acp-2020-746

Reply to the review of referee #2
by Dameris et al.

Thank you very much for your detailed review and the specific comments regarding our manuscript. Your statements and suggestions are highly appreciated and have helped to improve our manuscript. We have considered them in the revised version of the paper. A detailed response to your comments is given below.

In the following the points raised by the referee are displayed in black and our responses are given in blue.

General comments

I wonder if it is fair to refer to the Artic winter 2019/20 as an 'ozone hole'. While the authors are clearly correct in stating that total column ozone falls below the 220 DU threshold, typically used to define the edge of the ozone hole, it should be remembered that in the Antarctic column ozone values typically fall far below 220 DU, and for a timescale measured in months. Can the authors use further common metrics (ozone mass deficit, minimum column ozone, etc) in their evaluation to give a better understanding of the column ozone evolution? Additionally, I feel it would be beneficial if the authors included data from the Antarctic in their timeseries plots, so the reader can get an impression of the how the Arctic winter 2019/20 compares to what is more generally considered an ozone hole. The authors state that these low ozone values cover a large area (0.9 million km2), but that is a tiny fraction of the area covered by the Antarctic vortex. I feel that either the authors should refrain from using the term 'ozone hole' or to place this term into context by comparing it with the Antarctic ozone hole and state explicitly that it is much smaller and shorter lived than the

In the revised manuscript we are now talking about record low ozone values in spring 2020 or we name it an ozone hole-like feature. The term "Arctic ozone hole" is avoided in the revision. In addition, the Arctic values are compared and discussed with corresponding Antarctic values. The Figures 2, 5, 7, and 8 (now Figs. 3, 6, 8, and 9) have been updated to allow for a direct comparison of Arctic and Antarctic values and quantities. In accordance the title is also changed.

In Figure 7 (new Fig. 8) we are presenting the minimum TOC in the polar cap region (50°-90°). It is expected that the minimum TOC are detected in this latitudinal region, which covers also the inside of the polar vortex.

Further, a lot of emphasis is placed on the idea that the winter 2019/10 was the first instance of column ozone falling below the 220 DU threshold. However, the authors' Figure 7 shows that there are repeated instances of column ozone below 220 DU in the thin black line. While the authors refer to these as mini-holes, and explain the role in dynamics in their formation, I feel a distinction should be made between these and the 2019/20 winter – is it fair to say that this winter constitutes and ozone hole because these events are longer lived? While the winter 2019/20 is certainly atypical, it is wrong, based on this figure, to say, as the authors do on P11L17-18, that it is the first time these values have been observed. And if the qualify here is that they occur over a 'large area', does a new definition for an ozone hole need to include some measure of the areal extent?

You are right and especially the sentence ("For the first time …", P11 line 17-18) is misleading. We revised this sentence and also at other places in the manuscript; the statements are now hopefully clearer. In particular, we are now trying to avoid the impression that such low values (TOC) are observed for the first time. It is now clearly stated that it shows an ozone hole-like structure with such low values over a longer time period. We hope that it is now much clearer stated that such record low TOC values (below 220 DU) in the Arctic were detected over a period of five weeks in Arctic spring and that this is observed for the first time. And, it is now clearer stated that the TOC values are certainly higher than the respective values in Antarctic spring, and that the area of the Antarctic ozone hole is much larger in comparison with the area of low TOC values in Arctic spring 2020.

I miss in the introduction some general information on the processes involved in polar ozone depletion. While these processes are mentioned later in the manuscript, a paragraph in the introduction detailing the polar vortex, cold polar lower stratospheric temperatures, PSC formation, heterogeneous chemistry, and subsequent catalytic

ozone depletion upon return of sunlight to the polar vortex would significantly aid the reader. Additionally, I would like to see more information on how the Arctic and Antarctic differ: increased wave activity in the Arctic, the fact that the Artic vortex is often displaced from the pole, which can affect the amount of sunlight that can reach the vortex, the relative importance of chemical depletion vs transport.

A new paragraph is now included in the Introduction, which discusses briefly the involved processes regarding polar ozone depletion. Some more information (not only in the Introduction) about the differences between Northern and Southern winter conditions in the stratosphere is given, which is also related to the dynamics and the transport of air masses. Corresponding references are added.

The authors focus on the large-scale meteorological conditions within the winter 2019/20 Arctic polar vortex, particularly the area below the 195K threshold as a metric for PSC occurrence and chlorine activation. Can they say anything about local conditions, particularly for example the role of orographic gravity waves during the winter of 2019/20 and the impacts of these on local temperatures?

This is a very interesting and important question. Yes, in our study we are focusing only on large-scale processes. You are right, possibly orographic gravity waves can affect local temperature and this definitely could impact the formation of PSCs, in particular in the Northern hemisphere. Unfortunately, looking in more detail on local effect is beyond the scope of this study.

The Harris et al. (2010) paper cited in the manuscript highlights linearity between PSC occurrence and ozone depletion. Similarly, Hommel et al. (2014: Chemical ozone loss and ozone mini-hole event during the Arctic winter 2010/2011 as observed by SCIAMACHY and GOME-2) highlight linearity between total column ozone change at 100 hPa eddy heat flux. Are the authors able to say something about if the winter 2019/20 falls on these linear relationships identified in past studies? Or does this extreme winter violate the relationships identified in other studies?

So far, we did not carry out a more detailed analysis looking at the linearity between TOC and the change of the meridional heat flux at 100 hPa mid-latitudes as discussed briefly in the paper. Since the temporal evolution of the meridional heat flux in winter 2019/2020 indicates smaller values (variability) than usual (see GSFC webpage, which is mentioned in the manuscript) and the spring TOC values (see new Fig. 3) are low in the polar vortex, this assumption could hold. The same could be also true for the linear relationship between the occurrence of PSCs and ozone depletion. We have not analyzed the rate of chemical ozone depletion, but our analyses of conditions for the formation of PSC are hinting in this direction (see also the papers by Manney et al., 2020, Lawrence et al, 2020, and Wohltmann et al., 2020, which are considered and discussed in the revised manuscript). To our current understanding this was an expectable winter (with respect to the known dynamical conditions), leading to exceptional TOCs.

Some key references are missing from the manuscript, with many instances of only one, recent citation given during key discussion. I would encourage the authors to expand upon the literature already cited in the manuscript.

We have added several references in the revised manuscript. Some of them have been published very recently (see also our short replies to previous reviewer comments).

Specific comments:

P2L19: ‘Nevertheless, the current atmospheric content of CFCs is still enhanced. . .’. It would be beneficial to explicitly state a date here, i.e. ‘...still enhanced with respect to 1980s values. . .’

“with respect to 1980s values” is included now.

P2L22: Care should be taken when using a term such as full recovery. While several studies show that column ozone is projected to return to 1980s values by the middle of the century, is that really full recovery? Some of this signal is driven by stratospheric cooling resulting from increased CO2 mixing ratios, and is separate to recovery driven by reduction in ODSs. I would prefer the authors say something about ozone return to historic values, which is an important part of the recovery story, rather than 'full recovery'.

Has been changed accordingly.

P5L3: Is 'strong cooling' correct here, or are the cold temperatures a result of reduced warming? Can the authors say anything about the radiative and dynamical processes operating within the polar lower stratosphere? This thought is also applicable to P7L7.

The sentence has been changed. We state now clearer that the dynamical conditions in 2019/2020 with low planetary wave activity result in strong radiative cooling of the polar lower stratosphere during polar night, which causes a strong polar vortex. A more detailed discussion is given now about the importance of radiative cooling and reduced (meridional heat) transport of airmasses.

P5L21: The analysis here focuses on column ozone values north of 50°N. However, Figure 1 of the manuscript shows that the Arctic vortex is not symmetrical about the pole, and so this average includes considerable amounts of column ozone from outside the Arctic vortex. Is it possible to plot vortex averaged column ozone instead, and so separate out the low values from inside the vortex from the high values outside?

In Figure 7 (new Fig. 8) we are looking at the daily minimum TOC values north of 50°N. We are <u>not</u> looking at the mean TOC values in the polar cap region (50°-90°N). We are comparing the minimum values of the three Northern winters 1996/1997, 2010/2011, and 2019/2020. In the revised Figure 7 (now Fig. 8) we have added the seasonal evolution of the minimum TOCs over the polar cap of the two Antarctic years 2016 and 2019. In principle, we expect that a comparison of the averaged TOC of the polar vortex will provide a qualitatively information, which is similar to the minimum TOC. In addition, we have added a new Figure (Fig. 2 in the revision), which shows the respective PV values on the 475 K isentrope. Appropriate explanations are given in the revised manuscript.

P6L22: 'The daily accumulated ozone hole area in March and April was estimated with 4 million km2' – how does this value compare to that for September and October of a typical year in the Antarctic? I suspect the Antarctic value is many times larger. If so, is this a useful metric – I feel it may be misleading if not placed into context.

We set the numbers of the Arctic and the Antarctic winter/spring seasons into context, now. This comparison clearly shows that the values for the Arctic are very much smaller than those found in the Antarctic. Thus, misinterpretation is now hopefully avoided! The numbers discussed here and shown in Figure 6 (now Fig. 7 in the revision) should only be compared for the NH winter/spring seasons and are supposed to facilitate the intercomparison of the Arctic situations discussed.

P10L1-6: Care should be taken here attributing all of the low column ozone values to chemical depletion. The authors discuss the importance of dynamics in the preceding paragraphs in preconditioning the polar vortex, but the phase 'ozone depletion rates' to me describes ozone loss through catalytic reactions, whereas in actuality the low column ozone is driven in part by chemistry and in part by reduced transport of ozone to the polar cap. This is obvious from your Figure 7, as column ozone increases from December to May, and this is not driven by chemistry.

You are completely right! This paragraph has been revised.

Technical:

P2L26: Check use of ‘Exemplarily’

Changed to “For instance”.

P7L22: replace ‘cumulated’ with ‘cumulative’ – also other instances throughout the manuscript.

Changed.

• Page 1, lines 27–28: I would delete this sentence. This is exactly what I would call ”attention-grabbing”, but it doesn’t really transport information.

This sentence has been deleted.

P11L19: remove ‘a’ from ‘about a five weeks’

Done.

The x-axis label for all timeseries plots says ‘time [days]’, which I would expect to be a set of numbers, but the plot shows date on the x-axis. Please revise.

They have been changed accordingly in the new Figures 3, 6 and 7.