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

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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

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Discussion paper

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: âĂć 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.

âĂć 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 km2 compared to 20 million km2). 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. âĂć 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 âĂć 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.

âĂć 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.

âĂć 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.

âĂć Page 1, line 16: Change to "The record low values were caused. . . " Wording is slightly changed.

âĂć 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.

âĂć Page 1, line 20: "in the context of" is probably better. We keep "in context with". From our point it is the correct wording.

âĂć Page 1, line 20: Replace "ozone-hole like features" simply by "cold winters" Wording is changed.

âĂć 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.

âĂć 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.

âĂć 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).

âĂć 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.

âĂć Page 2, line 7: Delete ”so-called” Done.

âĂć 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.

âĂć 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.

âĂć Page 3, line 3: Perhaps replace ”with respect to low TOC” by ”in reaching low ozone columns”? Text is slightly changed.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

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

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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).

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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).

âĂć 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.

âĂć 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).

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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!

âĂć 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.

âĂć Page 10, line 24: See comment page 10, line 10. Done!

âĂć Page 10, line 27. Please rephrase "atypical ozone hole" to something like "record low ozone values". Is changed!

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

âĂć 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.

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

âĂć Page 6, line 16: I don’t think ”outstanding” is the right word here. Maybe ”prominent” or ”remarkable”? Changed!

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

âĂć Page 8, line 28: ”Discussion” and not ”Discussions” Changed!

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

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

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

âĂć Page 11, line 20: I would say ”the most recent datasets” Changed!

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2020-746/acp-2020-746-AC2-supplement.pdf

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