# Response to referee comments on "Continuous decline in lower stratospheric ozone offsets ozone layer recovery" by W. T. Ball et al

**General comments relevant to both referees:**

We thank both reviewers for their useful input that has led to clarifications of issues, particularly related to uncertainties, and to a streamlining and improved manuscript. Please see our comments (blue) below in response to the reviewers (black). Any major changes to the text (see below) have been put in bold font in the updated manuscript.

One point worth mentioning is that the method used to merge and account for artefacts in the composites, i.e. in Merged-SWOOSH/GOZCARDS and Merged-SBUV, was based upon an approach that was detailed in the review stage manuscript of Ball et al., 2017 (ACPD), which is now published in ACP. The final version of that paper changed some details in the merging algorithm, which also improved it. There was little affect on the overall result, and there are no changes in the conclusions, but some of the numbers/confidence levels within this manuscript currently under review have changed slightly (i.e Fig 1, 2 and 3). Most notably, the 92% probability of Merged-SWOOSH/GOZCARDS showing a decline in 'global' stratospheric ozone has increased to 95%.

Following comments from both reviewers regarding the title, it has been changed to: "Evidence for a continuous decline in lower stratospheric ozone offsetting ozone layer recovery"**.**

**Anonymous Referee #1**
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The manuscript "Continuous decline in lower stratospheric ozone offsets ozone layer recovery" by Ball et al. describes analyses of vertically resolved stratospheric ozone data sets of different origin with regard to detection of ozone recovery. For this, a relatively new method in ozone analyses, Dynamical Linear Modelling, is used. Obtained results indicate an increase in upper stratospheric ozone, especially in the midlatitudes, and a decrease in lower stratospheric layers, especially in the mid-latitudes and tropics. The stratospheric profiles of the different data sets are then integrated to partial columns to analyze the different trend behavior in more detail. Results are compared to tropospheric ozone time series and results of two chemistry-climate model simulations (calculated in specified dynamics mode) to better understand the lower stratospheric ozone trends in particular. The structure of the manuscript is clear, and it is very well written. The applied methods are described mostly in sufficient detail to allow the reader to understand what was done. It is also stated with plenty of references from the recent literature where this study compares to previous findings, and where new results are presented. There are a few minor things that I would like the authors to address (mainly clarifications, shortening/expansion of explanations, etc.) before I would recommend the manuscript for publication. General suggestions/comments:

- There are several acronyms that are specified multiple times throughout the manuscript (StCO, PCO, TrCO. . .). In most cases this is not necessary, but only slightly annoying for the reader. I would suggest either using the full name throughout the manuscript (if the authors think that the reader might not remember the acronym), or defining them once and using them from thereon.

All abbreviations of the TCO, PCO, StCO, and TrCO variety have been written out in their full form.

- In some cases throughout the manuscript the authors could be slightly more specific when describing something, e.g.: page 2, line 34 'Models predict. . .' -> what kind of models? - page 3, line 37 'Only recently has a TCO recovery been detected during the austral spring. . .' -> the recovery was detected in Antarctica, which is not necessarily deductible from the description; page 4, line 106 'Our aim here is to quantify the absolute changes in ozone. . .' -> which ozone is referred to here? Stratospheric or tropospheric, global or specific latitude bands? I would suggest that the authors check the manuscript carefully to make sure all descriptions are detailed enough so that it is clear what is described.

We have looked through the manuscript and changed as we saw ambiguous. Specific to the referee's suggestions, we have changed: 'models' to '**Chemistry climate** models **(CCMs)**...'; 'been detected during austral spring' → 'been detected over **Antarctica** during austral spring'; 'Our aim here ... ozone...' → '**Here, we quantify the absolute changes in ozone in different regions of the stratosphere, and troposphere, and their contributions to total column ozone, at different latitudes and globally, since 1998**...'

- Some lines in the contour plots (e.g. Figure 1, Figure 5, Figure A1. . .) are hard to see if the contour colors are very dark. If that is the case, maybe the contours for the probability changes (that are black now) could be white instead? That might help them having better visibility.

Agreed. White also has a similar (but opposite) effect at the interface between positive and negative, so after many tests, we settled on a darkish grey.

Specific comments:

Page 1, title: I think the title is not precise enough. I would suggest changing 'ozone layer recovery' to 'total column ozone recovery' (or something along those lines). As far as I understood, that was the focus of the study.

The referee makes a good point and we have considered this carefully. However, this is a little tricky, since the total column ozone recovery is not just the stratosphere. It appears that a significant portion 'may' be due to tropospheric increases, and then the recovery should not be attributed to the total column since it really refers to stratospheric ozone. Since it appears to be the case (whichever of the datasets analysed) that the lower stratosphere is decreasing in such a way that it compensates the ozone layer recovery and the total column ozone increase, we suggest the title represents the more confident result of the stratosphere itself.

Page 4, line 95: after the parenthesis, 'km' is too much

Done.

Page 6, section 2.3: This section is too brief in my opinion. It is not clear how exactly the DLM works, and how the probabilities are calculated. I don't think the explanations have to go into too much detail, but some more explanations would be great.

The DLM approach is explicitly detailed in Laine et al., 2014, and is too detailed to be expanded upon here. Nevertheless, we have added the following to section 2.1:
"We infer posterior distributions on the non-linear trends by Markov Chain Monte Carlo (MCMC) sampling; **the background trend levels at every month are included as free parameters, with a data-driven prior on the smoothness of the month-to-month trend variability. DLM analyses have more principled uncertainties than MLR since they are based on a more flexible model, and formally integrate over uncertainties in the regression coefficients, (non-stationary) seasonal cycle, autoregressive coefficients and parameters characterizing the degree of non-linearity in the trend.** The time-varying, background changes are inferred, rather than specified by [...]"

Section 2.3 has been restructured, we have added the following text to elaborate on how probabilities were estimated:
**"The posterior distributions that represent the change since January 1998 are formed from the (n=100,000) DLM samples from the MCMC exploration of the model parameters (see section 2.1). Then, probability density functions (PDFs) are estimated as histograms of the sampled DLM changes from 1998. Finally, the probabilities represent the percentage of the posterior samples that are negative; therefore, the posteriors and probabilities presented in all figures represent the full information inferred about the change in ozone since 1998 obtained from the DLM analysis; these are not always normally distributed."**

Page 7, line 188: '. . .developed by (Ball et al., 2017), . . .' -> parenthesis are placed wrong

Fixed.

Page 17, line 418-441: The comparisons between the CCMVal-2 results are too lengthy and in some aspects unnecessary. I think these paragraphs could be shortened quite a bit.

Following this, and the second reviewer's suggestion, these paragraphs have been significantly shortened and merged. It now reads:
**"The CCMVal-2 multi-model-mean 2000-2013 ozone changes in the WMO 2014 ozone assessment (Fig. 2-10) show a positive, but insignificant, change in the lower stratosphere at mid-latitudes, which suggests models may not be simulating that region correctly, consistent with the two models extended to 2016 here. While CCMs capture historical ozone behaviour in the upper stratosphere well, it is less clear in the UTLS region. Figs. 7.27-7.28 of the SPARC (2010) report indicate large differences compared to observations in winter/spring, perhaps related to factors affecting model transport (e.g. resolution, and gravity wave parameterizations). Whether these differences result from model design, incorrect boundary conditions (e.g. underestimated anthropogenic (Yu et al., 2017) or volcanic (Bandoro et al., 2017) aerosol contributions), or missing chemistry remains an open question."**

Page 18, Section 5: The conclusion section starts slightly abrupt in my opinion. It would be good to start with some perspective again: where do the findings fit in the bigger picture? What exactly did the authors want to present? Starting with this, it would be easier for the reader to follow the summary of the results that are given with the Roman numberings.

We have added the following at the beginning of the conclusions section: "**Following the successful implementation of the Montreal Protocol (MP), total column ozone stabilised at the end of the 1990s, and searches for the first signs of recovery in total column ozone have been underway since then (Weber et al., 2017; Chipperfield et al, 2017). We find that counteracting trends within different atmospheric layers are the reason a significant detection has remained elusive.** In summary..."

Page 18, line 467-468: parenthesis for the references Plummer et al. (2010) and Dietmüller et al. (2014) seem wrong

Fixed.

Page 19, last paragraph: The list of positive effects of the lower stratospheric ozone decrease (decreasing exchange with troposphere, radiative forcing offset, etc.) comes across a little too strong compared to the reasoning why the decline could be bad for life on Earth. The authors might want to think about rewording some of it to strengthen the point why the decline in stratospheric ozone might indeed be not so good.

We have reduced the strength of the positive benefits and shortened the paragraph overall, which heightens the more negative consequences of a decreasing ozone layer.

Page 19, line 485: 'trends' should be 'trend'? After all, it is only the lower stratospheric ozone that shows that decline

Agreed, and updated.

References:

Chehade, W., Weber, M., and Burrows, J. P.: Total ozone trends and variability during 1979-2012 from merged data sets of various satellites, Atmospheric Chemistry & Physics, 14, 7059–7074, doi:10.5194/acp-14-7059-2014, 2014.
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Weber, M., Coldewey-Egbers, M., Fioletov, V. E., Frith, S. M., Wild, J. D., Burrows, J. P., Long, C. S., and Loyola, D.: Total ozone trends from 1979 to 2016 derived from five merged observational datasets – the emergence into ozone recovery, Atmos. Chem. Phys. Discuss., 2017.