acp-2020-746

Reply to the review of Ingo Wohltmann
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. Regarding your remark concerning the joint special issue of JGR and GRL on the 2019/2020 winter, we explained the circumstances in our short comment (July 30), which has led to the submitted draft of the manuscript. In the revised version of the manuscript we are referring to the (accepted) papers of the JGR/GRL special issue and we have set the results of these papers into context with our findings.

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

General

In general, I found most parts of the manuscript to be scientifically sound and would recommend publication. While there are some issues (see specific comments), I don't think that there is anything which cannot not be resolved. I have to admit that I had more of a problem with the wording and readability in some places. On the positive side, I did not find a single typo. But on the other hand, wording was quite awkward in some places, and sometimes the text could have been less confusing and better organized, it felt a little bit rushed in places. I have the impression that asking a native speaker to go through the text would help in many places.

We thank the reviewer for this statement regarding our scientific results. We have tried to address the raised points of the reviewer and the choice of words has been selected more carefully in the revision, in particular with respect to the aspect of talking about an "ozone hole" over the Arctic. For instance the title has been changed appropriately. Moreover, specific passages from the text have been revised. Some of the figures have been slightly revised (labeling) and extended (e.g. regarding the Antarctic) to improve the readability of the manuscript and provide clearer scientific messages. A new figure (Figure 2) has been added.

Major comment

Unfortunately, however, I have a major comment on a less scientific issue. The general wording and tone of the paper are quite sensationalist in title, abstract and conclusions (or as a colleague who is a native speaker put it: "attention-grabbing"). I don't find the wording appropriate in several places. I don't think that you do yourself or the stratospheric community a favor with that. You need to phrase your manuscript more carefully. In addition, things are sometimes not put into perspective, which may lead the reader to draw the wrong conclusions. If you imply conclusions here that are at least debatable and at the same time choose a manuscript title that will attract the interest of the public or press, I think this could be problematic. In particular:

• Remove "first" from the title. This is not a contest, but a scientific paper. In addition, it is just not correct. There are papers under review in the upcoming GRL/JGR special issue that have overlap in content to what you write here, and the first paper has already been accepted.

We understand the reviewers concern and therefore we have changed the text. Among others, the title of the paper has been changed. As stated before (in our first short reply) our intention was not to be "the first" but to provide "an initial" overview of the situation in 2020. Further we also provide more context to avoid misunderstandings. Further papers of the GRL/JGR special issue, which are relevant to our manuscript, are addressed now and a paragraph pointing to the special issue has been added in the Conclusion.

• I find the repeated and prominent use of the phrase "ozone hole" highly problematic. In the abstract and title alone, it appears 7 times. This raises expectations and may imply conclusions for some readers which are not really backed by the facts or are at least debatable. Given that the phrase "ozone hole" has played a prominent role in the public discussion in the last decades, many people will have a certain understanding of the phrase which sticks in their minds, and we as a community should be careful what we write (in our own interest). I feel obliged to go a little bit more into detail why I think that prominently stating that the winter 2019/2020 showed an "ozone hole" is problematic. I think we probably agree that the winter 2019/2020 was exceptional. It was the coldest stratospheric Arctic winter on record and showed the lowest ozone columns and concentrations ever observed in the Arctic, which were comparable to typical values in the Antarctic ozone hole locally and for limited time periods.

The Abstract and the title are appropriately revised. And, also in the following we have changed our wording regarding the "Arctic ozone hole". We removed it from the text entirely. Our intention was to clearly state that the situation was "exceptional" (as you put it) as (i) the TOC values were very low over a relatively long time period, and (ii) the shape of the region of low TOC looks "ozone-hole-like" (while we agree that the values are still much higher than in the SH). We tried to be more precise and to avoid possible misunderstandings. We are now comparing the spring situation in 2020 not only with spring 1997 and 2011, but also with a typical ozone hole over the Antarctic (2016) and with the small ozone hole in 2019. The "old" Figures 2, 5, 7, and 8 (now Figs. 3, 6, 8, and 9) have been extended by showing the respective values for the SH.

Having said this, it still was far removed from the usual conditions in the Antarctic ozone hole.
– First, the area with Dobson values below 220 DU was much, much smaller than the typical area of the Antarctic ozone hole. First of all, the area of the Arctic vortex is typically smaller than the area of the Antarctic vortex (Manney et al., 2011, gives a number of 60 % for 2011). Then, the area with values below 220 DU covers only a small part of the vortex. This can easily be seen in your Figure 1. According to the numbers you give in the conclusions, even at maximum, the area was less than 5 % of the Antarctic ozone hole (0.9 million km^2 compared to 20 million km^2). In the end, this is a little bit of a problem with the standard definition of the ozone hole as the area below 220 DU. This definition does not take into account the area covered by the hole at all. But certainly nobody would call it an ozone hole if the area of the hole would be only one square meter.

In context with the discussion of Figure 8 (now Fig. 9), we have now compared the size of the Antarctic ozone hole with the situation in spring 2020 over the Arctic. Also, at other places in the manuscript (in connection with "new" Figures 3, 6, and 8) the differences between the Antarctic and Arctic have been pointed out to allow for a direct comparison.

– Then, the vertical extent of the layer almost completely depleted in ozone in the Antarctic is much larger than the vertical extent of the depleted layer seen in 2019/2020 in the Arctic. Usually, ozone is depleted to near zero values in a large altitude range from about 350 K to more than 500 K in the Antarctic (e.g. Kuttippurath et al., doi:10.1038/s41612-018-0052-6). While the ozone profiles of 2020 show a pronounced minimum, very low values (below, say, 0.2–0.3 ppm) were restricted to a layer of a few 10 K depth around 450 K (see plot of a sonde measurement in Ny-Alesund on 27 March, measurement is the blue line). This also is a weakness of the 220 DU definition. In the Antarctic, values will usually fall far below 220 DU, while they only scratch 220 DU in the Arctic in 2020 (please see my comment to Figure 7 how to improve on this).

Figure 7 (now Fig. 8) has been extended by adding the data of the two Antarctic ozone holes in 2016 (a typical one with respect to duration and strength) and 2019 (one of the smallest ozone holes). Some discussion has been added. Appropriate literature has been considered, e.g. Wargan et al. (2020).

– While the lowest mixing ratios reached in 2019/2020 were comparable to mixing ratios that can often be observed in the Antarctic ozone hole (0.1– 0.2 ppm), they did not reach the near zero minimum values (0.01 ppm) that are typical for the Antarctic ozone hole.
– And last but not least, it is also the time period. The Antarctic ozone hole lasts several months, while the time period with very low ozone values in 2019/2020 was at most 5 weeks or so.

In context with the discussion of Figure 7 (now Fig. 8) , we have now compared the duration and shaping of the Antarctic ozone holes in 2016 and 2019 with the situation in spring 2020 over the Arctic.

Thus, I really would suggest to phrase things more carefully, e.g. to speak of an ”ozone minimum” or ”values comparable to values observed in the Antarctic ozone hole” and so on. Please see the specific comments for the places where I think you have to phrase things more carefully.

• Sometimes, it seems that you would like to push the reader into a certain direction by omitting information in strategical places. While this means that you don't write anything scientifically wrong formally, you may push the reader to draw conclusions that are not correct. In particular, it would have been very easy to include Antarctic data in figures like Figure 1, 5, 6 and 7, and to discuss this data in the text to put things into perspective. I think it is really mandatory that you change the manuscript to be more balanced and to put things better into perspective.

This was not our intention and hence we revised the manuscript accordingly (see previous comments). We hope that the changes in our manuscript are now in balance and that it is improved with respect to the discussion of the spring 2020 situation, especially in context with a typical Antarctic ozone hole.

Specific comments

• Page 1, lines 11–12: Please rephrase to something like ”record low ozone column” or similar and avoid the term ”ozone hole”.

Has been changed.

• Page 1, lines 12–14: The sentence would work equally well when you omit ”A persistent ozone hole pattern”. Just start with: ”Minimum total ozone column values. . . ”. To make the ”for the first time” in the sentence work better, maybe you could write ”for more than 5 weeks” (or 4 weeks?) instead of ”about 5 weeks”.

Has been changed.

• Page 1, line 14 (next sentence): Suggestion: ”Usually such low total ozone column values have only been observed in the ozone hole in the polar Southern hemisphere (Antarctic) in spring over the last 4 decades, but not over the Arctic.” Slight change in text, but larger change in meaning. But please state here in addition that column values will go far below 220 DU in the Antarctic to put things into perspective. It would also make sense to state the other differences which I have outlined in my general comment here (smaller area, vertical extent and time period).

The suggested slight change of the text is considered. In addition at the end of the Abstract a sentence is added to make clear the differences in the Arctic and Antarctic with respect to the low total ozone values and the differences in the respective area and time.

• Page 1, line 16: Change to ”The record low values were caused. . . ”

Wording is slightly changed.

• Page 1, line 16: A stable vortex does not enable a cold stratosphere. This confuses cause and effect, when there is no wave activity. When there is wave activity, it is a little bit more complicated, please see my comment to page 5, line 3 (sorry, wrote that comment first. . .). Please phrase that correctly.

We agree with your statement. Wording is slightly changed.

• Page 1, line 20: ”in the context of” is probably better.

We keep “in context with”. From our point it is the correct wording.

• Page 1, line 20: Replace ”ozone-hole like features” simply by ”cold winters”

Wording is 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.

• Introduction: In a later comment (page 5, line 3–30), I suggest to add a short overview on how ozone depletion works somewhere in the introduction (PSCs, cold temperatures, chlorine activation, return of sunlight, . . .) to be able to streamline the text in the later sections a bit.

After the second paragraph in the Introduction section, a short paragraph about the processes of polar ozone depletion is added.

• Introduction: In some places, I find the references a bit odd, while I miss others. E.g. Langematz, 2019 and Loyola et al., 2009, would not be the first ones that come to my mind. I would expect the review paper of Susan Solomon from 1999 somewhere (Rev. Geophys. 37, 275–316, 1999). A paper that is mandatory to cite in a study like this is in my opinion Solomon et al., ”Fundamental differences between Arctic and Antarctic ozone depletion”, 2014, doi:10.1073/pnas.1319307111 (it is by chance that it is Susan again). This is a review paper about exactly the topic you are talking about here, and it also contains some very critical remarks about using the term ”ozone hole” for the Arctic.

Some additional references are included in the Introduction. The papers by Langematz (2019) and Loyola et al. (2009) are cited with respect to other points, i.e. the “definition” of the ozone layer and the importance of satellite measurements, which are monitoring the atmosphere; they are necessary prerequisites for a better understanding of atmospheric processes. But you are right: We are happy to add the papers by Solomon (1999 and 2014).

• Page 2, lines 5–10: You don’t need to explain the meaning of the term ”ozone column”. This is basic textbook knowledge.

OK, but finally we decided to keep this sentence.

• Page 2, line 7: Delete ”so-called”

Done.

• Page 2, line 11: You need a citation for the 220 DU threshold (it is for example defined in the WMO report 2018, along with further references). This is more or less an ”official” definition which most people agree on, and you will need some references for that. All of your following discussion depends on this definition.

The reference of WMO (2018) is added now here.

• Page 2, line 14: This is misleading. Changes of ozone column by transport and changes of the column by chemistry are correlated (e.g. Tegtmeier et al., 2008, which you may want to cite here). A more dynamically active winter means both more transport of ozone into the vortex and less ozone depletion because of higher temperatures. Your sentence reads as if the difference between 450 DU and 220 DU would mainly be caused by chemistry. In fact, one of the fundamental differences between the Arctic and the Antarctic is that transport plays a large role in determining the Arctic ozone columns.

Good point! The text is changed accordingly and the reference of Tegtmeier et al. (2008) is added.

• Page 3, line 3: Perhaps replace ”with respect to low TOC” by ”in reaching low ozone columns”?

Text is slightly changed.

• Page 3, line 4–5: Some references from the paragraph starting page 10, line 27 would also fit, and see also my comment to this paragraph for even more references.

Here we added only the references of Solomon (1999) and Tegtmeier et al. (2008), which are important ones. We think that also in the reference lists of the different WMO ozone assessments all relevant publications can be found.

• Page 3, line 11: It would probably be good to mention the official databases for ozone sondes here, i.e. WOUDC and NDACC.

These ozone data sets of WOUDC and NDACC have been briefly mentioned now (later) in the manuscript and the Wohltmann et al. (2020) paper is cited in this connection.

• Page 3, line 13: You need to phrase that more carefully: ”which led for the first time to ozone values below 220 DU in larger parts of the vortex for an extended time period”.

Thank you. The text is changed accordingly.

• Page 3, line 25: As far as I know, the nominal resolution of ERA5 is 0.28125 degrees. It does not really make sense to sample the data at a higher resolution (but does not hurt either).

Parts of the ERA5 data set are made available on the CDS at this resolution (cf. e.g. Hersbach et al., 2020). PV data on isentropes is not available on this grid, but on a reduced gaussian grid, which is then converted to the 0.28x0.28 degree resolution as stated by the reviewer.

• Page 3, line 24–28: You don’t need to go into detail how you do a daily average. I trust you that you are able to do this correctly :-) In fact, you can replace anything between ”For our investigations. . . ” and the end of line 28 by ”We use daily and monthly averages in the following”. This is totally sufficient.

We decided to keep this as is because this was one of the points raised by the Editor with respect to the first draft of the paper. Our intention is to make clear how we prepared the data for our analyses.

• Page 4, line 3 and line 8: Delete ”first”. This is really not relevant in the context of this paper. Again, this is no contest.

A lot of work was necessary before such a consistent data set has been available for scientific purposes. And again, this was one of the questions raised by the Editor (concerning the first draft of the paper) with respect to the personal achievements of the author team.

• Why is Figure 2 the first plot that you discuss in the paper? You should change the order of the plots, so that Figure 2 becomes Figure 1.

Figure 1 was already mentioned on page 3, line 8. Therefore we keep the order as is.

• Figure 2 and accompanying discussion: Lawrence et al. from the special issue contains similar figures and discussion. Please cite and discuss Lawrence et al. here.

The paper by Lawrence et al. (2020) has been cited and discussed in this context.

• Page 4, line 25 and Figure 2: At first, I was a little bit confused by the plot because I didn't realize that the dots are monthly mean values and the lines are daily values, causing the colored dots not to lie exactly on the lines. Maybe there is some information overkill in the plot. One could replace the dots by a grey area showing the range of the daily values in all years for every day.

We have slightly changed the figure captions. Hopefully it is clearer now that the dots indicate the monthly mean values.

• Page 4, lines 26–28: I would suggest to delete the part in parentheses. Either discuss this explicitely in the paper, or leave it at ”caused by planetary wave activity”.

We think that referring to GSFC analyses is helpful. It contains additional information if someone is interested. We have slightly changed the sentence.

• Page 4, lines 28: Would be good to use the established terminology here (major warming, minor warming, sudden stratospheric warming...)

Wording is changed.

• Page 4, lines 29: The wording is a little bit awkward and hard to understand. I would try wording like ”stable and undisturbed vortex”, ”circular shape”, ”not displaced from the pole” etc. ”deteriorated” is not the correct word.

Text passage is slightly changed.

• Page 5, line 2: Figure 3 does not add any relevant information which is not contained in Figure 2. You could delete this figure without loss of information.

We would like to keep this figure because it nicely indicates the persistent circular shape of the polar vortex.

• Page 5, line 3: You confuse cause and effect here. First, in the absence of wave activity, the polar region gets colder than mid-latitudes in winter due to a lack of sunlight and because of radiative cooling. Then, a pressure difference develops compared to mid-latitudes, which causes a geostrophic wind as response. For a correct discussion, you would need to explain the mechanisms of the Brewer-Dobson circulation in more detail. Interannual differences in polar temperatures or vortex strength are caused by differences in momentum deposition by breaking waves (mainly in the mid-latitude stratosphere) which drive the BDC. That means that both temperatures and vortex strength are correlated, but that this has a common underlying cause, and not that one causes the other.

Based on your comment with respect to the Introduction, we have introduced the new paragraph and some additional information (e.g. Tegtmeier et al.) is given. We slightly changed the text on page 5. Regarding the mechanisms of the Brewer-Dobson circulation, we think that it is not necessary to explain the BDC in more detail in this paper.

• Figure 5 and 6 and accompanying discussion: Lawrence et al. and Wohltmann et al. from the special issue contain similar figures and discussion (e.g. Figure 1 Wohltmann et al. and Figure 11 Lawrence et al.). Please cite and discuss Lawrence et al. and Wohltmann et al.

The papers by Lawrence et al. (2020) and Wohltmann et al. (2020) have been cited and shortly discussed in this context.

• Figure 5 and 6: Please add typical Antarctic values in the figures and discuss them in the text. This will help very much to put things into perspective and I think this is mandatory.

We have updated Figures 2 (now Fig. 3) and 5 (now Fig. 6), showing corresponding values for the Antarctic. We show the data for year 2016 as a typical situation (in particular with respect to a "normal" ozone hole situation; see also the revised Figure 9), and year 2019 with a stratospheric warming (not major) indicating one of the smallest Antarctic ozone holes (new Figure 9).

• Figure 5: It is a little bit confusing that you use a polar cap area and not the vortex area here as the area where you look for the minimum. That only gives the desired result because of the comparatively high temperatures outside of the vortex. You are interested in the minimum temperatures inside the vortex here, because these are relevant for ozone depletion. It would be more consistent to base the plot on the vortex area (the plot would look almost identical probably).

Looking at the polar cap (50°-90°) is a standard diagnostic, which helps to identify the minimum temperature values on a solid foundation, in particular in undisturbed winters. And you are (as we assume too) right: the corresponding analysis concentrating on the polar vortex area would yield very similar results. Therefore we would like to keep the analysis as is.

• Figure 5: Can you really learn something from the minima of the monthly mean values? These will by definition always be higher than the daily minima. I don't really see that they provide any insight. I would suggest to remove them and to replace them by some grey area showing the range of the daily values over all years.

The monthly mean values (dots) allow getting an overview of the variability of the atmospheric system and you can classify the individual years (color dots). A figure with grey area based on daily values would yield to similar information. In addition this figure is related to Table 1.

Page 5, lines 11–12: The numbers for the vortex area are a little bit unintuitive. At least I don't have a really good judgement of them. An alternative would be to divide the values by the vortex area, which gives a value in percent, which is more easy to grasp. In addition, the unit for the cumulative area can't be correct. There must be some time unit missing (probably "days").

Here we only mean the sum of the vortex area and not the integral. Therefore, the units are correct. To make it clear, a half sentence has been added.

• Page 5, line 14: The citation seems odd. An obvious citation would have been the original study of Solomon et al., Nature, 321, 755, 1986. Or the WMO report or the 1999 Solomon review paper.

We have added the Solomon (1999) paper.

• Page 5, line 16–20: Discussion on ”ozone hole”. Please phrase that more carefully. You could state here that record low values have been reached and that their temporal extent and the covered area were unrivalled in other years (but please check that, this is just what I suppose is correct). It is certainly also ok to mention the 220 DU definition of the Antarctic ozone hole here, but I think it is mandatory here to discuss the smaller area, the limited vertical extent and the limited time period compared to the Antarctic ozone hole to put things into perspective.

The text is changed accordingly. Setting the areas of low Arctic TOC values in context with the respective Antarctic values is done later in the manuscript.

• Page 5, line 20: As far as I can see, this is the first time you mentiom Figure 1. Please correct the order of the figures.

Figure 1 was mentioned first on page 3.

• Figure 1: Add contours for the 220 DU contour and the vortex edge. These are things that are really hard to see in a coloured contour plot. And the 220 DU contour is really central for the discussion in your paper.

We think that it is not necessary. As said in the figure caption: ”The area with total ozone values below 220 DU are denoted with the dark purple color.” This should be sufficient.

• Page 5, paragraph lines 3–30: The readability of this paragraph suffers because of two issues in my opinion:

First, you introduce 5 figures in this short paragraph, but only by mentioning them in parentheses. It would help immensely to insert a few sentences starting with ”Figure xxx shows. . . ” in this paragraph.

Text is changed.

In addition, you try to explain how ozone depletion works in some half-sentences and in parentheses here and introduce things like Cl activation and NAT clouds. I think you could improve your manuscript a lot by adding a short paragraph in the introduction explaining the basics of ozone depletion in a few sentences (cold temperatures, PSCs, return of sunlight, chlorine activation, CFCs, …). Then, you could refer to that later, and the text would read much more fluently. You have tried to explain even more basic things in this manuscript, like the definition of ozone column or the averaging, so it seems odd that you don’t explain some things in the introduction which would really help to streamline the text in your paper.

Such a short paragraph is now added in the Introduction. Then it should be clear in the manuscript.

• Figure 6: To make things more consistent, you could show the range of values of all other years in grey, as in Figure 2, 5 and 7.

We would like to suggest keeping the figure as is. The aim of Figure 6 (now Fig. 7) is only to demonstrate the differences between these three Arctic winter situations.

• Page 6, line 9: Why is this expected from January on, and why is this expected with values above 220 DU? I don’t understand your reasoning here. Maybe it would be better to simple write ”is observed”?

Wording is changed.

• Page 6, line 12: See comment page 3, line 11.

As said already, WOUDC and NDACC have been shortly mentioned as an additional data source.

• Page 6, line 21: Please compare the area below 220 DU to the area of the vortex here. This would really be important and interesting information which helps to put things into perspective.

Text has been revised; the Arctic area with TOC below 220 DU is about 4% of the polar vortex area, which is determined for March 12. It is based on our new Figure 2 (PV). Some text has been added.

• Page 6, line 22: Again, the units are not quite correct (at least formally). A unit of ”days” is missing.

As said already, the units are correct.

• Figure 7 (and accompanying text): What really would add a lot of value to your paper with relatively small effort would be to show typical values from the Antarctic in the same plot and to discuss the differences between Arctic and Antarctic in the text. Please add Antarctic values to the figure to put things into perspective.

As suggested, in the new Figure 8 (and also in the new Figure 9), we add respective ozone data of the Antarctic. As an example, for a typical ozone hole (i.e. with respect to the means of the ozone hole area and its variation with time) we choose the year 2016. In addition, we also display the corresponding data for the year 2019, showing a small ozone hole. Corresponding zonal winds and minimum temperatures are also added in Figures 2 (new Fig. 3) and 5 (new Fig. 6).

• Figure 7: The same comment as for Figure 5 applies: It would be better to base the figure on the vortex area and not on a polar cap area.

As said, looking on the polar cap (50° to 90°) is a standard analysis, and we think that it leads to robust results. Looking at the respective polar vortex area would very likely yield a similar message. Therefore we would like to keep the figure as is.

• Page 6, line 27: There is a lot of literature on the 2010/2011 winter. Maybe it wouldn’t hurt to cite one or two studies more here. Suggestions: Sinnhuber et al., 2011, doi:10.1029/2011GL049784, Hommel et al., 2014, doi:10.5194/acp-14-3247-2014, Strahan et al., 2013, doi:10.1002/jgrd.50181, Kuttippurath et al., 2012, doi:10.5194/acp-12-7073-2012, . . .

Here, we add the reference of Kuttippurath et al. (2012).

• Table 1: I could live well without the table. The information can already be found in the plots.

We would like to keep the table. It shows impressively (in our opinion) the differences of these three years.

• Page 8, line 9: The wording is awkward. Suggestion: ”The winter 2019/2020 showed a larger volume below the formation temperature of PSCs than other winters for an extended period of time.”

Thank you for the suggestion. We changed it accordingly.

• Page 8, line 11–15: This is visible in the HNO3 measurements of MLS. Please cite the Manney et al. paper from the special issue here. You don't need to speculate.

Of course! We are happy to cite Manney et al. (2020) here. It is nice to see that our thoughts are confirmed.

• Page 9, line 12–14: Can you get a little bit more quantitative here? What is the quantity you are looking at here? Vortex mean temperatures at some level? In the moment, these sentences do not convey enough information to be useful (I am well aware that the uncertainties are large and will only allow a qualitative statement).

Based on the information available in the SPARC newsletter article, we put in some more information in our manuscript. The statement by Labitzke and Naujokat here was very clear that the spring 1997 was the coldest in the Berlin time since from 1955 to 2000. We give a short comment on the uncertainties of the Berlin analysis.

• Page 9, line 25–32: And of course, there is the ozone hole split of 2002.

Of course! We add some information about the split-event 2002 and refer to respective literature.

• Page 10, paragraph 1–6: The statements in this paragraph are problematic and don't really tell the truth because you omit information. You don't mention that the temperatures in the Antarctic are considerably lower than in the Arctic (even for 2019/2020) and that the period of low temperatures is much longer in the Antarctic. If you would have added this information to your plots (e.g. Figure 5 and 6), this would be quite obvious.

It is also not true that this would result in ozone depletion rates that are comparatively strong. The depletion rates will also depend on the amount of ClOx, which in turn e.g. depends on the amount of descent in the vortex. In fact, this is a rather complicated topic. The crucial difference between the Arctic and the Antarctic which leads to very low ozone values is the much longer time period with ozone depletion. Since ozone loss is usually in saturation in the southern hemisphere, some details on the path to zero ozone don't really matter here.

You are fully right! The text is changed (some is deleted) and the respective information about the difference in Arctic and Antarctic temperature is discussed. Figure 5 (new Fig. 6) is revised by showing now also the respective Antarctic temperatures for 2016 and 2019.

• Page 10, line 10: Please give numbers for the chlorine content. This would show that the differences are not that large (I assume 10 % to 20 % difference?) and that it is not too surprising that differences in temperature are the main driving factor. But in principle, you are right, this is worth mentioning. Maybe some additional discussion along the lines above is appropriate.

The change of the atmospheric chlorine content over the about last 20 years was already mentioned on page 2 (line 18). The number (15%) is given here again with the corresponding reference (Chapter 1 in WMO, 2018). And following your suggestion a sentence is added to highlight the importance of the low temperatures.

• Page 10, line 17: In fact, a dehydration event can clearly be seen in the MLS H2O measurements. Please cite Manney et al. from the special issue here.

Done!

• Page 10, lines 18–22: You repeat what you already have said on page 8. Please delete. See also the comment to page 8, lines 11–15.

We think that a short repetition here is helpful because we are now in the discussion section. The two sentences have been revised and Manney et al. (2020) is cited again. The sentence about our suspicion is deleted and the half sentence about 1997 and 2011 is also deleted.

• Page 10, line 24: See comment page 10, line 10.

Done!

• Page 10, line 27. Please rephrase ”atypical ozone hole” to something like ”record low ozone values”.

Is changed!

• Page 10, line 29 and line 32: I totally agree that you never can tell climate change from a single year and that you have to look how this evolves in the future in the context of a longer time series. On the other hand, you can have a look into the winters observed so far and don't need to wait for the future to have a long timeseries. And the year 2019/2020 does add information to this timeseries, since it was the coldest Arctic winter observed so far and the coldest winters have become colder in the last years quite consistently, at least according to some metrics and meteorological data sets. You could cite Wohltmann et al., Figure 1, from the special issue for a figure illustrating this quite well. But in general, I agree with you.

The paper by Wohltmann et al. (2020) has been cited here.

• Paragraph Page 10, line 27 to page 11, line 10: Some additional recent relevant studies on this topic: Ivy et al., 2016, doi:10.1175/jcli-d-15-0503.1, Rieder and Polvani, 2013, doi:10.1002/grl.50835, Butchart et al., 2010, doi:10.1175/2010JCLI3404.1, Bednarz et al., 2016, doi:10.5194/acp-16-12159-2016. These contain a lot of interesting discussion on this somewhat controversial topic (Are coldest winters getting colder etc.), which you may want to add here.

References of Bednarz et al. and Ivy et al. are now included.

• Page 11, line 2: I think an important point to mention here is that the other important driver of changes in stratospheric temperature (apart from changes in radiative cooling by greenhouse gases) are changes in the strength of the BrewerDobson circulation (e.g. Langematz et al., 2014 and many more), and the BDC in turn is affected by climate change.

Thank you for your point. For the future most climate models indicate a strengthening of the BDC. But comparisons of climate model (incl. CCM) results with analyses of observations since 1975 (e.g. Engel et al.) so far did not show a consistent picture for the past. We have now added a sentence indicating the possible role of an intensified atmospheric circulation in the future. Langematz et al. (2014) was already included in the manuscript, but this paper is now placed in better relationship.

• Page 11, line 11–14: This paragraph seems a little bit out of context.

Yes, but we got such questions about the enhanced CFC-11 emissions many times in connection with the record low Arctic ozone values. Therefore this small paragraph at the end of the discussion part is included. We hope that you do agree to that.

• Page 11, line 16–17: The sentence seems overly complicated and wording is a little bit awkward. Suggestion: ”This study presents a description of the Northern winter and spring season 2019/2020 considering the. . . ” ”regarding” seems not to be the correct English word to me, I think ”considering” is what you mean).

Thank you! Your suggestion is accepted.

• Page 11, line 17–19, 23–26: Please phrase more carefully. It is ok to discuss that the observations were below the 220 DU threshold for a longer period of time, but please also discuss the differences to the Antarctic ozone hole in area, vertical extent and duration that I have mentioned in my general comment right after lines 17–19. I am very happy that you finally discuss some of this in lines 23–26, but please move it a few lines up and discuss a little more. I.e. put things into perspective. Please don't call it an ”ozone hole”. And don't claim that this year was the first occurence of an ”ozone hole”. This was already claimed by some people in 2011, and it didn't help in the discussion back then, too. In general, I don't find this chase for superlatives very helpful, and it does not help to advance our scientific understanding.

This paragraph is slightly revised. The expression "ozone hole" is deleted.

• Page 11, line 23–26: I am very happy that you finally discuss this here, but I hope you agree that it would have been necessary to mention this much much earlier (and more often) in your manuscript.

It is now discussed in connection with the Figures 2, 5, 7, and 8 (new Figs. 3, 6, 8, and 9).

Technical corrections

• Page 3, line 18: You can delete ”It must be noted that”.

Deleted!

• Page 6, line 16: I don't think ”outstanding” is the right word here. Maybe ”prominent” or ”remarkable”?

Changed!

• Page 8, line 1 and 3: I would write ”ice PSC” and not ”ICE-PSC” (ice is not an abbreviation).

Changed!

• Page 8, line 28: ”Discussion” and not ”Discussions”

Changed!

• Page 9, line 17: ”recall” is not the right word. Suggestion: ”We note”

Changed!

• Page 11, line 19: You very probably mean ”compared” and not ”confronted”?

Changed!

• Page 11, line 20: I would say ”which show” and not ”which are showing”

Changed!

• Page 11, line 20: I would say ”the most recent datasets”

Changed!

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.

acp-2020-746

Reply to the short comment of Gloria L Manney
by Dameris et al.

Thank you for your interest regarding our paper and in particular for your detailed comments and the specific points regarding our manuscript. Your statements and suggestions are appreciated and have helped to improve our manuscript. We have considered your critical notes in the revised version of our paper. A detailed response to the comments is given below.

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

Other comments have already discussed the overly-casual and ill-defined use of the term “Arctic ozone hole” and I believe that subject has been covered well already (though my primary scientific comment below will touch on it in the context of comparison of specific winters).

Other comments on / reviews of this paper have also already discussed the claim of “First” and the lack of citations of other papers in review and published on this exceptional winter, and I agree overall with their remarks. I do have additional “philosophical” comments on this subject: I am happy to see numerous papers submitted …

These are, as I said above, philosophical rather than scientific views, so I can only ask the authors (of this preprint and others!) to ponder them and make revisions according to their judgement of the merit of these points.

As mentioned in the replies to the referees, we have changed our choice of words, in particular with respect to the terms “Arctic ozone hole” and “First”. We would like to mention again that first to us meant not “the first” but rather “an initial”. We did not want to convey that the situation in 2020 in the Arctic is similar in size and duration to Antarctic ozone holes. To avoid any misunderstandings, we have formulated it more carefully in the revised version. The missing literature is now cited and discussed, and also the accepted papers of the special issue of JGR/GRL with respect to the winter 2019/2020. The submitted and not yet accepted papers, which are related to our work, are also mentioned. Further we added a short paragraph referring to the corresponding special issue in our Conclusion section.

Major Scientific Comments:

The biggest issue that has not been raised in other comments / reviews at the time I’m writing this, and that I believe must be addressed before peer-reviewed publication, is the comparisons of 2019/2020 with 2010/2011 and 1996/1997, and the failure to communicate the very large differences in polar processing and ozone loss in 1996/1997 compared to the other winters studied. In the context of comparing superficially similar springtime lower stratospheric vortex conditions in 1996/1997 and 2010/2011, the very large differences in polar chemical processing in those two winters have been extensively highlighted, first in detailed discussion in the supplementary information (SI) of Manney et al. (2011, Nature), and in numerous later publications culminating in a detailed summary / synthesis in the WMO 2014 Scientific Assessment of Ozone Depletion (section 3.2.3.3), which provides further references. In short, for numerous reasons (very late lower stratospheric, LS, vortex development and late drop of temperatures below PSC thresholds, smaller altitude region of low temperatures, weaker LS vortex throughout the winter, little/no denitrification, etc), chemical ozone loss was much less in 1997 than in 2011 (and hence than in 2020, which saw as much or more chemical loss as in 2011, eg, Manney et al, 2020; Wohltmann, et al, 2020; Grooß and Müller, 2020).

We have added some statements and corresponding references in the revised version, which clearer point out the larger differences of chemical ozone loss of the three winter-spring seasons 96/97, 10/11, and 19/20.

Moreover, dynamical conditions led to frequent ozone mini-holes (e.g., Coy et al., 1997) and higher tropopause altitudes (e.g., Manney et al, 2011, Nature, SI) in spring 1997 that contributed to lower column ozone via dynamical processes than followed other winters with comparable chemical ozone loss. This is an important distinction that it is essential to address for the comparisons in this manuscript to provide accurate information on the similarities (a few) and differences (many) between 1997 and the other two winters considered. Statements such as (to pick only one example, page 10, lines 8-9) “...all three years showed particularly strong ozone depletion...” are scientifically inaccurate. This also folds in with the inadvisability of lightly using the term “Arctic ozone hole”, as 1997 is a classic case of a situation that looked superficially similar to the Arctic winters, 2011 and 2020, with the most chemical ozone loss and in some ways “Antarctic-like” conditions (see WMO 2014, Section 3.2.3.2; Manney et al, 2020; Wohltmann et al, 2020), but which in fact had chemical processing that was in no way comparable to that in the Antarctic.

Thank you for the comment that in spring 97 ozone mini-holes are frequent and clearly affected the TOC. We added this point (including the corresponding literature). With respect to the mentioned statements: You are fully right! The used mode of expression was misleading as "ozone depletion" is likely to be understood as "chemical ozone depletion". We have formulated it more carefully in the revised manuscript. (Our intention was to say that in all three years particularly low TOCs were observed in March.)

I also have concerns with the description of the dynamical conditions in relation to previous winters. The dynamical situation is described using only 10hPa zonal mean winds and 50hPa temperatures.
Zonal mean winds in the middle stratosphere (10hPa as opposed to the levels around 50hPa where chemical processing maximizes) are virtually irrelevant to the state of the lower stratospheric vortex, because:

(1) Vortex strength, size, and geometry vary strongly with altitude in different ways in different winters -- we have seen winters (such as 2010/2011) where the vortex was exceptionally strong in the lower stratosphere but not in the middle stratosphere, and winters (such as 1997) where the vortex was for much of the winter fairly typical in the middle stratosphere but exceptionally weak in the lower stratosphere.
(2) The Arctic vortex is rarely close to symmetric or pole-centered, even in the coldest and/or most dynamically quiescent winters (see, e.g., Figure 1 in Manney et al, 2020, or any of numerous other publications in the past ~20 years), and its size, shape and position vary dramatically both intraseasonally and interannually. Thus, zonal means, even were they examined at altitudes in the range where LS polar processing occurs rather than at 10hPa, provide very little information on characteristics of the polar vortex such as size, location, and strength.

To get an overview of the dynamic state of the stratosphere in specific winters it is usual to analyze the temporal evolution of the zonal mean wind at 60° (10 hPa or 30 hPa) and the zonal mean temperature in polar regions (e.g. at 80°, 30 hPa or 50 hPa) or alternatively the minimum temperature in the polar cap region (50° to 90°, 50 hPa). Both are providing a good overall view of the dynamic situation. For our considerations, in Figures 2 and 5 (now the new Figs. 3 and 6) we identify obvious differences in the seasonal behavior in the individual winters (NH and SH), which support our interpretation of the different years. In our discussion of results, we are concentrating on these pressure levels.

Lawrence et al. (2020) also focused on the zonal mean zonal wind at 60°-65°N, 10 hPa (their Fig 1) and the minimum temperatures poleward of 40°N at 50 hPa (their Fig 11). But you are right that we have to point out more clearly the height dependence of the polar vortex and its changes. We have mentioned it in the revised version, also by citing the paper by Lawrence et al. (2020).

In addition, Figure 2 (new) shows the PV on 475 K with respect to Figure 1, indicating the strength and position of the polar vortex.

Similarly, 50hPa minimum temperatures north of 50N and area of T < 195K at 50hPa, while very relevant to polar chemical processing, are by themselves inadequate to characterize the potential for chemical ozone loss in the LS vortex because:

(1) The vertical structure/location/extent of the region with temperatures conducive to PSCs varies strongly interannually and within seasons; this is one of the reasons why one of the most useful measures of polar processing / ozone loss potential (both day-to-day and as a measure of total ozone loss potential in a given winter) is V_psc, the area below the PSC or chlorine activation threshold integrated over all lower stratospheric levels.
(2) Because the LS vortex varies strongly in size, shape, and position, while the high-latitude minimum associated with the polar vortex is usually north of 50N in dynamically quiet winters, this may not always be the case, and is certainly not always the case in winters with strong SSWs during the cold period (Dec--Feb).

With respect to Figure 6 (new Fig. 7) and the corresponding paragraphs, we have decided to keep our analysis only on the 50 hPa level. The results of this altitude range are representative. They are mostly qualitatively in line with the results of nearby layers (for instance 30 hPa; not shown in the paper). Our findings are in qualitative agreement with the V_psc analyses in the lower stratosphere. Therefore, we have now cited the papers by Wohltmann et al. (2020) and Lawrence et al. (2020). The text in the paper is changed accordingly. In addition, we have mentioned the importance of the characteristics of the polar vortex and that it varies with height and in different Northern winters.

Furthermore, in relation to column ozone and its relationship to the LS vortex and low temperatures, because low column ozone is strongly spatially correlated with low LS temperatures by dynamical processes (see, e.g., discussion and references in SI of Manney et al, 2011) and the region of low temperatures in the LS is often not well-correlated with the lower stratospheric vortex (see, e.g., Manney et al, 1996, GRL; Mann et al, 2002, JGR; SI of Manney et al, 2011; Lawrence et al, 2015, ACP; and references in those latter two works), in absence of strong chemical depletion, the shape / extent of the region of low column ozone is not expected to be correlated with the shape / extent of the polar vortex. Therefore, in the maps of column ozone in Figure 8, we cannot judge whether the morphology of the low region is consistent with strong ozone loss unless we know how it relates to the morphology of the LS vortex. E.g., one of the most commonly used metrics for this is a contour or contours of potential vorticity (PV) on an isentropic surface somewhere in the LS (somewhere between about 450 and 550K is typical, commensurate with the approximate levels where ozone contributes most to the column), with value(s) such that it is (they are) in the region of strong PV gradients bounding the vortex.
(Similarly, the strength of PV gradients along the vortex edge is a common and valuable metric of vortex strength -- while maximum windspeeds at an appropriate level would also be informative of vortex strength, zonal mean winds are not.) PV on isentropic surfaces is readily available in all modern reanalyses including the ERA5 reanalysis used herein. Please note that Lawrence et al (2020; as I write this in late August, nearing completion of minor revisions for JGR, and available on ESSOAr since mid-June) in their section 3.4 (submitted version) provide a detailed discussion of LS vortex strength and chemical processing potential in 2019/2020 in comparison with the record from 1979 through 2019 and in particular compare with 1997, 2011, and 2016 (2016 is of interest because, while low temperatures and chemical processing ended much earlier, it still is the record cold winter in January and February, and had overall greater polar processing potential and more chemical loss than that in 1997; e.g., Manney and Lawrence, 2016, ACP; WMO 2019; and references therein), using widely accepted diagnostics / methods with uncertainties quantified. Because this paper for the AGU Special Collection provides an overview of the dynamical conditions during the winter, it is designed to provide this "foundational" material in a thorough way so that other papers on this winter can start with that comprehensive description as background. [While not as relevant to this paper and the 2019/2020 ozone loss, Lawrence et al (2020) also describe thoroughly in their Section 3.1 the evolution of 10hPa zonal mean zonal winds in 2019/2020 versus climatology (their Figure 1) and in the context of the time series since 1959 (their Figure 2).]
[The points above for which I have not provided references are well documented, e.g., in the past ~4 WMO Assessments and references on Arctic polar ozone and ozone loss therein. The relevant aspects of the meteorological situation are also discussed in Lawrence et al (2020), Manney et al (2020), and/or Wohltmann et al (2020).]

The aim of Figure 8 (new Fig. 9) is to indicate the differences of the TOC values with respect to the monthly means of the spring months (March and October). It should demonstrate that March 2020 showed the smallest TOC in comparison to March 1997 and March 2011 and that the Arctic region shows significantly lower TOC. You are right, based on our Figure 8 (now 9) we cannot decide whether the morphology of the region of low TOC is consistent with strong ozone loss unless we know how it relates to the morphology of the polar vortex in the lower stratosphere. Therefore, we checked the contours of PV at 475 K and 530 K. In Figure 2, we are now showing the results for the 475 K level (530 K shows basically the same result) with the same dates as given in Figure 1 (TOC). In more detail we have discussed now the strength of the polar vortex in the lower stratosphere and the chemical processing potential in 2019/2020. The corresponding papers are cited.

Other Scientific Comments / Questions:

What do the authors mean by "classification" in the title? The term is typically used for grouping and comparing things with similar characteristics, but it is unclear what sort of classification is being attempted herein.

Title has been changed, also due to the other comments and statements. The choice of word was not adequate, "comparison" would have been better.

Given the novelty of the ozone datasets used in this paper, I would like to see, especially in Section 2, more discussion of the TROPOMI data, the GTO-ECV data, and the relationships between them -- especially in conjunction with Figure 7, which compares time series from GTO-ECV in previous years with that from TROPOMMI in 2019/2020. What are expected biases between the two datasets? Are the discontinuities in the GTO-ECV data that might result in biases between some of the earlier years, or in non-physical trends?

Most information is provided by Coldewey-Egbers et al. (2015; 2020). Some more explanations have been added in the revised manuscript. Besides, in the revision we have now also mentioned the results of an initial comparison of TOCs from TROPOMI and OMI (not published so far); it indicates that TROPOMI TOCs are slightly smaller (about -1%) than OMI TOCs.

Page 2, line 12: How is "the expected ozone value in austral spring" defined? Do you mean the value that would be expected at that time of year with no chemical loss? If so, how is that determined?

Yes, it is the long-term (climatological) mean value, which is based on observations before 1980. The text is slightly changed ("climatological mean ozone value in austral spring, which was determined for the years before 1980 (…").

Page 2, line 25: Statements such as "...due to a strong and stable polar vortex in winter…" are too over-simplified, since there is (particularly in the much more dynamically active Arctic) no one-to-one relationship between vortex strength and temperature.

We agree that the statement here is oversimplified! The half sentence has been deleted to avoid misunderstanding.

Page 3, lines 6-8: What is this statement based on? Given the similarity of LS temperature evolution to that in 2016 and later 2011 as the winter progressed, and the large chemical ozone loss and resulting low column in 2011 (coupled with the knowledge that dynamical variations that can reduce column ozone play a significant role even in the coldest Arctic winters, and the large interannual variability making winters as cold as or colder than 2011 very likely "sometime"), I see nothing unexpected about what happened in 2020!

We agree, that the dynamical conditions found in NH winter/spring 2019/2020 are not unexpected. However, having on the one side a significantly reduced stratospheric chlorine content of about 15% (compared to the years around 2000) and on the other side detecting new record low TOC (below 220 DU) in the Arctic polar stratosphere over a longer period is still noteworthy. The text has been changed accordingly.

Page 5, line 29: I think "to large parts" is too weak here -- **no** chemical processing of any kind is needed to produce "mini-holes" as defined here.

We deleted the half-sentence containing this phrase as this is already addressed in the next sentence by referring to Millán and Manney (2017).

Page 6, lines 8--9: There is no reason to expect this, since (as per above discussion in the major points) the strength and coldness of the vortex are not necessarily closely correlated, and "an ozone-hole like pattern" in the sense of chemical loss driving the ozone morphology would never be expected before March because chemical loss is limited when the polar regions are in darkness.

You are right! The word “expected” has been changed to “observed”.

Page 6, lines 14--15: But you don’t even show the polar jet (which would have to be zonally resolved to show where the polar vortex was) in relation to the TOC, and show nothing about it at a level that is appropriate to determine where the LS vortex is. See comment on lack of definition of polar vortex in major comments above.

In Figure 3 (now new Fig. 4) we show the ERA5 monthly mean horizontal wind fields for January, February and March 2020. Maximum wind speeds are given. The figure indicates the persistence of the polar vortex (here at 10 hPa) in late winter and early spring 2020. In addition, the new Figure 2 shows PV at 475 K for particular days in March and early April (same dates as in Figure 1 showing TOC, to allow for a direct comparison).

Page 6, line 21: If you accept that this value is an accurate reflection of the similarity of chemical loss in the 2020 Arctic to that in the Antarctic, it would be helpful to point out that this is about 10% of the **smallest** Antarctic ozone hole area (in 2019) on record, and less than 5% of typical Antarctic values (see, e.g., Figure 1 in Wargan et al, 2020, accepted article online for JGR, https://doi.org/10.1002/essoar.10503445.1; and references therein).

Good point! We discuss briefly the comparison with the Antarctic ozone holes in 2016 and 2019, and we will bring up this point again later in the discussion. See the updated Figures 3, 6, 8, and 9.

Page 9, lines 4--8: Per previous comments, cold winters with substantial ozone loss can have weak polar vortices (e.g., 2004/2005), and not all winters with large ozone loss have unusually strong vortices. “Cold” and “strong” are not synonymous in relation to the polar vortex.

OK, we got your point and we agree in principle, but what we have written in these two sentences is not wrong. Therefore, we would like to keep this part as is.

Page 9, lines 17--24: This paragraph seems irrelevant to this paper. If indeed 2018/2019 needs to be mentioned, this could be done in a sentence by simply citing one or more of several papers that have been published on that winter (e.g., Butler, et al, QJRMS, 2020, and references therein).

We would like to keep this short paragraph as is because it shows nicely the differences. We already cited the paper by Lee and Butler (2020; published in Weather 10.1002/wea.3643) in this context. From our point of view this paper fits better than Butler et al. (2020) in QJRMS.

Page 9, lines 24--32: This paragraph seems largely irrelevant as well, and if the 2019 Antarctic ozone hole needs to be mentioned, that could be done by citing one or more of the several papers published on it (in particular the Wargan et al, 2020 paper mentioned above, which provides a detailed analysis of the dynamical and chemical mechanisms leading to the unusually small ozone hole in 2019; but there are also a couple of earlier references given therein).

We do not think that this comparison is irrelevant. With respect to our reply above (regarding the Wargan et al. (2020) paper, we slightly revised this paragraph.

Page 10, line 27 and line 30: “From our point of view” is a statement that would appropriately preface an opinion, not a scientific statement. If the statements following these can be backed up with evidence, there is no reason to use this language; if they cannot, they should not be made in a scientific paper.

The turn of phrase is deleted.

Page 10, lines 13--14: If this is intended to convey something beyond the point made in the previous sentence, a citation or some evidence should be given..

We assume that page 11 is meant here. The sentence is deleted.