Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-87-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimates of Ozone Return Dates from Chemistry-Climate Model Initiative Simulations" *by* Sandip Dhomse et al.

Anonymous Referee #2

Received and published: 13 April 2018

This paper details ozone return dates from the CCMI model intercomparison. As such, the new estimates of the return dates are the primary new finding of the paper and most of the other results discussed are previously known results. The paper is clearly written and outlining the latest results on ozone return dates is an important task for the upcoming ozone assessment. I have a number of comments but most are relatively minor and thus I recommend publishing the paper after the comments are adequately addressed.

As stated above most of the results outside of the specific details that are derived from the models simulations were already known. It would be good if you could highlight the new results from this paper in the abstract and conclusion and state more clearly the results that support earlier work.

Printer-friendly version

Discussion paper

It is important to state more prominently that since the models all use mixing ratio boundary conditions an important source of uncertainly has not been included. Thus, the uncertainty given are likely underestimated.

The resolution of the figures is poor, making them hard to read. This should be fixed in the final versions.

Page 2, lines 16-18: Are your later return dates within the uncertainly estimates given in the 2014 Ozone Assessment? This would be interesting to add.

Page 2, line 38: Change “also changes ozone return” to “also increases the ozone return” so the reader knows the direction of the change.

Page 3, line 5: I think you mean tropospheric chlorine and bromine peaked in 1993 and 1997, although I’ll leave it to you to double check.

Page 3, line 16: The statement “therefore requires” is unjustified from what comes before. I’m not saying it is does not “require” 3D models, but just that you have not presented evidence that would support the claim.

Page 3, lines 47 to end of paragraph: The “faster removal of ODSs” is largely uncaptured by the models here since they have fixed mixing ratio BCs. I say “largely uncaptured” since while the ODS loss term in the stratosphere is affected, it does not affect the surface mixing ratio as it should. This needs to be made clearer since this discussion is likely to mislead the uninformed reader.

Related to this effect it would be useful to have a figure of the CFC-11 lifetime (or some other long lived tracer) as a function of time from all the models.

Page 7, line 17: Using the period 1980-1984 is a bit unfortunate since there is significant ozone loss during this period, especially in the Antarctic region. Depending on how you have done the calculation this will bias your return dates to earlier values. Since you have model data starting much earlier I would suggest using 1978-1982 instead, or at least discussing the sensitivity of your results are to this choice. I suspect

you made this choice due the availability of SBUV data but it should be possible to derive an adjustment for the data for your figures.

Page 7, line 28: I don't see any shading. Is this in reference to figure 1?

Page 9, lines 10-35: It is curious that nearly all of the model simulations are below the data. Any idea why? Seems worthy of mention and speculation.

Page 10, lines 1-9: The variation shown on figure 7 at 5 hPa is worrisome, a fact that should be highlighted in the paper. At 5 hPa most of the organic chlorine should be liberated (as can be seen by the similarity of the left and right panels) and thus both the plots should be close to the surface values with a 2-4 year lag to account for the age of air. The peak should be close to the peak in total chlorine in the surface concentrations and the values during the falloff should be very close to the surface values (since a 2-4 year shift is a small change). Thus, the models that are outliers on this plot are evidently not conserving chlorine and their results throughout the paper should be in question. This needs to be stated.

Page 10, line 40: Title should probably be "Sensitivity of ozone return to GHG concentrations and climate change"

Page 10, line 47: The tropospheric impact of CH4 has been pointed out in many papers before Morgenstern et al. 2018, so why chose this reference.

Page 11, line 4-6: Actually, the effect of GHG is as comparable in the Antarctic to the other regions. It is just harder to see because the scale of the chlorine depletion is so much larger. From you graph, I estimate a 20, 30, 5, and 8 DU change between RCP45 and RCP85 scenarios at 2100 in the 1st four panels. Thus, the Antarctic appears to be second largest instead of "small". This makes sense since the effect of the GHG is primarily above the ozone hole and thus should be similar. The main complication to this is the increased importance of Cl+CH4 in the ozone hole.

Page 11, lines 14-17: You state what is on figure 12 but say nothing of what it tells the

Printer-friendly version

Discussion paper

reader. Either discuss or remove.

Page 12, line 3: Change “chemically inert” to “chemically inert in the troposphere and stratosphere” since CO2 is broken down in the mesosphere and above.

Page 12, line 10-11: Change “most important” to “most important for dynamical changes” or something similar.

Page 12, line 48-49: As above I disagree with this statement. It seems comparable to me looking at your plots if one adjusts to the greatly different scales. If you plotted the difference between the scenarios it would be clear.

Page 13, lines 39-44: The fact that the global value for the return date is seemingly inconsistent with the different latitude regions implies that it is poorly constrained and you can conclude little to nothing about the effect of N2O changes.

Page 14, line 39: Again, it is not the weakest but only less evident.

Page 15, line 7-9: You need to point out there are still serious issues with the chemical and/or transport schemes in some models.

Page 15, line 19-41: The points made in these final two paragraphs are important and should be made in the abstract as well.

Page 28: You should mention the shaded regions in the caption.

Page 30: Add the abbreviations SCO and TCO to the caption in the correct places to help the reader who doesn’t read the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-87, 2018.