Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-746-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "First description and classification of the ozone hole over the Arctic in boreal spring 2020" *by* Martin Dameris et al.

Anonymous Referee #2

Received and published: 20 August 2020

Dameris et al. present a documentation and evaluation of the evolution of observed total column ozone during the winter 2019/20, contrasting it with two other winters with anomalously cold, stable polar vortices, as well as climatological Arctic column ozone. This study makes an important contribution to the study of Arctic ozone, and it is timely given it evaluates the most recent winter. I found the manuscript to be well written, the analysis clear and the figures well presented. I feel the paper fits the scope of ACP and would recommend publication of the manuscript after the authors address the comments below.

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

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?

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.

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?

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?

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.

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. . .'

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'.

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.

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?

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.

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.

Technical:

P2L26: Check use of 'Exemplarily'

P7L22: replace 'cumulated' with 'cumulative' – also other instances throughout the

manuscript.

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

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.

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