year analysis in this paper (see the major comment). That sentence refers to the minimum numbers of years of IASI measurements that would be required to detect a specified trend.

Line 121: speeding up of o3 changes? Or speeding up of ozone recovery?
We mean the speeding up in the O_3 change rate. It might concern both the recovery and the decline.

Line 136: measurements of atmospheric composition? Or list some trace gases here.
"Measurements of atmospheric parameters" has been added.

Line 150: what is the vertical resolution of o3 profiles?

The resolution is variable along the vertical profile, but typically around ~6-8 km. As mentioned in Section 2.1, the vertical resolution allows to retrieve up to 4 independent levels of information on the vertical profile.

Line 158: Please state the accuracy of the retrievals
The accuracy is specifically discussed later in line 163-166.

Line 166: if 20% is the bias or accuracy, then how could that affect your statistical analyses presented, in terms of the ozone recovery estimation?
A so-called "bias" affects a dataset over the whole period of measurements, hence, it has no influence on the trend calculation. The distinction has to be made with the "drift" which describes a change in the bias and which has been specifically discussed in the manuscript (Section 2.1, L.168-188).

Line 177: "resulted from"
We believe "results from" is more appropriate here. The origin of the drift was not explained in the validation paper.

Line 181: full IASI period. Please state that particular period
The period is already mentioned in several places through the manuscript, e.g., title, abstract...

Line 186: is it the "trends" or any other "tendency"
By tendency, we referred to the trend. It has been changed to: "… trend observed in the IASI datasets".

Line 192: flags determined or selected
We choose to use the word "develop" because the IASI flags applied in this study have been specifically built to filter the IASI O_3 datasets (based on biased or sloped residuals, suspect averaging kernels, maximum number of iteration exceeded, fractional cloud cover …) as reported in previous IASI studies.

Line 303: it is a large range 25 to 95%. Could you please state where it reproduces the best and worst?
Done as suggested.

Line 322: delete "and discussed in this paper"
Done as suggested.

Line 368: Influence of QBO30 is greater, due to its altitude dependence?
The influence of the QBO is indeed altitude dependent. The lower influence of QBO10 vs QBO30 in the LSt would be explained by changes in the phase of the QBO10 response with destructive interference in the mid-low stratosphere as reported by Chipperfield et al. (1994) and Brunner et al. (2006) and mentioned in L.372 of the manuscript. Other couples of orthogonal functions could be used to account for both the strength and the phase of QBO (i.e. to adjust the time lag), but the QBO10 and 30 usually show the strongest anticorrelation.

Line 397: Yes, this has also been discussed in

Roscoe, H. K. and Haigh, J. D.: Influences of ozone depletion, thesolar cycle and the QBO on the Southern Annular Mode, Q. J.Roy. Met. Soc., 133, 1855–1864, 2007. and in Kuttippurath et al., 2013 (you have already cited this publication in this article).
Thank you for these additional references. They have been added.

Line 437-442: Please split this sentence, too long to comprehend
The sentence has been split.

Line 560: I do not understand this 80 DU here.
80 DU represents here the amplitude of the O_3 variability captured by IASI in the LSt during the austral spring in the highest southern latitudes, as illustrated in Fig.7a.

Line 620: 0.90; is it 90% CI level
"0.90" corresponds to the probability to detect a trend of a specified magnitude given at the 95% confidence level according to the trend detection formalism of Tiao et al. (1990) and Weatherhead et al. (1998). It does not refer to the confidence interval.
Note that the 95% confidence interval associated with the number of years required is also given in the manuscript (e.g. "detectable from ~2020-2022 ± 6-12 months"). This is now better explained in the manuscript.

Line 674: A correction: Kuttippurath and Nair (2017) discussed the vertical trends in ozone, not only total column ozone. In addition, they also discussed the ozone trends in summer months. A recent study from the same authors reported recovery signatures even at the saturation altitudes (doi:10.1038/s41612-018-0052-6).
The sentence now refers to the summer period as well and the reference Kuttippurath et al. (2018) has been added in the manuscript as suggested.

Line 805: You were discussing about 90% in previous pages. Now its 95%?
There is a difference between the 95% confidence level associated with the trend estimation and the probability of 90% to detect a specified trend (here given at that 95% confidence level).
Through the whole manuscript, as indicated in L216 in Section 2.2, the uncertainties associated to the trend values or to any other regression coefficients are given in the 95% confidence level, i.e. 2-sigma level.
Only the detection year for a specified trend is calculated within a probability of 90% according to the formalism of Tiao et al. (1990) and Weatherhead et al. (1998).

Line 808: Only 2- years are required? What is the basis for this statement?
It refers to the "additional years" of IASI measurements that are required to derive significant trends in the MUSt, as calculated from the Tiao et al. (1990) and Weatherhead et al. (1998) formalism in L. 621 (Section 4.1) of the revised manuscript.

We thank the Editor and the two referees for noting all the English style/technical/typos which have been corrected through the manuscript.