

Atmos. Chem. Phys. Discuss., referee comment RC1  
<https://doi.org/10.5194/acp-2020-1297-RC1>, 2021  
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## Comment on acp-2020-1297

Anonymous Referee #1

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Referee comment on "An Arctic ozone hole in 2020 if not for the Montreal Protocol" by Catherine Wilka et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1297-RC1>, 2021

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There is a great deal of interest in the extent of ozone depletion during the recent cold Arctic winter of 2020, with a number of papers appearing, especially in a JGR/GRL special issue. This paper adds to that work mainly by investigating how much worse the depletion would have been without the controls of the Montreal Protocol. This is done through a specified-dynamics 3-D model run with a scenario that assumes no protocol. Secondary aspects of the paper are presentation of Arctic ozone sonde observations (including 2020) and an investigation of different model denitrification schemes.

I find that that paper has some interesting results would could make it suitable for publication in ACP. However, I do find that further analysis is needed in order to back-up some of the conclusions, along with clearer organization of the main points. I give my comments below.

### General Comments

1) One main message of the paper is that the Arctic ozone depletion of 2020 would have been much worse without the Montreal Protocol. That is without doubt. However, it is not possible to know exactly how demand for CFCs and similar gases would have evolved. The 3.5%/year growth since 1985 is an assumption and that should be made clear.

2) It seems like more use can be made of the available sonde data for evaluation of the Real World run. There are ozone plots with model only (5, S6a), and plots with data only (2a, S3). Better use of the data could be made for evaluating the model.

3) One message from the abstract is that the large Arctic ozone depletion of 2020 can be used to test the parameterization of PSC denitrification. Here the results presented do not go into enough detail. Maps of HNO<sub>3</sub> are shown for 70 hPa. A number of questions come to mind. How well do the simulations do at other altitudes? How would this affect other winters? I realise that these are major questions but normally a study which aims to present an improved denitrification model would be based on more than just one altitude in one winter. Also, the ultimate choice of the denitrification scheme is based on the impact on ozone which is not shown. This is an indirect test and we need to see how large the sensitivity of ozone is.

4) It is not clear if the paper is presenting the 2020 Arctic sonde data for the first time. Is that a novel aspect of this paper?

#### Specific Comments

5) I think that the abstract needs a lot of work. It is short compared to what is possible in ACP and it lacks some details. Also, it seems to jump around in the topics covered. The abstract mentions 'record observed local lows'. It is not said where these numbers are from and if this paper is presenting the 2020 sonde data for the first time. The abstract also says that 'This provides an opportunity to test...' without stating how the parameterizations are being tested and what the results are. It is also not clear that it is the RW run which is used for the testing; the abstract makes it sound like the WA run allows the testing. After this, the abstract returns to the WA run so the summary of that is split. Overall, I think that the abstract needs a more logical flow to cover the results and more information to summarise what was found.

6) Line 23. Farman et al.

7) Line 24. 'PSCs were the primary culprit'. This is not really true. PSCs are an essential part of the chain of events which contains a few steps. CFCs (and other chlorine gases) could be seen as a culprit and the one that humans can control. Alternatively, PSCs could be described as the key step that was not understood and why the ozone hole was not predicted etc.

8) Line 39-40. 'near-complete recovery'. What does this mean? We don't expect a smooth return to e.g 1980 ozone levels everywhere. Models suggest the tropics may not get to those levels before column ozone starts to decrease due to circulation changes.

9) Lines 68-69. You should make it clear that this scenario of 3.5% year growth since 1985 is an assumption and state why you choose these parameters.

10) Line 87. Why do you use the smallest value for the WA runs? Please explain.

11) Line 104. Waters et al seems to be a general MLS reference. Please also give one for the specific HNO<sub>3</sub> product.

12) Line 112. Spell out WOUDC.

13) Lines 113. Give the location (latitudes at least) of all the stations here.

14) Lines 118-119. The paragraph ends on a confusing note because it is stated that Eureka is used for profile comparisons, but that leaves the reader wondering what the other sonde data is used for (time series at 50 mb, as it turns out).

15) Lines 133-134. Here figure for the size of the 2011 Arctic ozone 'hole' is given – 11 million km<sup>2</sup>. This is still significant (see statement on what is an ozone hole on line 59) but I think that the authors point is that it is smaller than that which has occurred in the Antarctic since the mid 1980s when the term 'ozone hole' was first used? These points need to be made clear here in the results section. This also relates to the title of the paper, which presents 2020 as the first time that historic meteorology would have led to an 'ozone hole'. A big factor in this change since 2011 is clearly the assumed increasing chlorine, but also the meteorology of 2020 had, I believe, anomalously low dynamical ozone replenishment. Can you comment on the importance of these factors? There are at least references for the low absolute column ozone in 2020 in the JGR/GRL special issues.

16) Line 147. Figure 1. Please use (a), (b) etc here to label the panels.

17) Line 157. Caption for (b) should make it clear only model O<sub>3</sub> is plotted.

18) Line 158-159. Confusing as worded. The sonde, which shows the record low O<sub>3</sub> values, is at Eureka, and then compared to the nearest model profile.

19) Line 167. Better to say 'instrument' not 'satellite'.

20) Line 170. The figure caption is not very specific to the individual panels. Better to call out each individual panel and state what is changing in each case.

21) Line 174-175. Please indicate which of the runs shown is the 'standard' WACCM approach.

22) Line 175. Six? Figure 3 has 4 model panels? Also, could use labels (a)-(d) rather than saying 'first two rows'. That seems tidier.

23) Line 177. Figure 4 caption. This continues the pattern of the figure captions being too brief and not fully explaining the plot. The caption starts by calling out the 2020 points but these only extend from January to April. Also, the points 'prior to 2020' are, more specifically, years 2010-2019 (based on title). Therefore, I would suggest first describing the majority of the plot (minimum ozone by day of year for 2010-2019), and then describe the 2020 points. The left hand column in the legend should have the dates (2010-2019) to make clear the distinction with the right-hand column.

24) Line 180. 'bottom row, middle panel'. There is only 1 panel? I would suggest using '(e)' in any case.

25) Line 181. three?

26) Line 182-183. There is a major leap in the argument here. Figure 3 is comparing model v MLS HNO<sub>3</sub> to test the denitrification, which impacts HNO<sub>3</sub> directly. Then, the argument jumps to using the O<sub>3</sub> profile to test the model. There are many other processes which would affect the model ozone profile (chemistry, dynamics) and so there needs to be much better justification for this.

27) Line 184. 'insofar as they can be determined from reanalysis'. Better to say 'insofar as they are represented by the reanalyses'.

28) Line 188 'dailyminimum' – space needed.

29) Line 188-190. This is a long sentence and difficult to read. It is not clear when it mentions 'for the past decade ... compared to the preceding years'. Please revise.

30) Line 190-191. 'the scatter of the three overlaps'. The RW run and OMI should ideally have the same scatter! Better to compare RW versus OMI, and if the model is good you can compare RW v WA.

31) Line 194. Figure 5 caption. The bold/non-bold text is reversed compared to other figures. The caption should state 'ozone' somewhere. Give the latitudes of the stations so that it is clear Eureka is north and Syowa is south. These are sonde stations so please show the data for comparison (or choose a close day when there is data).

32) Line 196. I don't think you can use 'record' for a hypothetical model run? A larger assumed use of CFCs would give even lower O3!

33) Figure S1. Please give a brief explanation of the 'total equivalent effective chlorine'. I assume this is the tropospheric equivalent chlorine loading? State the alpha factor used for the calculation of equivalent chlorine. What does 'effective' mean in this context?