

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

Excellent, well written, paper discussing an important issue, namely the timing of the return of the stratospheric ozone hole to levels prior to human influence. The major contribution of this paper is to then examine how this recovery could be affected by further emissions associated with rogue use of chemicals like CFC-11 and the potential effect of short-lived gases not considered in the Montreal Protocol. This paper definitely should be published in Nature. However, I do think that the paper should add further discussion on uncertainties in their analyses, especially on the potential effects of climate change on their analyses.

Reviewer #2 (Remarks to the Author):

Review of the paper by Dhomse

This is a very interesting paper that should be published, but the paper would benefit from additional calculations to illustrate specific uncertainties and from substantial editing and modification.

My comments are as follows. Overarching comments are:

1) The paper presents results from a series of calculations. It was extremely difficult for me, even as someone who has followed the issues quite closely, to follow all the cases because there is nothing in the main text to succinctly summarize them; too much is relegated to the methods section and it isn't clear there either. The paper badly needs a descriptive table, to be included in the main body of the paper, presenting a compact description of the various cases to which the reader can refer.

2) There are many assumptions involved in the various cases. Many of them are presented as if they were subject to no uncertainties at all, when in fact they are highly uncertain. The paper's key figures and text would greatly benefit from giving ranges rather than separate lines or numbers reflecting the uncertainties in key factors. These include for example how much of the current mystery emission is extra production versus continuing emission from uncertain banks, how much of the implied extra production goes into foams rather than being released directly (again, not just a single number 1/7), etc. If the figures showed shaded lines instead of dotted and solid lines that could be one way to approach these in a clearer manner.

3) The paper is somewhat scattered in the way it goes from excess CFC-11 emission to uncontrolled VLSL. It may be better to put the latter in a later paper. If it stays here, more should be done to clarify what is new compared to their own group's earlier published work on the subject (by Hossaini et al.), and there may not be room to do so, particularly after other comments are addressed.

Specific comments are:

4) The paper contains some loose language such as that on line 21 'will largely cease to exist' (what does that mean?); in some cases accompanied by very specific numbers (2063-2068, same line). Please rework for clarity.

5) There are a number of unreferenced pieces of information. For example, I don't disagree that CFC-11 does represent about a quarter of the chlorine reaching the stratosphere (line 67) but where is the reference for this statement?

6) I understand lines 68-77 but I don't think the non-specialist reader will find the order of presentation clear. Move up lines 74-77 to the front of this part and then the rest can be made much clearer.

7) Line 91. What's new and different here?

8) line 111. Could it be delayed by more, or less, depending on the assumptions you have made? Stating this as if it were a single number when there are numerous assumption underlying your cases (see comment 2) seems misleading. Also on line 112, same point. Is this for the WMO scenario assuming all of the current emission is continued? That is somewhat extreme, isn't it?

9) Line 119-121. What's the point here? That the CTM produces a good lifetime so you get agreement? I don't know why this is relevant.

10) Line 126. There is uncertainty in the 2002 bank, and there is uncertainty in the release fraction from the banks, please include estimates of those.

11) Line 133-134. You've spanned some of them, but there are more. Please rephrase this to avoid giving the impression you've done a comprehensive study of uncertainty, unless your revised version does so.

12) Line 158. Some studies suggest significant depletion even before 1980. Do you mean first satellite data points? Then it could be given as 1979?

13) Line 172. This isn't a new point in the literature. Please refer to earlier papers.

14) Line 199. The comment about the free running models could be hard to understand for the non-specialist, and requires a reference as well. I think it's better deleted here.

15) Line 217-219. These are interesting results. It would be clearer as 'Whenever years with extremely cold stratospheric conditions occur in this century, the Arctic would be...'

16) Line 224-225. I think you mean 'and highlights the importance of how rapidly they were detected by atmospheric monitoring to safeguard the ozone layer'

16) Lines 225-227. Sentence is garbled.

17) Line 231. 7x is subject to large uncertainty as well as being unreferenced. Needs a range instead here.

18) Line 236. To say you have modeled the banks is a bit expansive. Better to say 'includes an estimate of the banks'. Hopefully one that includes uncertainties in some way in the next draft!

19) Line 239. The 2-D models are 2-D, but I don't think it's fair to say they have less detailed treatments of the chemistry. Rephrase.

20) Line 338-339. Confusing. Which case is this?

21) The figures are hard to decipher at present, in part because of the way different cases are labeled and in part because of the lack of a summary table describing the cases. For the key figure 1, I think it would be better to have three panels, one for each of the three scenarios. Even though they would be smaller they would be clearer. And they should have shading to show uncertainties, as discussed above. Could be split into two figures to avoid being too busy, emissions and CFC-11 mixing ratios, if one of the other figures is deleted (see below).

22) Figure 4 seems non-essential here; it could be moved to a supplement.

23) Figure 6 is interesting but a technical point that also could go to the supplement.

Reviewer #3 (Remarks to the Author):

Overall, this is a very well written paper which is rather easy to follow for a non-expert as I am. To my knowledge this is a solid piece of work, performed by a respected group.

My main points are:

Reading the paper I am wondering about the potential emissions of the unreported production and especially in relation to the bank that might have been formed. Assuming a 10 yr period (2010-2019) of unreported production leading to an average emission of 20Gg and 6/7 going in to the bank the bank would have grown by 120Gg per year to 1200. Am I correct that the loss rate of this bank has not been accounted for and may delay the recovery date further? Wouldn't this bank alone not already double the emissions for the coming decades (when I compare to 1480)?

Discussion: The sensitivity to the meteorological year is clear and will cause years with less and more strong ozone holes in the future with declining amplitude towards the recovery year. Given an unchanging climate this would give a random variability. The choice of indicator gives different horizons for the estimated recovery year but the systematic changes around the central estimates seem stable. Hence, the emission changes give rise to systematic changes in recovery estimate. Could you place these spreads in to perspective in comparison to the potential systematic impact of a changing climate? Would climate change and associated dynamics be significant compared to the unexpected emission impact? This issue could be handled in a few lines in the discussion. Now the upper stratosphere impact is shortly covered in the methods section.

Smaller remarks:

Line 19: changing chlorine emissions is obvious, so possible can be removed. Or do you mean not anticipated emissions deviating from the expected path?

Line 19/20: The term "chlorine source gas emissions" sounds a bit odd, would "active chlorine precursor emissions" or something alike be preferable?

Line 23: as 2021-2041 : doesn't this mean during any year from now on?

Line 56 Thus the averaging in this assessment of the interannual dynamical variability is not represented in the sigma range indicated above?

Line 66: here the accounted CFC-11 is meant

I apologize for the delay!