Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-747-RC2, 2020

Atmospheric Chemistry and Physics Discussions
Open Access

Interactive comment on "Modelling the potential impacts of the recent, unexpected increase in CFC-11 emissions on total column ozone recovery" *by* James Keeble et al.

Anonymous Referee #3

Received and published: 18 January 2020

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.

SPECIFIC COMMENTS

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

- 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.

- 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.

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.

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)?

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.

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.

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).

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.

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.

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.

- 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?

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.

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!)

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-747, 2019.