

***Interactive comment on* “Stratospheric ozone trends for 1985–2018: sensitivity to recent large variability” by William T. Ball et al.**

Anonymous Referee #3

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Ball et al. provide an update on Ball et al., further examining trends in stratospheric ozone derived from satellite observations. Additionally, they discuss the recent results of Chipperfield et al. (2018), who modelled large increases in lower stratospheric ozone in a CTM, indicating that the observations used in this study give a smaller increase and suggest that this is consistent with interannual variability driven by the QBO. Further, they suggest that, if this is the case, then the lower stratospheric ozone increases will decrease in the near future. Despite these increases, longterm trends in lower stratospheric ozone trends remain negative. The analysis and discussion presented in the paper is of a high standard and explores an important and relevant topic within the scope of SCP, and as such merits publication following revision. I have several comments the authors should address before publication:

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General Comments:

1. As a major point of consideration, the authors frequently use the term recovery, or lack of, when discussing ozone increases and decreases. However, recovery of stratospheric ozone is really reserved for increases of ozone resulting from reductions in stratospheric Cly expected due to the Montreal Protocol. Changes resulting from dynamical variability or stratospheric cooling resulting from CO₂ increases are not strictly recovery. As the manuscript does not explore all the drivers of ozone changes at different latitudes/altitudes, I feel the authors should refer only to ozone increases/decreases where they are not attributing ozone changes to change in ODS.

2. A second major comment is that the authors should state specifically what they are discussing in each section. Too often terms like decline/recovery/trend are used with no reference to ozone. This is particularly true in the sentence on L367-369, which states 'The quasi-global lower stratosphere continues to exhibit a monotonic decline that is still highly confident with 99% probability (Fig. 7 and Table 1), and the whole stratosphere continues to remain lower than in 1998...'. Further, more clarity should be provided so the reader knows when the authors are discussing ozone trends and ozone values.

3. There is no discussion in the manuscript on the chemical lifetime of ozone – the reason that dynamical variability plays such a key role in the lower stratosphere is that here the chemical lifetime is long. A brief description of this fact, with references, would add to the introduction.

4. The authors frequently refer to 'climate change' as a potential driver of the examined dynamical variability. However, I feel they would be better served by using a phase such as 'changes to stratospheric dynamics resulting from anthropogenic greenhouse gases,' which is closer to a mechanistic analysis. Climate change is itself a response to changing GHGs.

5. I miss any discussion on why the CTM results of Chipperfield et al. and the ob-

servations presented in this study differ so greatly. Is there any consensus on why this is? Does it represent some failing of the chemistry in the CTM? Is the re-analysis dataset used not accurate? IS it within the uncertainties of the observations? Further discussion on this point would improve the manuscript.

Specific comments:

L1: Add 'and its subsequent amendments' following Montreal Protocol

L6: Replace 'wiped out'

L18: Important here to say that dynamical variability is counteracting the effects of the Montreal Protocol on stratospheric ozone recovery, not on the regulation of halogenated ODS.

L19: Swap '(30-60)' with 'variations'

L33: Add 'statistically' before significant to indicate that you do not mean large increase

L33-43: The authors could cite here, or elsewhere in the manuscript, that these observations are supported by CCM studies (e.g. Meul et al., 2016; Keeble et al., 2017; Dhomse et al., 2018).

L44-49: Stress that these composites are observations

L45: Use consistent spelling of timeseries throughout the manuscript

L60: Add 'ozone' between 'negative trends'

L93: Remove 'see'

L94: IS the Froidevaux et al. paper cited her now published? If not I would recommend removing it from the manuscript, instead saying that the v2.20 dataset used here is an update of Froidevaux et al. (2015).

L125: delete 'point the reader to Laine et al. (2014) for details on this method and' – the paper is already cited in the sentence.

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L135-137: Why do the authors allow the seasonal component to vary but keep the regressors constant? Is it more likely that one should vary in time than the others? Some additional text explaining the rationale behind this decision is warranted.

L137-140: This is not true if EESC is used to represent the longterm trend

L128-155: Are there any references in the literature to support the claims made in this section? There is a lot of literature on using MLR techniques when determining ozone trends, and any literature which assesses the DLM technique should also be cited.

L151: What is MCMC – it does not appear to be defined in the manuscript.

L197-202: The authors should be clear here that they are discussing ozone trends

L204: Add 'in lower stratospheric ozone' between increase and reported

L225: Replace 'upswing' – also elsewhere in the manuscript.

L240-241: specifically in the lower stratosphere, where the lifetime of ozone is long – please clarify that in this sentence.

L250: The authors could add here further discussion, with appropriate references, discussing other drivers of variability.

L273-287: This paragraph is very confusing and should be re-written to aid the readers understanding of the QBOs influence on the transport of ozone, and how this differs at different latitudes. This is a key point of the paper, and so spending some time clarifying this section will significantly improve the manuscript.

Figure 4: Why does the red dashed line in the lower panels not have the same value where it intercepts the left and right hand side axes? The figure caption says the red dashed line is for January to October 2017, so the July value should be the same on both, as both are July 2017.

L311: Remove 'other'

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L325-334: The sensitivity of total column ozone trends to the choice of end year is also discussed by Weber et al. (2018) and Keeble et al. (2018), both of which could be cited here. Weber et al. has also now been published and the citation in the reference list and throughout the manuscript should be updated to reflect this.

L373-284: The authors should clarify that here they are discussing ozone trends and not ozone values, which should not be affected by the addition of subsequent years.

L387: change 'tropical' to 'tropics'

L401-403: Care must be taken here not to equate the success of the Montreal Protocol with increases in ozone. The Montreal Protocol is working – look at decreases to anthropogenic Cl – but it is not possible to say, without exploring the drivers of the changes, that increasing ozone in the upper stratosphere is evidence of that success. See also major comment above for use of ozone recovery and ozone increases.

L412-414: Consider rewording this sentence for clarity – as it is currently written, it is very hard to follow what is meant.

L423-425: Clarify here that this is a result from a modelling study, not an extension of the observed record of ozone changes.

L454: Replace 'spread on' with 'spread in'

L464: Replace 'predictions' with 'projections'

L465-484: There is an important feedback here, as ozone changes also affect the dynamics. Several CCM studies have highlighted the impacts of ozone depletion on stratospheric circulation, and as ozone starts to recover due to reductions of ODS, stratospheric dynamics will respond.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-243>, 2019.

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