We thank the reviewer for their time and comments. Please find our responses to each comment below - original reviewer comments in bold, author responses beneath.

**Anonymous Referee #3**

**The paper reports on the impact of recent emissions of CFC-11 on the ozone layer, first documented in Montzka et al., and most recently, in Rigby et al. 2019. The paper explores a number of possible scenarios of CFC-11 emissions, and concludes that if emissions are allowed to continue into the future, significant delays in the recovery of the ozone layer are expected (by a decade or more in the global TCO, and even more in the Antarctic stratosphere). The subject of the paper is of high relevance and interest for the readership at ACP, and for the wider community. The paper is well written and the conclusions of the paper are supported by the data. There are nonetheless a few issues in the paper, which need to be addressed in order to make the paper suitable for publication. One of them is the missing discussion of another paper, which recently came out on the same subject (and in this same journal), namely Dameris et al., ACP 2019 (https://doi.org/10.5194/acp-19-13759-2019). I believe this paper is not discussed nor mentioned in the text because it came out quite recently, so the authors did not see it. Dameris et al., similar to this study, investigated the impact of two CFC-11 emission scenarios on ozone projections, and found that a substantial delay of ozone recovery by up to 20 years could occur due to unabated CFC11 emissions. Are the results in this paper consistent with their conclusions (bearing in mind their differences in the CFC11 scenarios)? There are also some issues in the missing discussion & quantification of the chemical mechanism leading to the delayed ozone recovery. Dameris et al. 2019 found that actually, some catalytic cycles are slowed down (e.g. Ox and NOx), leading to ozone increases under increased CFC11 emissions (which compensate the ozone depletion from enhanced ClOx cycles). For example, how important are ClOx cycles, relative to the other depleting cycles? In addition, the authors tend to only cite their own past papers, neglecting the much more vast body of literature on e.g., BDC and its impacts on ozone, and multi-model comparisons. Please cite the first papers that studies these problems, and not only your own ones; see specific comments below. The paper would benefit from addressing these issues.**

We thank the reviewer for their time and comments. Please find our detailed responses below. The referee rightly indicates that more comprehensive referencing is requited in some areas – we have been happy to do this.

**SPECIFIC COMMENTS**

**- Can the present results be compared and discussed in the context of the recent paper by Dameris et al., 2019?**

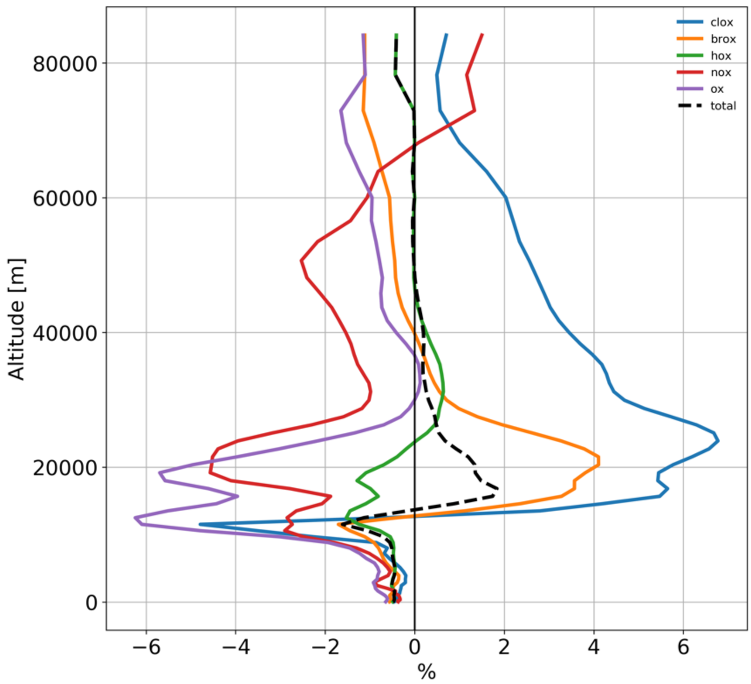
The Dameris et al. results have now been discussed in relation to the results presented in this manuscript in the introduction and discussion sections.

**- Can the authors please try to pick a more descriptive experiment tag? SCENx-y is quite cumbersome and does not convey the basic information of the experiments setup. For example, how about "SCEN35" and "SCEN90" so that the reader immediately knows the total emission in terms of mass of CFC11... or something similar? The authors need**

**to constantly remind the reader what each tag means, when they wish to emphasize some key result. This could be avoided by picking a better experiment tag.**

We feel that the scenario names chosen best reflect the emissions scenarios being considered as they include both the scenario type (1 = direct emission, 2 = formation of a bank, 3 = co-production of CFC-12) and also the duration of the assumed CFC-11 production, in years, in as succinct a manner as possible. It is not possible to use only the total emission of CFC-11 to name the scenarios as the emissions change through time for the scenarios which assume a bank is produced, and nor would it reflect the duration of the production.

**- Could the authors look at the ClOx and other catalytic cycles, so that the reader can get a glimpse into the key chemical mechanism, and its dependence on height (without just simply assuming that the Molina and Rowland (1974) cycle is enhanced...? While I don't expect the authors to perform a full ozone budget analysis for each of the ensembles, showing the relative change in ozone production rates in a few key experiments would improve the paper.**




We agree with the reviewer that it is unlikely that only the ClOx catalysed ozone depleting cycles will be affected by the changes to the CFC-11 and CFC-12 lower boundary conditions. Above is a plot of the relative difference in global mean ozone depletion through the ClOx, BrOx, NOx, HOx and Ox catalytic cycles between the SCEN2_30 simulation and BASE (averaged from 2030-2040). As found in the Dameris study, increases to ozone destruction through the ClOx catalytic cycles are somewhat offset by decreases to ozone loss through other catalytic cycle. However, interpretation of the drivers of these changes are complicated. This is partly through the chemical coupling of the ClOx cycles with the NOx, HOx and BrOx cycles, and partly through changes to the Brewer-Dobson circulation affecting the oxidation of source gases. Diagnosing the changes to these cycles is further complicated by the need to account for the changes to O3 concentrations. Increases to ClO will deplete stratospheric ozone, and so we may expect, for example, NOx catalysed ozone depletion to slow as 1) there is extra formation of the ClONO2 reservoir, and 2) there is a lower concentration of O3.

Because of these reasons, we feel that addressing the question of which catalytic cycles are responsible for the TCO changes examined in the paper is beyond the scope of this study. It should be stated that we have not assumed in the text that only the Molina and Rowland cycles are affected but have added text to the discussion to include the points above and cited the Dameris et al. ozone budget results in the discussion section of the manuscript.

**MINOR COMMENTS**

**L20 page 2: Ball et al., ACP 2018 (https://doi.org/10.5194/acp-18-1379-2018) is another key paper that should be cited here.**

This citation has been added to the discussion.

**L5 page 5: why is the lifetime fixed? Shouldn't it be a strong function of altitude, and depend on the photolysis rate (which in turn is a function of the actinic flux)?**

While the local lifetime of CFC-11 is dependent on its destruction through photolysis, the 55 year lifetime is used to convert the emissions fluxes into a mixing ratio at the surface of the model, and so represents its bulk atmospheric lifetime (i.e. the global burden/global loss). Imagining a pulse emission of CFC-11, some is transported into the middle and upper stratosphere where it is rapidly destroyed, but the majority (by mass) remains in the troposphere and lower stratosphere, where its lifetime is much longer. A lifetime of 55 years accurately projects the decline in the mixing ratio observed at the surface, and hence its use in this study. The value of 55 years for the lifetime comes from Chipperfield et al., 2014 (referenced in the manuscript).

**L12 page 7: again, Salby et al., 2009, and more recently Ball et al. 2018, also discussed the impact of variability on the detection of a recovery. So, these papers should be cited, too.**

We have added the Ball citation to the manuscript. We could not find a Salby et al., 2009 publication, but have cited instead the Salby et al., 2011 paper which discusses the impact of variability on the detection of Antarctic ozone recovery (Rebound of Antarctic ozone, GRL).

**L15 page 7: how about the technique used in Eyring et al. 2013 and in all the WMO reports (the TSAM method)? This method seems more customary and it should be at least mentioned what the differences between this paper's method and that one is.**

This method has been added to the manuscript, along with the Scinocca et al. (2010) reference. The sentence now reads:

"To mitigate these impacts, the effects of natural processes (such as volcanic eruptions, the QBO, ENSO and solar cycle) can be removed from the data using statistical techniques (such as Multiple Linear Regression, e.g. Staehelin et al., 2001; WMO, 2007 or the Time series Additive Model, Scinocca et al., 2010), or the data can be smoothed by averaging across multiple years (e.g. Dhomse et al., 2018)."

**L31 page 8: this is probably subject to a lot of variability and running multiple ensembles may wash out this little increase TCO values. Or perhaps, there may be some compensating effect from NOx and Ox cycles. It would be good if the authors could look at the production rates (see specific comment above).**

As the reviewer states, these differences likely result from variability between the integrations and would likely be removed by the use of more ensemble members. However, it was decided it would be better to explore a larger spread in possible future CFC-11 production scenarios. Please see our comment above about analysing the ozone budget terms in these simulations.

**L6 page 9: the imapct of BDC accelleration on TCO has been studied in more studies than just these ones. Most recently, this has been shown in Chiodo et al., J.Clim. 2018 (DOI:10.1175/JCLI-D-17-0492.1). So, at the very least, add this paper to the references here.**

Additional references have been added to this discussion. The sentence now reads:
"The observed ozone loss in the tropics has been small and, furthermore, future changes in the tropics are driven both by reductions in the stratospheric abundance of halogens, which tend to increase ozone, and the strengthening of the Brewer-Dobson circulation, which tends to decrease column ozone (e.g. Oman et al., 2010; Eyring et al., 2013; Meul et al., 2014; Keeble et al., 2017; Chiodo et al., 2018)."

**L13 page 9; this has been first studied in Oman et al., 2009 (https://doi.org/10.1029/2010JD014362), so this paper should be cited, too.**

This reference has been added to the manuscript.

**L4-16 page 10: I don't get what the scenario-independent aspect would be, in this context. Cly clearly depends on the CFC emission scenario so there is no scenario independency here.**

By 'scenario independent' we mean one could use the linear relationships identified between the amount of chlorine emitted and the timing of TCO return to estimate the delay in TCO return date for any particular chlorine emission. The benefit of this extends to emissions not explored in this study (i.e. not one of the 10 scenarios investigated). As the reviewer states, the $Cl_y$ projection depends on the assumed CFC emission scenario, but it is not known what possible future emission scenario will be followed. Rather than argue that one of the scenarios explored in this study is more likely than the others, we have looked for ways to say 'if the emission of CFC-11 was x, then the delay would be y', which we believe would represent a positive benefit to policy and regulation.

**- The paper by Stolarski et al. 2012 (https://doi.org/10.1029/2012JD017456) shows that the sensitivity of ozone to temperature decreases as chlorine increases in the stratosphere. Hence, it should increase at a slower rate compared to the reference scenario, as chlorine decreases at a slower rate. This may impact the response of ozone to the upper stratospheric cooling, as a function of much Cly we have in the stratosphere. Could this explain some of the non-linearities found by the authors?**

The reviewer is correct to highlight this process as a potential cause for the non-linearities seen here. An additional factor is coupling between the chemistry and dynamics. However, despite these non-linear couplings, and the interannual variability inherent in CCMs, the linear relationships identified in this study between the amount of chlorine emitted and the delay in ozone recovery are robust, with high r2 values, suggesting that these factors a minor in the face of the dominant effect of changing stratospheric Cl and depletion of ozone.

**L8 page 12: other papers have reported this in the past, a lot more than just Keeeble et al., 2017, so they should be cited. At the very least, Butchart et al., 2014 should be cited here.**

The Butchart et al. and Meul et al., 2016 references have been added to the discussion.

**L20 page 13: where should this non-linearity come from (i.e. the same CFC11 change in 2080 having a different effect than in 2020) ? The Cl + O3 reaction rate changing because of O3 background concentrations? (the Cl change would be the same!)**

Please see our response to the same question from referee #2