acp-2020-746

Reply to the second review (revised version) of Ingo Wohltmann
by Dameris et al.

Thank you very much again for your very detailed remarks and the specific comments regarding our revised manuscript! We greatly acknowledge your work! Your statements and suggestions are very much appreciated. We have considered them in the second revised version of the paper.

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

Unfortunately, I think that this manuscript needs another revision. The "Major comment" and the comments under "General" were not fully addressed. In addition, the number of specific comments (new ones and old ones) adds up to more than 6 pages. I have also added 10 pages of suggestions to improve language, grammar etc.

We appreciate the reviewer's effort in providing comments and suggestions to improve the readability of the manuscript and will address them point by point in the following.

Title:
I am much happier with the title now. However, while I am not a native speaker, I have the impression a native speaker would not have phrased it like this. Just "Record low ozone values over the Arctic in boreal spring 2020" sounds better to me. Or may be "assessment of"? Or "Record low […] spring 2020 compared to other winters"?

The title has been changed accordingly.

General:
"I have to admit that I had more of a problem with the wording and readability in some places. 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."
I have the impression that you misunderstood my first comment in the "General" section or confused that with my comment under the section "Major comment". This comment was not about phrasing things carefully and the use of the term "ozone hole". It was about readability, conciseness, correct English language and grammar, awkward phrasing etc. Unfortunately, that means that this comment was not addressed adequately.
Maybe I did not express myself clearly enough. But in fact, your manuscript is very hard to read. I would try to improve language, grammar etc. in the manuscript in your own interest.
Even though I am not a native speaker, I can tell that there are many places of awkward writing, or not using the correct English phrases and words. In addition, the text is often very convoluted and confusing, and contains many repetitions and filler words. You probably could shorten the manuscript considerably without loss of information. As a result, you would get a more concise and readable text.
I have read the manuscript again and have compiled a long list of concrete suggestions how you could change the text (see last section **"New comments part 2: language"**). But this list is certainly not exhaustive.

Thank you very much for all your hard work on our manuscript. We very much hope that we got now all your points correctly. The manuscript has been revised according to your comments (see below our answers and the revised manuscript).

Reply to my major comment:
I appreciate your effort to phrase the script more carefully and I am happy that you acknowledge my

major comment in your reply. The manuscript has certainly improved.
Unfortunately, you contradict what you write in your reply as early as in the first sentence of your revised manuscript. You have rephrased "atypical ozone hole feature" just to "exceptional ozone hole-like feature". I am sorry to say that, but for the majority of the readers (and certainly for me) that will mean too much the same as before.
May be I can illustrate that with the following (tongue-in-cheek) example: Imagine you see a bike but write, "I see a car". Somebody responds: "But this is a bike!". You reply, "Ok, you are right. This is a car-like thing", because you are big fan of cars. Somebody else says, "Now, that you say it, it looks a little bit like a car, and it is a vehicle after all". I hope that makes clear where the problem is.
I will not go into detail, why I would not call it either an "ozone hole" or "ozone hole-like feature". You got very detailed comments on this, which I do not need to repeat here.
Unfortunately, this problem is not restricted to the first sentence of the paper. Further occurrences are at **page 1, line 10, page, 1, line 14, page 6, line 29, page 7, line 22, page 7, line 31, page 11, line 7, page 13, line 5 and page 15, line 2**.
I would suggest writing "ozone minimum" or, if you prefer "pronounced ozone minimum". That would be fine and still get to the point in a clear statement. If you insist on calling it an "ozone holelike feature", you unnecessarily provoke associations. As I already said in my original comments: Do not push the reader into a certain direction if you cannot back up this by facts.
Please understand that I am really not nitpicking here or trying to force my opinion on you. In my opinion, this is not something subjective, but really needs to be addressed. This was my only major comment, and I would be happy if this comment would be addressed not only reluctantly.
From your reply: "(ii) the shape of the region of low TOC looks "ozone-hole-like""
What do you mean by that? I think we agree that the area of the ozone minimum was only about 5% of the Antarctic ozone hole. Do you really refer to the geometrical shape here, i.e. the feature is roughly circular like the ozone hole? Does this have any relevance?

We got your major criticism and critical points (again) and revised the manuscript accordingly. Sorry for some of the misunderstandings. We hope that this version of our manuscript considers all your points adequately and that critical wording is now avoided. Furthermore, as the reviewer and another reviewer were not satisfied with some of the rephrasing in the revised manuscript (see upcoming Specific comments that are related to previous reviewer comments), we have completely deleted phrases such as "ozone hole-like" to avoid any misunderstandings.

Specific comments (I will refer to the original page and line numbers(!), so that you can quickly find the comment I am referring to)

Page 1, lines 11-12
You say this has been changed. You did not really. See major comment.

Has been changed to "show exceptionally low total ozone columns in the polar region…"

Page 1, line 14
You say "In addition, … sentence is added to make clear … differences in the respective area and time". In contrast to your statement in the reply, you discuss area here but not time. Did you forget to include this?

You are right. A sentence (at the end of the abstract) has been added.

Page 1, line 16
You say this has been changed. You did not really. See major comment again.

It has been changed now to “The record low total ozone columns were caused by a particularly stable polar vortex …”. See also our reply to your major comment.

Page 2, lines 5-10:
This is certainly nothing I would insist on, but I have the reader in mind here. This is one of quite a few places which are either phrased quite awkwardly or where you will make the reader wonder why you write this.

Thank you! We decided to keep this paragraph as is. We do not think that the written text is misleading or inaccurate … it is kind of a motivation for the study.

Page 3, lines 24-28:
I think I have to make some serious comments to Farah here (only joking). But seriously, you make the reader wonder again why you write this. At least the part with the CDO tools makes the impression as if it would have been a challenge for you to calculate a simple mean. I am quite confident that this was not the case. Maybe you can keep the information that the daily mean was based on hourly values, but everything else is detail that is not needed. You can thank the authors of the CDO tools in the acknowledgments.

As this was also noted in another review, we have shifted the detailed discussion to the data availability section.

Page 4, lines 26-28:
The reason for my comment was that this is rather hard to read. May be I did not express this clearly enough.

We tried to improve this paragraph; we slightly changed it now.

Page 4, line 28:
Ok, you did change this, but not to the established terminology, as requested.

It is changed now, as requested.

Page 5, line 2
Certainly, nothing I would insist on, but that does not change my opinion that the figure is not needed. You can see the circular shape easily in Figure 1.

Ok. We like the figure (Figure 4) since it shows the persistence of the polar vortex; and that the wind speeds are high. It is now somehow related to the new Figure 2, which shows PV (475 K).

Page 5, line 3
This is still misleading and ambiguous. The radiative cooling does not mainly result from the dynamical conditions. It is there because there is no sun in the polar night. It is present in every year. It is somewhat modulated by different amounts of radiatively active gases transported to the polar regions in different years, but I do not think you have this in mind here. Temperature variations are caused by adiabatic warming (and by the triggered additional(!) radiative cooling) by the BDC. I agree that you do not need to go into detail here, but the sentence should be correct. Suggestion: “The dynamical conditions in winter 2019/2020 with low planetary wave activity result in very low temperatures…” Only a slight change, but physically correct now.

Changed.

Figure 5 and 6: Please add…
Why don't you add Antarctic data for Figure 7 (was Figure 6), too, as for the other figures (and as suggested by me)?

See also the comment below (related to Page 5, lines 11-12). The used proxy (cumulative area) is used to point out the differences of the three Northern winters. From our point of view, there is no necessity to add the SH also in this figure as we focus on Arctic conditions. Furthermore, we have added and discussed SH conditions in detail in the text and the other figures to avoid any possible misunderstandings. Therefore, we decided not to show respective values for the Southern polar region. Furthermore, adding the SH in Figure 7 (old Fig. 6) would also expand the written text.

Figure 5: Can you really learn…
(Either relevant information is missing or there is a problem with what is shown in Figure 5, now Figure 6)
I think you have totally confused me here and I did not get it right in my original comment. Either, something must be wrong in the description in the figure caption or with the values shown the figure, or there is not enough information given to understand what you have done. This may carry over to the text on page 9 and 10 (new manuscript) and Table 1.

The lines in Figure 6 are indicating the minimum values in the polar cap region of each day. The dots show the minimum values in the polar cap region in the monthly mean field. The description of data (dots) presented in Figure 6 should be consistent with the description of values presented in Table 1.

What exactly do the dots show in Figure 5 (now Figure 6)? You write "minima of monthly mean temperature". But thinking about it, this would only give one dot per month (you have the monthly means of polar cap temperature for 30 years, and then take the minimum, which is one value).
Or do you mean that you first take the monthly mean at all grid points for every year, and then look for the minimum in the monthly mean field in the longitude-latitude grid? That would be consistent with the data points in the plot, but would be a rather convoluted quantity, where I am not sure if it would make sense to look at the quantity.

Yes, the latter is the case. You get one point per month for each of the years. Furthermore, the colored dots in Figure 6 (new) are showing the same temperature values, which are given in the table. This is also in line with the temperature data presented in Figure 5 (monthly means). The quantity of minimum monthly mean temperature is helpful because it shows first that the stratospheric temperatures in winter 2019/22020 were low. Second, we use this quantity because it indicates that the winter 2019/2020 was unusually cold over the complete winter season including early spring. In addition, with the help of the grey dots, it is demonstrated that 2019/2020 is in each winter month at the lower end of observed minimum temperatures. Furthermore, to look at the minimum monthly mean temperatures is useful because it indicates the differences between the winters with respect to the conditions for the formation of PSCs. Also, short time-scale and small-scale events are not overly highlighted in the monthly mean data.

E.g., values would by definition be higher as the daily
minima, and since the minimum is not always at the same position, values would smear out.

Correct. This smearing out however, is also useful a small-scale and temporally limited events will not be noted so much.

Or is it the monthly mean of the daily minimum values north of 50 degrees for each individual year?
In this case, I have the impression that the dots are placed far above the position where I would have expected them. Take for example the blue dot for January. It seems that the daily blue values (small

dots) for January are almost all placed below the big blue dot. But this can't be possible if the big blue dot is the mean of the small blue dots.

See comments above and below.

I think that some area with the daily range of values would be the quantity that the reader would expect here and would immediately understand (this is usually shown in the plots in the literature). If you first take the minimum of the monthly mean values, this is a rather convoluted quantity. It certainly does give you some impression about the range of the values, but is not as straightforward as it could be.

We very much hope that after our revision of the text it the description of the used data is better understandable now. Thanks for raising this point. However, we think that showing the variability of the minima of monthly mean data is not some "strange" quantity. Furthermore, these values relate to Fig. 5 (new) and Table 1.

Page 5, lines 11-12
I do not think that you understood the problem here. I think it is mathematically not correct and therefore confusing. A sum like the sum here is always an approximation of an integral over time, and will always yield a value similar to an integral if done correctly. If you would take values every 12 h instead of every 24 h, you would get approximately twice the value by simply summing up. If you however do consider the units, you would divide by 2 in the case of 12-hourly values. That would give a similar value as before. The point is you are already implicitly assuming a value of "days" here, because you sum up values given every 24 hours. You probably do not want to depend your result on the number of values that you sum up in a given time period.

See also the comment below (Page 6, line 22): We have understood the problem (i.e. your point!). It is mathematically correct and also the unit for the cumulative area, i.e. "10^{12} m^2". We have only calculated the daily (mean) area below 195 K at 50 hPa for each day, and finally we have added the daily values of the complete winter season (simply an addition of daily areas below 195 K). It is a simple proxy created for the comparison of the discussed three winters. The numbers for the SH are significantly larger and for instance scale of the cumulative values would be difficult to put them into relation with the "real world".

But we have expanded the text to provide a precise definition of what this quantity is supposed to show. Hence, there is no mathematical inconsistency. We agree that using 12h data would roughly double the values, but if defined correctly (as sum over the 12h values) and applied correctly this would be the case for all of the years shown, which would in turn allow for an intercomparison again as the relation of the quantities for the various years would not change.

Figure 1: Add contours for the 220 DU contour and the vortex edge. These are things that are really hard to see in a colored contour plot. And the 220 DU contour is really central for the discussion in your paper.
You say this is not necessary in your reply. I would not have made this comment if I would not have large problems to discern things here, and I think many people would share this view. This is not only a continuous color scale, but it also lacks contrast in the relevant range. In this context, "dark purple" is subjective at the least. And it is easy to add a line contour to the plot (say, in white). This should be feasible and easy to do with whatever graphical package you are working with. I do not think I am asking for too much work here. Same applies for the vortex edge. The new Figure 2 helps, and this shows that it should be easy to add e.g. the 36 PVU contour to Figure 1 (in a color different from the 220 DU contour). You could omit the new Figure 2 then.

We have highlighted data below 220 DU in the revised Figure 1. The area below 220 DU is now indicated in white. (To have consistent depictions, Figure 9 has been also updated.) We do not want to combine the Figures 1 and 2, to avoid overloading the figures with too much information. With respect to Figure 2, we would like to

keep it as it, because it also contains temperature contours (in red).

Figure 6
Sorry, but I do not see the qualitative difference to the other figures (Figures 3, 6, 8 in the new manuscript). I cannot follow your arguments. Again, I do not think I am asking for too much work here.

As said in our reply to your first review, this figure has been created only to demonstrate the differences of the discussed three winter seasons with low TOC. Added are also the values of the "warm" winter 2018/2019, which indicates that the "cold" areas (below 195 K) are very small (in this case only some view days around mid-December 2018). Our Figure 7 (new) indicates that the most obvious difference (for instance differences between the first half of December and first half of January between 2010/11 and 2019/20), which support the results of the other analyses discussed in the paper. We would like the figure as is (without showing the "range of variability") and hope that you will agree.

Page 6, line 22
No, they are not, see page 5, lines 11-12.

In your first review you said that "The numbers for the vortex area are a little bit unintuitive." We think that the given number are correct and also that the unit is correct. The cumulative value should be taken as a proxy to document the differences between the three NH winter seasons. We have given a clear definition in the text. As per that definition, we calculated the daily (mean) areas below 195 K, and we have summed the daily areas (total sum of all daily areas), which at the end provide clear differences between these three winters. And therefore, the correct unit is "10^{12} m^2". We agree that using another definition as integration over time, which would yield units of area X time would be possible as well, however the units might sound strange as we talk about an area.

Page 8, lines 11-15
Mostly OK. Page 10, line 28 of the revised manuscript: "denitrification was much stronger than in 2011". That is not was the text says in Manney et al., 2020. My interpretation of the text is that 2011 was comparable in magnitude. Please change accordingly.

Has been changed ("much stronger" now reads "stronger"); see also below. Please also note, that we talk here about 2011 and not 2016.

Page 9, lines 12-14
OK in the revised manuscript. But I do not understand your reply "The statement … was very clear that the spring 1997 was the coldest …" This was not clear at all in the original manuscript. There is no obvious and straightforward way to define the "coldness" of a complete winter. You can take minimum temperatures, VPSC, North Pole temperatures etc. and this will give different results.

Ok. Maybe our reply was misleading, sorry. Thank you for your comment.

Page 10, line 10
OK, but I do not think that 15% is "clearly higher". It is only 15%. It would suffice to write "higher".

Deleted.

Page 11, lines 11-14
I certainly do not mind. It is your freedom as an author. My concern is just how the readers conceive this. This is all about readability and conciseness.

Ok, but we think that the given statement is correct and that it will not lead to wrong interpretation. Therefore, we would keep it as is.

Page 11, lines 17-19 etc.
You say you deleted the expression "ozone hole". You did not. See major comment.

It was changed to ozone hole-like in the revised version. But as stated in our reply to the major comment, we have now deleted the use of the phrases "ozone hole" and "ozone hole-like" entirely when referring to the NH.

Technical corrections

Page 8, line 1 and 3 (now page 10, lines 12 and 14)
You did not change all occurrences. You can also just write "(PSC type 2; see" for "(PSC type 2, ICEPSC; see".

Changed now in the complete manuscript.

New comments (part 1: related to content, page and line numbers revised manuscript)

Page 3, lines 13-14:
"However TOC below 220 DU have not been observed in these two years." This is not true. Have a look at your Figure 8. Please phrase correctly, e.g. add "for an extended time period in spring".

Changed.

Page 5, line 19:
"In connection with Figure 1". Sorry, I cannot follow you. The sentence would read totally correct for me if you would delete this part. This seems to be superfluous and not correct.

Deleted.

Page 6, line 33 to page 7, line 2:
It is not clear what you compare to, add "than in the Arctic" or similar.

Changed.

Page 7, line 25:
"not shown". This is not true anymore in the revised version.

"(not shown)" has been deleted.

Page 9, lines 13-15:
Looking at the plot, this is not really true. Daily minimum values for 2010/2011 and 2019/2020 are very similar in December and, to a lesser extent, they are also quite similar in January.

Statement has been slightly changed. It should be correct now.

Page 10, lines 6-7:
That is possibly misleading. That depends on what quantity you are looking at. Vortex averaged loss was not so different, at least in my study. The maximum loss made the difference.

Wording has been changed ("maximum" instead of "chemical" ozone loss).

Page 10, lines 19-20:
Delete. There is an almost identical sentence only a few lines before (page 9, lines 13-15). In addition, the statement may not be quite correct (see there).

Correct. Sentence has been deleted here.

Page 10, line 25:
"due to heterogeneous reaction". I do not think this is true. Don't you just mean "by uptake of HNO3"?

You are right! Changed.

Page 10, line 28:
See "specific comments, page 8, line 11-15". I do not think this statement is correct.

This statement has been slightly changed. It should be correct now. See previous comment.

Page 11, lines 16-27:
I found the information content of these lines to be small. This is well known from the literature and textbooks. Not that it would be wrong to mention this here, and it is all correct, but could you try and come a little bit more quickly to the point here?

This paragraph has been slightly shortened.

Page 12, lines 3-7:
How do you compare 30 hPa North Pole temperatures from FU Berlin to the temperature metrics used in your study? These are not the same quantities, and 2019/2020 is not included in the FU time series. E.g., North Pole temperatures could be hampered by the fact that the polar vortex is not always situated over the North Pole. Wouldn't it be better to base your statement that "2019/2020 is outstanding … since … 1950s" on a comparison of the same quantity?

Your point is correct! The discussed quantities are not the same. Therefore, we have mentioned very clearly that a range of uncertainties is given with respect to the classification of the winter 2019/2020. The given statements are vague. In this case, the results of Lawrence et al. (2020) also helped, which are based on JRA-55. This paragraph has been slightly changed (wording).

Page 12, line 17 to Page 13, line 6:
Again, it would help if you would come to the point here more quickly. It is totally ok to discuss this here, but this is a rather long and meandering discussion. The main topic of your paper is the 2019/2020 winter. While it is ok to compare to other winters, I do not really see the relevance of all the details given here. Where does this aim at? This seems like a repetition of facts from other sources.

These two paragraphs have been significantly shortened.

Page 13, line 11:
"although the dynamic features are similar". What exactly do you mean by dynamic features here? I think this statement is too subjective and should be deleted.

Deleted.

Page 13, lines 16-18:
This has already been discussed. Delete. In addition, I think this statement is not really correct, see comments I have already made.

We have not deleted this part; we have modified the statement, so that it is also correct.

Page 13, lines 20-24:
Delete. You have already discussed that in preceding section in detail. You only repeat thinks here you have already said.

We would like to keep this part in the discussion section. The sentence has been slightly changed (according to your suggestion given below).

Page 13, lines 24-26:
Delete sentence starting with "It is obvious…" You have stated exactly the same a few lines above (lines 16-18). Lines 16-18 were already a repetition. In addition, the statement is not correct.

Deleted.

Page 13, line 27-30:
Delete. You have already said this and only repeat things here.

Parts of the sentence have been deleted.

Page 13, line 33:
Probably you mean "in particular" and not "especially". However, that does not really fit either. You could delete "especially". However, here is a deeper problem. In fact, you are speculating here and cannot really back this up by facts. I would suggest either to cite a reference here or to replace "especially" by something like "possibly" or "probably".

Changed ("was especially caused" to "might have been caused").

Page 14, line 1-2:
This slipped through my attention in the first review. Again, this cannot really be backed up by facts. While it is plausible, you do not do an analysis of dynamical and chemical contributions to the observed ozone column. You need to phrase that more carefully.

We have added "might have" to keep the sentence.

Page 14, line 7:
Delete "only". I think I already made a comment that you can also learn something from the past here. Deleted.

Page 14, line 8:
Delete “more or less”. Either it is cooling or not. This is not really a scientific statement.

Deleted.

Page 29, line 4:
As said in the specific comments, something is wrong with the “minimum of the monthly mean temperatures”, or there is not enough information given.

As said already above, the lines in Figure 6 are indicating the minimum values in the polar cap region of each day. The dots are given the minimum values in the polar cap region in the monthly mean field. It should be now clear in the text and caption. See our previous comments regarding minima of monthly mean temperature data.

New comments (part 2: language, grammar etc., page and line numbers revised manuscript)

Page 1, lines 23-24:
“emphasizes the noteworthiness” does not sound like good English to me. “highlights” or “underlines” is probably better. “noteworthiness” sounds strange here. Maybe “highlights the unique evolution of…” or something similar.

Changed.

Page 2, line 4:
Delete “especially”. You probably mean, “In particular, unusually low ozone…”.

Changed.

Page 2, line 7:
Suggestion “…are found in the stratospheric ozone layer…”.

Changed.

Page 2, line 21, Page 13, line 24, Page 29, line 11:
Change “ICE-PSC” to “ice PSC”.

Changed all.

Page 2, line 23:
Replace “for instance chlorine” by “chlorine and bromine”. It is only these two species.

Changed.

Page 2, line 25:
Would help to replace “ozone depletion begins” by “ozone is depleted by catalytic photochemical cycles” or something similar. Only a few words more, but some more relevant information.

Changed.

Page 2, line 27-30:
This is a very long sentence and hard to read. Maybe you could just shorten "… in response to the Montreal protocol, 1987, and its amendments…"

Slightly shortened.

Page 2, line 31:
"atmospheric content" seems not the right phrase for me. I suggest "atmospheric burden" or "atmospheric concentrations" etc.

Changed to "burden".

Page 3, line 5:
Change "heavy ozone depletion" to "severe ozone depletion".

Changed.

Page 3, line 7:
Delete "clearly".

Deleted.

Page 3, line 10:
You could delete "as we will see in the upcoming analysis".

We would like to keep this sentence as otherwise this statement seems is not backed up.

Page 3, lines 10-13:
The sentence is somewhat convoluted and does not read well. Suggestion: "Comparable dynamical conditions in the Northern stratosphere in spring were noted in the literature for 1997 (references…) and 2011 (references…)".

Changed.

Page 3, lines 14-16:
This is phrased awkwardly. Suggestion "Although the dynamical conditions in winter and spring 2019/2020 were unusual, they are in the expected range of stratospheric dynamical fluctuations in Arctic winter and early spring (e.g., Langematz et al., 2014).".

Changed ("expected" to "natural").

Page 3, lines 28-29:
Again, this is phrased awkwardly. Suggestion: Replace "It allows an evaluation of the current situation by the comparison with similar dynamical conditions in Arctic spring of other years" by "We compare the current winter to winters with similar dynamical conditions in Arctic spring…".

Changed.

Page 3, line 31:
Replace “far away from the usually observed Antarctic ozone hole” by “far removed from the conditions usually observed in the Antarctic ozone hole”.

Changed.

Page 4, line 10:
Delete “For our investigations”.

Deleted.

Page 4, line 17:
“The focus is laid on stratospheric zonal winds, polar temperatures and potential vorticity (PV).” I think you do not need that sentence.

Sentence has been deleted.

Page 5, line 18:
“turned out to be persistent” is phrased awkwardly. Suggestion: “Arctic winter and early spring 2019/2020 showed a persistent stratospheric polar vortex with strong zonal winds from midDecember until early April.”.

Changed.

Page 5, line 20-21:
Phrased awkwardly. Suggestion: replace “representing the dynamic state of the lower stratosphere with respect to the position and strength of the polar vortex.” by “and shows the position and strength of the polar vortex.”.

Changed.

Page 5, line 21:
Change “PV-gradients” to “PV gradients”.

Changed.

Page 5, line 24:
Phrased awkwardly. Suggestion: “Figure 3 shows strong zonal mean zonal wind speeds at 60°N, 10 hPa (about 30 km altitude) in the ERA5 data (magenta line and dots in the figure), …”.

Changed.

Page 5, line 25:
Replace “which are high with respect to” by “which are higher than”.

Changed.

Page 5, line 26-27:
Delete “very much”. Do not exaggerate.

Deleted.

Page 5, line 27:
Delete “the respective”.

Deleted.

Page 5, line 31 to page 6, line 3:
I do not think that this sentence, which is very hard to read, does convey any information that is useful in the context of this paper. Please delete.

This sentence (with the explanations) has been shortened. We think that a reference is needed here.

Page 6, line 6:
Delete “clearly”.

Deleted.

Page 6, line 10:
Change “lower stratosphere temperatures” to “lower stratospheric temperatures”.

Changed.

Page 6, line 11:
The 50 hPa level is not a height range. It would be better to say, “which is inside the height range”. I would also say “important for ozone depletion” and not “of vital importance for ozone depletion”. Better English in my opinion.

Changed.

Page 6, line 14-15:
You can delete “(i.e. the Cl activation threshold at this altitude, see for instance Figure 4-1 of Chapter 4 in WMO, 2018)” now. You now have that in the introduction. That was exactly the reason why I was asking to write it in the introduction. You can streamline the text here.

Deleted.

Page 6, line 21-22:
That could be shorter now: “This led to conditions allowing the formation of NAT-PSCs at 50 hPa for about 3.5 months (see Figures 6 and 7).” You introduce the abbreviation in the introduction.

Changed.

Page 6, lines 24-26:
You can shorten this significantly now. Again, that is why I asked for this in the introduction.

A sentence has been deleted.

Page 6, lines 31-32:
This is phrased awkwardly. I think you should delete “exemplarily” and start with “In Figure 3 and 6, corresponding…”.

This has been changed to: “In Figures 3 and 6, corresponding…”

Page 7, line 26:
You could delete “indeed”.

Deleted.

Page 8, lines 6-7:
Shorter: “The maximum area with TOC below 220 DU was 0.9 million km2 (= 0.9·1012 m2) on March 12 (Figure 1).”.

Changed.

Page 8, line 7:
You very probably mean “polar vortex area” and not “polar vortex”.

Changed.

Page 8, lines 7-8:
Phrased awkwardly. Suggestion: “This is in the order of 4% of the polar vortex area at the 475 K isentropic surface inside the 36 PVU contour (e.g., Wohltmann et al., 2020).”.

Changed.

Page 8, line 11:
Change to “minimum TOC is clearly higher” (singular).

Changed.

Page 8, lines 18-19:
“spring” is mentioned twice in the sentence. You could omit “in spring”.

Changed.

Page 8, lines 24-26:
This is an overly complicated sentence for a simple fact. Much shorter without loss of information: “The evolution of the polar vortex at 10 hPa and the minimum temperatures at 50 hPa are commonly used to examine the dynamical state of the stratosphere (see e.g. Lawrence et al., 2020).”.

Changed.

Page 9, line 4:
It is not clear what “see below” refers to.

Changed.

Page 9, line 5:
Probably a "," would help: "…are similar, reaching…".

"," added.

Page 9, lines 6-9:
This is a very long sentence. It would help to split it into two and to shorten it.

Changed.

Page 9, line 10:
Delete "the respective".

Deleted.

Page 9, line 11-12:
Suggestion: "The temporal evolution of the observed daily minimum temperatures at 50 hPa is shown in Figure 6.".

Changed.

Page 9, lines 12-13:
Phrased awkwardly. Suggestion: "The minimum temperatures were below the threshold temperature for the formation of NAT PSCs (195 K) in February and March of all three years.".

Changed.

Page 9, line 13-15:
Phrased awkwardly. Suggestion: "Minimum temperatures at 50 hPa in December 2019 and January 2020 were lower than the minimum temperatures in December/January 1996/1997 and December/January 2010/2011 most of the time." (but see comments above that this not really true).

The sentence has been rephrased.

Page 9, line 16:
What do you mean by "indicating the characteristic of"? I am clueless.

"the characteristic of" deleted.

Page 10, line 1:
It must be "of the two winters 1996/1997 and 2010/2011". "to" makes no sense here.

Changed.

Page 10, line 2:
You probably mean "in particular" and not "especially".

Changed.

Page 10, line 3:
Phrased awkwardly. Suggestion: “Severe chemical loss was observed in spring 2020 (Manney et al., 2020).”.

Changed to “Severe chemical ozone loss was observed in spring 2011 (Manney et al., 2011).”

Page 10, line 4:
Change “which was mentioned in” to “according to” (or just cite the studies at the end of sentence and skip this part of the sentence).

Changed.

Page 10, line 13:
Change “was determined with” simply to “was”.

Changed.

Page 10, line 17 and 18:
You could easily delete “To summarize” and “Our analyses show”.

We think the introductory phrase “To summarize” helps the reader to understand why some results are repeated here. The second phrase has been deleted.

Page 10, line 20:
You could delete “It is worth mentioning that”.

Deleted.

Page 10, line 22:
Delete “in” at start of line. Seems to make no sense here.

Deleted.

Page 10, line 22:
You could delete “Having”.

Changed.

Page 10, line 23:
“more efficient”. Compared to what? You could just delete “more”.

Changed.

Page 10, line 23:
Had to think a moment about what you mean by “they”. I would just write “PSCs”.

Changed.

Page 10, line 24:
You probably mean “in particular” and not “especially”.

Changed.

Page 10, line 26:
Suggestion: “enabled a period of ozone depletion that was longer than usual”.

Changed.

Page 10, line 27:
Unnecessarily complicated sentence (and not phrased very well). Just start with “Manney et al. (2020) analyzed…”.

Sentence has been shortened.

Page 10, line 30:
I was confused here over reading “July” and “June” for the Northern hemisphere, until I realized that you are talking about the seasonal evolution of ozone over the period of a complete year. Can you try to phrase that a little bit differently?

“temporal” has been changed to “seasonal”.

Page 10, lines 30-32:
A very long sentence. Please split into two sentences.

Structure of the sentence has been slightly changed, i.e. split the sentence into two.

Page 10, line 32 to page 11, line 2:
Unnecessarily long and complicated sentence.
Suggestion: “Typical features of a strong polar vortex can be observed in February 1997 and February 2011, with low TOC values in the vortex and relatively high TOC values in the collar region of the polar vortex (not shown)”.

Changed.

Page 11, lines 3-6:
Another very long sentence. You could delete “An important point to be mentioned is that”. You could also delete “and which were then followed by a typical chemical ozone loss”. What is a typical loss? Does this add any important information?

The sentence has been shortened as suggested.

Page 11, line 7:
“shows” and not “is showing” is probably the correct tense.

Changed.

Page 11, line 8:
Delete “clearly”. Deleted.

Page 11, lines 11-14:
Again, a very long sentence. Can you split this in two?

Structure of the sentence has been slightly changed, but it has not been split in two parts.

Page 11, lines 28-30:
Phrased awkwardly. Suggestion: “The winter 2019/2020 shows an extraordinary dynamical situation with a persistent strong and cold polar vortex over the complete winter season, when compared to the last four decades (the period of the ERA5 dataset).”

Changed.

Page 11, line 31:
You could delete “our dynamical analyses based on”.

Not deleted, see comment above.

Page 11, line 31:
It is not clear what “*the* historical data set” refers to. Better, “An analysis of historical data was…”.

Changed.

Page 12, line 5:
Phrased awkwardly. Suggestion: “Temperatures in January 1997 were near the climatological mean value.”.

Changed.

Page 12, lines 5-6:
You could delete “In combination with our research results”.

Deleted.

Page 12, line 7:
Delete “for instance”.

Deleted.

Page 12, line 10:
Delete “their investigations of”.

Changed.

Page 12, lines 11-12:
Delete “(i.e. in the Berlin analysis it ranked second after 2019/2020)”. You stated the same a few lines before.

Changed.

Page 12, lines 12:
Phrased awkwardly. Change “(i.e. in the Berlin analysis it indicated as a cold winter, but not extraordinary)” to “(indicated as a moderately cold winter in the Berlin analysis)”.

Changed.

Page 12, lines 12:
Change “turned out to be” to “were”.

Changed.

Page 12, lines 13-15:
Phrased awkwardly. Suggestion: “The slightly different results for the record years indicate that results depend on the considered quantity” (or “considered meteorological variable”).

Changed.

Page 12, line 15:
Delete “Further it is clear that the”.

“Further” was deleted.

Page 12, lines 15-16:
Phrased awkwardly. Suggestion: “Nevertheless, it is obvious that the winter 2019/2020 was one of the coldest winters in the last 65 years, and that it showed an exceptionally strong and stable polar vortex.”.

Changed.

Page 12, line 17:
Delete “We note that”.

Changed.

Page 12, lines 17-19:
I would suggest splitting this long sentence into two.

Rephrased.

Page 12, line 23:
Change “(around the long-term mean)” to ”(similar to the long-term mean)”.

Changed.

Page 12, line 25:
Phrased awkwardly. Suggestion: “The Southern hemisphere spring seasons of 2002 and 2019 provide two additional examples for the importance of stratospheric dynamics in the development of low ozone columns”.

Changed.

Page 12, line 28:
"The other example happened in September 2019." Phrased awkwardly. Just delete and continue with "In 2019, the polar vortex was…".

Changed.

Page 12, line 29:
Throughout the paper, you talk of "zonal mean zonal wind", but here you say "zonal mean west wind".

Changed.

Page 12, line 30:
Change "happened" to "was observed".

Deleted.

Page 12, line 32:
Change "Afterwards" to "After this event,".

Deleted.

Page 13, line 8-9:
Change "lower stratosphere temperatures" to "lower stratospheric temperatures".

Changed.

Page 13, line 10:
Delete "that". Grammatical error.

Deleted.

Page 13, line 11-12:
You could split this sentence into two.

Changed.

Page 13, line 13-14:
I would just say, "…were similar to the conditions in early spring…".

Changed.

Page 13, line 14:
Delete "Further". Or write at least "Furthermore".

"Further" has been deleted.

Page 13, line 15:
I would suggest “Minimum TOC values were below 220 DU for several days in March 2020, although…”

Changed. But we also added that 220 DU were not reached in March 1997 and 2011.

Page 13, line 16:
Delete “clearly”.

Deleted.

Page 13, lines 16-18:
Delete “Our comparisons show that especially”.

Deleted.

Page 13, lines 18-19:
Change “all the time below 195 K” to “below 195 K most of the time”.

The sentence has been rephrased.

Page 13, line 19:
Add “,” following “In this context”.

Added.

Page 13, line 20:
Delete “Our analyses show that”.

Deleted.

Page 13, line 30:
It is not clear what “However” does refer to. Delete.

Deleted.

Page 14, line 3:
Phrased awkwardly. Suggestion: “Record low stratospheric ozone values over the Arctic in 2020 are not an unequivocal result of climate change.”

Changed.

Page 14, line 4:
Phrased awkwardly. Suggestion: “The dynamical situations in February and March of 1997, 2011 and 2020 were similar.”

Changed.

Page 14, line 4:
Delete “Beyond that”. It is not clear what you refer to and it is confusing to read.

Deleted.

Page 14, lines 6-7:
Awkward phrasing. “is possible” sounds like that would be a surprise, but we expect cold winters from time to time. Suggestion: “The NH winter 2019/2020 is a perfect showcase for a Northern winter with low planetary wave activity, a strong and stable vortex and low temperatures.” (replaced “less” by “low”, since you do not compare anything here).

Changed.

Page 14, line 12:
Phrased awkwardly. Suggestion “… showed that cold Arctic winters may possibly get colder in the future.”

Changed.

Page 14, line 18:
Delete “respective”.

Deleted.

Page 14, lines 23-25:
I think that I have already made a comment that this seems strangely out of context. I would really suggest deleting this.

The question of the possible role of the unexpected CFC-11 emissions was raised (e.g., by colleagues and journalists) with respect to the record low Arctic TOC in spring 2020. For this reason, we would like to keep this statement in the manuscript.

Page 14, line 27:
Why did you add “consistent” to my suggestion? That sounds strange. I would delete that. Nobody would expect an inconsistent description here.

Word deleted.

Page 14, line 29:
“in the vicinity or below 220 DU”. Phrased awkwardly. Suggestion: “Record low TOC values of 220 DU and less were detected…”

Changed (around 220 DU or less).

Page 14, line 29:
It has to be “large” for “larger” and “extended” for “longer”. You do not compare anything here.

Changed.

Page 14, line 30:
Phrased awkwardly. Suggestion: "2019/2020 is compared…" or "The situation in 2019/2020 is compared…"

Changed.

Page 14, line 31 to Page 15, line 2:
Awkward phrasing. Suggestion: "We have used recent meteorological data from ERA5 and recent total ozone column data of GTO-ECV (based on…) in combination with TROPOMI onboard Sentinel-5P."

Sentences has been changed.

Page 15, lines 2-5:
Extremely long sentence. Please split into two.

We keep it as is. The text between the hyphens should be seen separately. See next comment: the brackets have been deleted.

Page 15, line 4:
It seems the hyphen is not at the correct position. It should be "…mid-October) – and TOC…"

The brackets have been deleted and the sentence was slightly rephrased

Page 15, line 5:
"were not observed before over a period of 5 weeks". It should become clearer that you refer to past years and not to the same winter here.

Change "before" to "in previous years".

Page 15, line 5-6:
You could delete "The results of our study pointed out that".

Changed.

Page 15, Line 7:
"supporting". Is this really what you want to say? You probably want to express that ozone depletion was more severe than in other years. Maybe "leading to enhanced ozone depletion compared to other years" or similar.

Changed.

Page 15, line 8-9:
Shorter and not so convoluted: "The special dynamical situation in winter 2019/2020 is the cause for the significant reduction of the TOC in spring 2020 …". This removes also the ambiguity what "despite" refers to.

Changed.

Page 15, lines 11-14:
Very long and complicated sentence. Suggestion: "Numerous studies of the 2019/2020 winter season can be found in a special issue of Geophysical Research Letters and Journal of Geophysical Research – Atmosphere (e.g., Manney et al., 2020; Wohltmann et al., 2020, Lawrence et al., 2020; Grooß and Müller, 2020)." Note that I removed statements that are only true at the moment of your writing. Soon, nobody will care that some of the studies were not published at the time of writing.

Changed.

Page 15, lines 18-20:
Phrased so awkwardly that I had to read it several times to get the meaning. Suggestion: "However, in winters with a cold and stable polar vortex, a persistent region of low TOC might also develop in the Northern hemisphere in the future again."

Changed.

Page 15, line 21:
You probably do not mean "documented" but "considered".

Wording is changed ("watched carefully")

Page 15, line 22:
"enable well founded scientific explanations of special ozone features" is phrased awkwardly. Suggestion: "Continued monitoring of ozone with a suite of instruments will be key to understand the future development of Arctic ozone" or similar…
However, this rephrasing would also mean to change the following sentences a bit.

Changed.

Page 25, lines 6-7:
Suggestion: "The grey line is highlighting the 36 PVU contour."

Changed.

Page 26, line 6, Page 29, line 7, Page 31, line 14:
I would move "(attention: the respective data are shifted by six months)" to a separate sentence. Delete "attention" and write "Southern hemisphere data are shifted by six months."

Changed.

Page 29, line 10:
Change "broken" to "dashed".

Changed.

Page 31, line 11:
"Note the …." I would not write that in the figure caption without more context. I think this belongs into the main text.

Changed.

acp-2020-746

Reply to the review of the revised manuscript of Gloria L. Manney
by Dameris et al.

Thank you very much for your remarks and the specific comments regarding our revised manuscript. Your statements and suggestions are very much appreciated. We have considered most of them in the secondly revised version of the paper.

In the following the points, which you raised, are displayed in black and our responses are given in blue.

Overview Comments:
This revised paper is, for the most part, scientifically sound, and provides a different view using new datasets of the evolution of total ozone column (TOC) in 2019/2020; the material is thus ultimately appropriate for publication in ACP. In the revision, the authors have gone a long way towards addressing several serious concerns in the reviews of and SCs on the initial ACPD version, but they have not entirely succeeded in some cases, as detailed below. In addition, there are some changes that should be made to the figures and text that should not be difficult or consuming of time or other resources but would have great value in making the main points of the paper more clear. I recommend publication in ACP if these concerns are addressed.
I do find the overall focus (what you might call "balance") of material in this paper somewhat problematic, because two already published papers (the Lawrence et al JGR paper and the Wohltmann et al GRL paper; these are augmented by complementary information in the Manney et al 2020 GRL paper) describe the meteorological situation in the lower stratosphere (where most relevant to chemical ozone loss) and its relationship to other Arctic winter/spring seasons with extremely strong and/or cold polar vortices much more completely that does this paper. All of these papers detail the comparison with 2011; in addition, Lawrence et al compare with 1997, and all of these papers also compare with 2016, which is notable for having an extended period, primarily in Jan/Feb, with the record cold of any Arctic winter in the past approximately 70 years (e.g., Manney and Lawrence, 2016, ACP; Matthias et al, 2016, GRL), the most denitrification and dehydration on record, and chemical ozone loss as rapid as or more rapid than that in 2011 (or 2020) until the early vortex breakup in March (Manney and Lawrence, 2016, ACP; Khosrawi et al., 2017, ACP; Johansson et al., 2019, ACP). In contrast, while there is some material on TOC in the papers published so far on the 2019/2020 Arctic winter (including comparisons of OMI and ground-based data with climatology in Bernhard et al, 2020, revised and resubmitted with very minor revisions for GRL, original ESSOAr link: https://doi.org/10.1002/essoar.10504414.1, a paper focusing primarily on the associated UV anomalies; and analysis including TOC comparisons with other winters using the CAMS reanalysis in Inness et al, 2020, https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2020JD033563), the current manuscript does offer a different view of this with long-term comparisons using different, and relatively new, datasets. Thus I would strongly encourage the authors to rebalance the paper so as to focus less on the description of the meteorology (which could, for the most part, be described sufficiently by citing published material that was readily available well before this paper was initially submitted) and more on the impacts of that on TOC and clearly detailing how those impacts are seen in the TOC data that they show. I do, of course, realize that, with the special issue, previously published material is a "moving target" and with a special issue (which I like to think of publications in other journals as informally contributing to), a certain amount of approximate duplication is inevitable -- so I hope it is clear that I am not asking the authors to remove all of the dynamical material (even where I do point out below in specific comments that a paper has covered the point already), just to cite the published work appropriately (which is already much improved, though not perfect, in the revised paper) and to focus less on that and more on the parts of this manuscript that are unique.

At the beginning of this reply to your review, we would like to explain again the circumstances for the preparation of our paper. Please also take notice of our recent answers to the different comments and reviews.

When we started to write this paper (beginning of April 2020) we did not have notice of the mentioned papers and the special issue. Our paper was submitted first on June 9 (acp-2020-573) This version was rejected due to a misunderstanding with respect to the used data material. After this misunderstanding was resolved, a re-submission of our manuscript was published in ACPD on July, 21 (acp-2020-746). As we were trying to retain the initial publication record (i.e. keep the version as acp-2020-573) it took quite some time between the initial submission and the re-submission (e.g. because of absences). Unfortunately, in the end we were told, that this is technically not possible and hence the re-submission has a new version record (acp-2020-746). In the meantime, we realized that there are many other papers on the way with respect to the winter 2019/2020. We have replied to the referees and to the authors of the given short comments, where we did describe our situation. In the revised version of our paper, we have discussed in detail the results of the newly published and submitted papers, in particular the mentioned studies of Manney et al. (2020), Wohltmann et al. (2020) and Lawrence et al. (2020). In addition, several other papers have been included in the revision to acknowledge the work of others in this field of research.

Thank you for raising the point that " previously published material is a 'moving target' ". This is the reason, why we would not like to change the focus of our study. As described above, the structure and focus of this paper had been chosen before other material regarding this topic was published. The study is concentrating on the dynamics of the stratosphere and its role for the reduction of the ozone layer. In combination with our ozone data sets (GTO-ECV in combination with TROPOMI), our study provides additional information. Based on the comments by Ingo Wohltmann (2 reviews), by another anonymous referee and your comments and this review, we think that in the secondly revised version a good "balance" is found with respect to the previously published papers and our investigations. We are not only giving the respective references, but also discuss the results of these paper. Our analyses provide some additional information, which should be discussed together with the previously published paper. For instance, it is also useful to demonstrate that independent analyses of the same atmospheric situation provide very similar results. On the other hand, we have reduced some passages of our manuscript, which were presented in the ACPD version (and the first revision). We hope that you can agree with our changes (see below and in the 2nd revised version – attached), although not all points and suggestions raised by you are fully implemented.

In the next section, I make comments on some points raised in the initial reviews and SCs that I don't think the current revision completely addresses (where I don't make comments, I either think the authors' responses are adequate or that I do not have anything to add that cannot be better evaluated by the authors of the initial reviews). Following that are some more specific comments based on the revised manuscript.

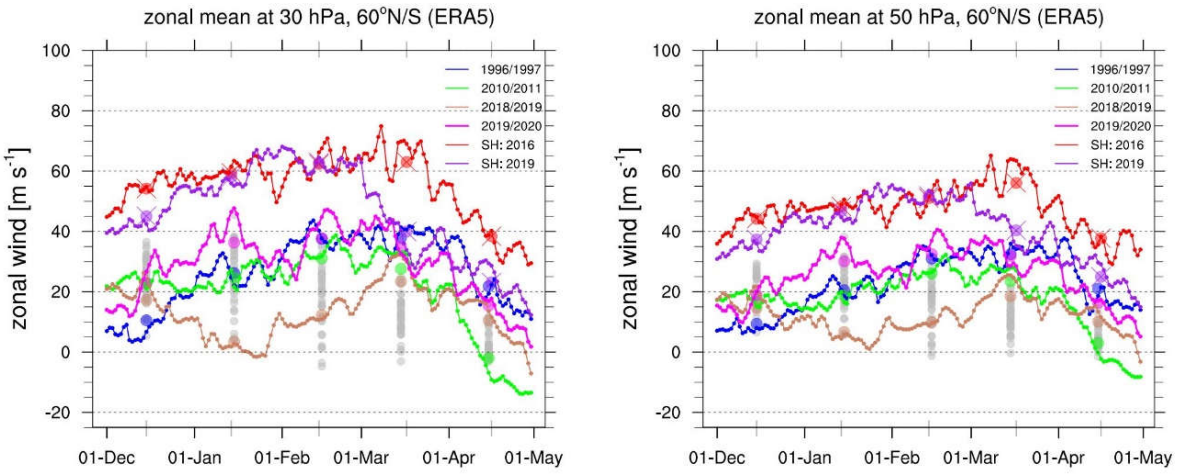
Comments Related to Author Responses To Initial Reviews/SCs:

All of the reviews and SCs questioned the suitability of using the term "Arctic ozone hole". While the revised manuscript is improved in this respect, in particular in presenting the TOC in the Arctic in the context of that in the SH, which helps greatly in conveying the ways in which the TOC values/patterns in the Arctic in 2020 did and did not resemble those in the Antarctic. However, there are several places (including in the abstract) where the authors have simply replaced "ozone hole" with "ozone hole-like pattern", which in my opinion can still be misleading and does not really address the fact that dynamics (e.g., very low temperatures in the vortex in cases where the vortex and the temperatures are very concentric) could in principle produce a pattern that looks superficially like an ozone hole even in the absence of any chemical processing. Of course, in practice what happens is that dynamical and chemical mechanisms reinforce each other when there are widespread low temperatures in the vortex. I think "ozone hole-like" (which, if you were going to use it should be "ozone-hole-like" since all together it is one compound adjective) really does not much change how the reader sees it, and thus the term should be avoided, especially in the abstract.

In the revised manuscript, we have changed or deleted all critical text passages (i.e. with respect to “ozone hole” or “ozone hole-like”). We got your point and the critical phrases have been eliminated. No misleading information or statement should be given now.

Regarding the focus on 10 hPa winds (now Figures 3 and 4), in relation to the comment in my SC about their lack of relevance to this paper, and also Ingo’s comment that Figure 3 (now Figure 4) was redundant, I do not think the authors’ response is adequate. They note that Lawrence et al (2020) also show zonal mean winds; however, Lawrence et al are giving an overview of the polar vortex throughout the stratosphere and its relationship to numerous other phenomena, and are using zonal mean winds in relation to common definitions of “strong vortex” and “weak vortex” events that are used for examining stratosphere / troposphere dynamical coupling. In contrast to this, the focus of this paper is on TOC and the chemical and dynamical processes in the lower stratosphere (e.g., near 50hPa) that lead to low TOC via chemical ozone depletion. In the section in Lawrence et al that focuses on lower stratospheric polar processing and ozone loss, they (because 10hPa winds in any view, as well as zonal mean winds at any level, give little information relevant to that) use diagnostics that are vortex-centered and that are at levels that are in the altitude region where these processes take place. Figures 3 and 4 (new numbering) do not add any information that is relevant to the focus of this paper, but simply serve as a distraction or misdirection. I think they should be deleted. If the authors believe it is necessary to include interannual comparisons of diagnostics of vortex strength, those should be diagnostics that capture vortex strength at the levels where the vortex strength is relevant to the evolution of TOC (e.g., centered near 50 hPa if on isobaric levels, near 450--550K if on isentropic levels). In fact the authors’ response to Ingo’s comment (that what is now Figure 4 illustrates the nearly circular shape of the vortex) is true only for the middle stratospheric vortex, since the shape of the vortex commonly varies greatly with height and how it varies is different every year -- thus this tells us little, if anything, about the shape of the vortex in the lower stratosphere. (Per my overview comment above, such diagnostics have been more thoroughly covered by Lawrence et al, and thus referring to that paper for this material may be sufficient to make the points about the vortex strength in relation to other years that are important for this paper.)

As mentioned in the manuscript, the zonal wind at 10 hPa is a common measure for the strength of the polar vortex. Of course, the shape (and strength) of the polar vortex can be height dependent. This was mentioned in the revised manuscript. It was also stated that with respect to 2019/2020 lower levels (altitudes) are showing qualitatively similar structures (i.e. a stable vortex). [See the added figures below.] You are right, the focus should be laid on the altitude range of 20 km (50 hPa), the height range of importance for ozone changes. Therefore, we have added Figure 2 (new) in our paper, which shows the PV field at 475 K. We also checked the PV field at 530 K, and it looks qualitatively similar (not shown). Figures 3 and 4 (new) are both containing information, which is quite relevant for the discussion of stratospheric dynamics and its variations. And, in this connection, we are discussing the results of Lawrence et al. (2020), to provide a consistent picture. We would like to keep those two figures in the paper. With respect to the discussion of the TOC changes, we are focusing on the minimum temperature in the polar cap at 50 hPa (Figure 6) and the potential area for PSC at 50 hPa (Figure 7), which is the altitude of importance for TOC changes.



Same as Figure 3 in the manuscript, but for 30 hPa (left) and 50 hPa (right). This work contains modified Copernicus Climate Change Service information (Hersbach et al., 2018; 2019a).

With regard to the authors' response to Ingo's comment re Page 4, lines 26--28 (regarding wave activity) in the original manuscript, a better response to this would be to make this point by referring to Lawrence et al (2020), who show and discuss wave activity / fluxes / propagation / reflection and its variations throughout the winter in more detail (note that the GSFC people who produce the information on the website currently mentioned in this manuscript are co-authors on the Lawrence et al. paper, meaning that discussion has been vetted by them and is consistent with what they post on their website).

At this place of the manuscript, we have added now also the paper by Lawrence et al. (2020) and changed the text a bit.

I agree with Ingo regarding Figure 5; it is common to show the years that are not highlighted as a grey envelope (with an indication of the range and standard deviation) because it conveys more complete information about the interannual variability of the daily values. Diagnostics based on the monthly mean values do not do that. The authors have used a format like that commonly used in Figure 8 (new numbering), and this would be a more informative way to show the comparisons with years that are not highlighted in other figures.

We have commented this also in the reply to Ingo. You get one point per months (i.e. the minimum in the monthly mean field) and the colored dots in Figure 6 (new) are showing the same temperature values, which are given in the table. It is also in line with the temperature data presented in Figure 5 (monthly mean field over the polar cap). The quantity of minimum monthly mean temperature is helpful because it shows firstly that the stratospheric temperatures in winter 2019/22020 were low. Secondly, we use this quantity because it indicates that the winter 2019/2020 was unusually cold over the complete winter season including early spring. In addition, with the help of the grey dots, it is demonstrated that 2019/2020 is in each winter month at the lower end of observed minimum temperatures. This is another kind of presenting the differences (the range of variability). Finally, showing the minima of the monthly means has the benefit, that small-scale or temporally limited events do not impact this analysis too much … We think that the description of the used data should be correct now.

In relation to my concern about showing the lower stratospheric vortex structure, and Ingo's request to add a 220DU contour and vortex edge contour to the plots in Figure 1, I think the new Figure 2 does help with the vortex definition, but also think Figures 1 and 2 should be combined -- the PV and column ozone maps could be shown side-by-side, if desired, but whether or not

the PV maps are shown, the 220DU contour (the color scale is not appropriate for the reader to be able to distinguish a particular color as the authors suggest) and a vortex edge contour (e.g., an appropriate PV value) should be overlaid on the TOC maps. It would also be helpful to add a 50-hPa 195K temperature contour to illustrate the relationship of the vortex to the cold region (as I recall, it was particularly concentric in this past winter, which is relevant to TOC morphology, especially before much chemical loss has occurred).

We have highlighted TOC data below 220 DU in the revised Figure 1. The area below 220 DU is now indicated in white. (To have consistent depictions, Figure 9 has been also updated.) We do not want to combine the Figures 1 and 2, to avoid overloading the figures with too much information. With respect to Figure 2, we would like to keep it as it, because it also contains temperature contours (in red).

I agree with Ingo that Figure 6 should show the comparison with the other years. And, per my comments above, think it would be much more useful if the other years were represented in a manner similar to that in Figure 8 (new numbering).

We have commented this also in the reply to Ingo. As said in our reply to the first review of Ingo, this figure (now Figure 7) has been created only to demonstrate the differences of the discussed three Arctic winter seasons. Added are also the values of the "warm" winter 2018/2019, which indicates that the "cold" areas (below 195 K) are very small (in this case only some few days around mid-December 2018). Our Figure 7 (new) indicates that the most obvious differences (for instance differences between the first half of December and first half of January between 2010/11 and 2019/20), which support the results also of the other analyses discussed in the paper. Further, the focus of this manuscript lies on Arctic conditions. Nevertheless, as the reviewers requested, we have added and discussed SH values at many instances in the manuscript to avoid any misunderstanding regarding the relation of ozone holes in the SH to low Arctic ozone values e.g. in 2020. We are thankful that the reviewers have raised this issue of relating SH to NH values as it helped to improve the manuscript. However, we think that this issue has been dealt with sufficiently and raising it here again is unnecessary and would make the manuscript longer.

The cumulative value should be taken as a proxy to document the differences between the four NH winter seasons... We would like to keep the figure as is (without showing the "range of variability" and respective values for the Antarctic) and hope that you will agree.

Regarding the authors' response to the question by referee #2 about P5L3 (re radiative cooling and dynamical processes), the authors' response is ambiguous and could be misleading. In absence of dynamical heat fluxes, lower temperatures lead to less radiative cooling because they are closer to radiative equilibrium. So the question here is really the balance of that with the reduction in warming because of reduced planetary wave activity (i.e., dynamical heat fluxes). The wording of the statement in the revised paper is likewise ambiguous and should be modified to clarify this point.

This statement has been modified in the 2nd revised paper version.

Regarding my comment about page 9, lines 4--8 (relation of cold and strong vortices), I still believe this is misleading and should be modified. Yes, you can have these conditions. But you can also have weak vortex / strong mixing / substantial ozone loss, as was the case in 2004/2005.... And in 1997, even after the temperatures became unusually low, the vortex was never remarkably strong (and was remarkably weak -- but only in the lower stratosphere -- earlier in the winter) (Manney et al 2011, Nature; Lawrence et al 2020).

To bring up here in addition the situation of 2004/2005 would make the things more complicated. Therefore, we decided to shorten this part of the text. The general statement in the beginning (starting with line 4) has been

deleted. Please have a look at the 2nd revised manuscript.

Other Comments / Questions on Revised Manuscript (in order of appearance in paper, not importance):

Page 1, line 21, and abstract in general: The point about the different mechanisms for the low TOC in 1997 vs the other two years compared (that is, the much smaller chemical loss in 1997) should be made in the abstract.

From our point of view such specific comments in the abstract about 1997 would not be helpful. Since we are focusing on the winter season 2019/2020, the abstract should highlight the results, which are related to the exceptional low TOC values in spring 2020.

Page 1, line 26--27: "larger" than what? Presumably than in other Arctic winters in the first usage and in the Antarctic than in the Arctic in the latter -- but since the same wording is used in two different ways, you need to be explicit about what you are comparing to in each case.

We said "larger" to make clear that it is not a single point, but that low TOC values are found in a considerable area. Therefore, we changed it to "considerable".

Page 2, lines 1-2: It seems odd to me to make a general statement like this and give only a reference that discusses three instruments measuring column ozone. What about the numerous instruments with vertically-resolved measurements of multiple species (which are also critical to fully monitoring ozone loss/recovery and the processes involved)?

Therefore, we said "for example". Since we are focusing in this paper on TOC (derived from GTO-ECV and TROPOMI), we are only referring to the paper of Loyola et al. Certainly, we could mention also other papers highlighting other measurements or measurement techniques, in particular vertically-resolved observations. But from our point of view they would be not directly related to the presented results …

Page 2, lines 16--18: There are also direct effects of lower temperatures, and a relationship to higher, colder tropopauses, that work in the same direction (see SI in Manney et al, 2011, Nature, and references therein, in addition to references you already give later on tropopause heights).

We added a short sentence about the possible role of tropopause changes and refer to Manney et al. (2011).

Page 2, lines 20--21: This should be reworded to make clear that the threshold temperatures are approximate values that depend on HNO3 and H2O concentrations, and that there are several other types of particles (e.g., STS, etc) that form at temperatures similar to those of the NAT particles.

The required information regarding HNO_3 and H_2O have been added.

Page 2, lines 21--22: This sentence (contrasting NH and SH) should be moved to the end of the paragraph, after the description of the chemistry, so that it doesn't interrupt the description of the steps leading to ozone loss.

Changed.

Page 3, lines 6--8: At this point in the paper, no evidence has been presented as to whether

this is due to chemical ozone loss. Therefore, it is premature to make this statement assuming it is related to chlorine-catalyzed chemistry.

We have tried to adjust the corresponding text.

Page 3, line 9: "stable" is not an appropriate word here, as it has a specific formal meaning in relation to the dynamical stability (e.g., barotropic or baroclinic instability) of the flow; "quiescent" or "undisturbed" would be appropriate terms.

Changed from "stable" to "undisturbed".

Page 3, lines 16--19: The papers cited here are all, with the exception of Tegtmeier et al, primarily chemistry papers, that is, they discuss the links of particular dynamical conditions to chemical loss. It would be worth citing some of the papers that discuss direct dynamical mechanisms in addition to those focused on in Tegtmeier et al (see, e.g., references in Manney et al, 2011, Nature, SI).

We have added the reference of Petzoldt (1999).

Page 3, lines 22--24: Instead of this detail / URL, and in addition to Wohltmann et al, please cite Bernhard et al (2020), submitted to GRL; this paper details column ozone anomalies in 2020 from OMI and from ground-based measurements and the corresponding UV anomalies. (Since this paper details TOC anomalies in different datasets than the ones used here, there are probably a few other places in this paper it could be cited and the consistency of their results with this paper mentioned. The same is true for comparison of TOC results with those in Inness et al. (2020).)

During the phase of the record low TOC (late February until mid-April), we looked at this URL daily for comparison with the TROPOMI data. Therefore, we mention the website here. It provides a complete overview. And a corresponding reference (van Geffen et al., 2017) is given. Of course, we could cite other papers, but I think we should not cite papers, which are still not accepted. At this place, we could live with the reference of Ingo's paper (in addition to the "URL").

Page 3, line 27, and page 4, lines 2--4: As noted above, a comprehensive (much more so than in this paper) description of the dynamical situation in 2019/2020 winter (also compared with 1996/1997, 2010/2011, and 2015/2016) is already published in Lawrence et al (2020).

Ok, therefore we are referring to the Lawrence et al. (2020) several times including a discussion of the results presented in his paper. At the end of the Introduction we are saying what we have done and what will be presented in this paper.

Page 4, lines 13--14: From "using the CDO" to the end of the sentence should be deleted, or, if you feel it is very important to give this detail, moved to the "Data Availability" section.

Deleted as suggested and this part of the sentence has been moved to the "data availability" section.

Page 4, line 25: Using "less than" and "up to" with signed values is a bit imprecise, technically it should say, for example, "less than +1% or more than -1%". It would be best to rephrase this so you talk about the magnitude of the bias and standard deviation (which isn't a signed value to begin with) rather than stating a signed value. I also fail to see why you need to give a range when it is prefaced by "up to" -- just say "up to 2.5%".

Slightly changed, as suggested.

Page 6, lines 17--18: The results of Lawrence et al and Wohltmann et al are more comprehensive than those shown here, so it might be sufficient to replace the minimum temperature plot (Figure 6) by a brief description of their results with the citations.

As said, a first version of Figure 6 was prepared in April 2020 without the knowledge of the other studies and figure therein. Among other, now this figure contains other (SH) information. We suggest to show this figure as is.

Page 6, line 25: Dameris 2010 is a rather obscure reference to cite for what is textbook material. In addition to Solomon 1999 (or instead of in this case), I would suggest Chapter 7 of the 2000 textbook "Chemistry and Physics of Stratospheric Ozone" by Andrew Dessler.

Dameris (2010) has been deleted. The citation of Solomon (1999) should be sufficient.

Page 6, line 33 to page 7, line 2: Should cite Wargan et al (2020) here.

The reference of Wargan et al. (2020) has been added here.

Page 7, lines 13--15: Manney et al 2011, Nature, also show the impact of tropopause height variations on column ozone, comparing 1997 to 2011.

The reference of Manney et al. (2011) has been included here.

Page 7, line 19: The statement "...the polar vortex existed already in late November and early December 2019" should be compared / contrasted to the other years considered here (this could be done very briefly by citing Lawrence et al 2020, who contrast the early development of the vortex in fall 2019 with other years.

Lawrence et al. (2020) has been added here.

Page 7, lines 22--23: This is a good example of a place where it is particularly inappropriate to say "an ozone hole-like pattern". In January, there has been little chemical ozone loss (almost none in most years) so the pattern of low ozone inside the vortex is primarily related directly (dynamically) to the low temperatures and concentricity of the cold region with the vortex. Even in July (+6mo) in the SH, the "ozone hole-like pattern" is mostly due to dynamical effects of low temperatures -- it is generally mid-July before the chemical loss signature overwhelms the dynamical ones. It is not appropriate to call every large low ozone region within the polar vortex an "ozone hole-like pattern".

As said above, such phrases have been eliminated all in the manuscript. The text has been changed accordingly.

Page 7, lines 24--25: As discussed above, the 10hPa winds provide no information about the strength, size, or shape of the lower stratospheric vortex. In addition, rather than saying "(not shown") you could cite Lawrence et al (2020) for strong PV gradients.

In this context, Lawrence et al. (2020) was cited already (see beginning of next sentence).

Page 7, lines 27--30: This could be replaced by citing Wohltmann et al (2020) and Bernhard et al (2020).

The reference of Wohltmann et al. (2020) is already included here.

Page 7, lines 31--32: This ("strong horizontal gradient in the vicinity of the polar jet with strongest zonal winds") is not shown in any of your figures.

Text has slightly changed and "not shown" has been added.

Page 8, lines 22--26: Other dynamical effects that vary interannually (direct effects of low T, tropopause variations) could also be mentioned here, with appropriate references as already suggested above.

Respective literature has been added.

Page 8, line 27 through page 9, line 9: This paragraph is again discussing middle-stratospheric fields as if they were (1) relevant to the lower stratosphere and (2) had the same relationship to the conditions in the lower stratosphere in each year. Neither of these is true.

The aim of the figure is to show dynamical differences of the stratosphere with respect to the three winter seasons. Although we are only looking at 10 hPa, the results give a consistent picture in comparison with the results presented in other related studies, for instance Lawrence et al. (2020) and Lee and Butler (2020). Therefore, both papers have been cited. As said already, we also looked at other lower altitudes (i.e. 30 and 50 hPa) and there the zonal wind data showed qualitative similar results with respect to the temporal evolution of the mean zonal wind at 60°N. In addition, we have added the new Figure 2, showing the PV at 475K, representing the dynamics of the lower stratosphere in spring 2020. In combination with the results of Figure 6 (minimum temperature in the polar cap region at 50 hPa) we think that the overall message should be (more or less) clear. Our results (and conclusions) do not show (present) a different (or wrong) picture, in comparison with the results presented in the other published studies.

Page 10, lines 6--7: This is not true. Manney et al and Wohltmann et al found that ozone loss was very similar in the two years. Ozone values were lower in 2020 because chemical loss started early and possibly because of less replenishment by descent and/or less mixing.

Correct! You are right! We changed it and another sentence has been added.

Page 10, lines 8--29: The first paragraph here is an example where examining V_psc (or V_psc / V_vort) would provide a more complete picture. Both Lawrence et al (2020) and Wohltmann et al (2020) do this. These paragraphs could be condensed in light of that published information.

We have slightly reduced these two paragraphs. The mentioned papers have been cited many times, also at other place. The discussion of "our proxy" (T-area below 195K at 50 hPa) provide another view of the differences between these three winter seasons. The given numbers help to distinguish between the years of interest. The results are in line with the other investigations. And here we would give an interim conclusion, before the discussion of results start. We would like to keep it …

Page 11, lines 5--6: Please clarify what you mean by "typical" here. The ozone loss in 2011 was not typical, rather it was "unprecedented".

This was referring to 1997. However, the last part of the sentence has been deleted.

Page 11, lines 21--22: This is too oversimplified (see previous comment on radiative heating vs dynamical heat fluxes).

We have slightly changed this paragraph. The formulation is now more vague pointing to possible conditions and relationships.

Page 11, line 23 through page 12, line 16: Could be condensed, since this information content is already in published papers.

Again, we have slightly reduced the text. Nevertheless, as you said (previously published material is a 'moving target'), it is difficult for us to reduce our statements in a way, that it looks like that "everything" was already published before. The outline of our paper was created at a time, when we did not know the scientific content of the other papers.

Page 12, line 17 through page 13, line 6: These two paragraphs seem tangential to the focus of the paper, and, since they are entirely discussing results shown in already published papers without making any cogent about the relevance to this paper, seem more of a distraction than anything else.

The reason for having these two paragraphs is that one of our focusses is the role of stratospheric dynamics for the causing situation with reduced TOC in polar regions. Here we would like to point out that the dynamical behavior in both hemispheres yield to (more or less) equal circumstances. From our point of view, it makes sense to put together the available information (in particular based on recent years) and discuss it in the context (see the figures, which contain the data of other years of relevance). Nevertheless, we have again tried to shorten this part of the manuscript.

Page 13, lines 7--12: This has already been discussed in Wohltmann et al (2020) and thus could be condensed or removed.

Same reply again. We think that our Figure 6 contains an interesting comparison showing not only NH conditions, but also those of the SH. A direct comparison of NH and SH conditions helps to get the main message (i.e. the role of stratospheric dynamics). We would like to keep this paragraph …

Page 13, lines 13--26: This has already been discussed in Lawrence et al (2020) and thus could be condensed or removed.

Same reply again. Yes, Lawrence and colleagues discussed it already (in parts) … and therefore Lawrence et al. (2020) has been cited (again) in this context.

Page 13, line 30: "However" is not appropriate here -- the "extended phase of active stratospheric chlorine" leads to the "substantial ozone depletion", whereas with "However" you are saying that the latter is in contrast to the former. (Note also that neither of these is a result of this paper, though both are shown by Manney et al, 2020, and the latter by Wohltmann et al, 2020.)

The word "however" has been deleted. We did not include the references before as we thought the reasoning is typical textbook knowledge and we made vague statements ("were pointing to"). Nevertheless, we added the references as suggested and rephrased the whole part.

Page 14, line 3: No one has suggested that it was demonstrably due to climate change. Wohltmann et al (2020; and to a lesser degree Manney et al, 2020) have also already discussed this.

Ok, we also would like to raise this point. Such a question is lying on the hand and therefore is of general interest. And therefore, we have added the following paragraph, putting together the conflicting results and interpretations.

Page 14, lines 23--25: This statement does not appear to be related to anything else in the paper and seems completely out of place. It also doesn't follow from anything shown in this paper. I suggest deleting it.

The question of the possible role of the unexpected CFC-11 emissions was raised (e.g., by colleagues and journalists) with respect to the record low Arctic TOC in spring 2020. For this reason, we would like to keep this statement in the manuscript to make clear that the low ozone values in spring 2020 were not affected by enhanced CFC-11 emissions.

Page 15, line 4: It isn't clear what "and TOC values below 220 DU are seen for up to about four months" is in relation to here. Is this for the Antarctic? For the Arctic in extreme winters?

The text has been revised. It should be much clearer now.

Page 15, lines 11--14: Add Bernhard et al (2020), DeLand et al (2020; https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2020JD033271; this discusses OMPs PSC measurements in 2020 and compares them to Antarctic PSCs), and Inness et al. (2020). There are also two other papers published for this special section, on aspects of strat/trop coupling and S2S forecasting, but I don't think these need to be cited specifically here, since they are not directly related to the topics of the current manuscript. However, "a couple" should probably be changed to something like "several".

This paragraph has been changed / shortened. We would like to avoid citations of papers, which have not been discussed in our paper. We now are saying that "numerous papers" can be found in this special issue.

Typos / Grammar / Minor Wording:
Page 1, line 27: should be "...(on the order…" Changed!
Page 2, line 5: "allow" should be "allows" and "hamper" should be "hampers" Changed!
Page 2, line 8: "an altitude" should be "altitudes" Changed!
Page 2, line 19: "lower polar" should be "polar lower" Changed!
Page 3, line 5: "heavy" should be "large" or "strong". Changed to "severe"!
Page 3, line 13: "have" should be "has". Changed!
Page 3, line 21: delete comma after "noteworthy". Changed!
Page 3, line 31: "far away" could just as easily mean "far below" as "far above"! Changed!
Page 4, line 15: "to" should be "as for". Changed!
Page 4, line 17: delete "laid", and "data is" should be "data are". The first sentences in line 17 (page 4) has been deleted; "data is" has been changed to "data are!
Page 6, line 11: "50 hPa" isn't really a height "range". Wording has been changed!
Page 6, line 15: Please say "approximate activation threshold". This part of the sentence has been deleted.
Page 6, line 31: I have no idea what you mean by "exemplarily corresponding" -- this is certainly incorrect English usage in this sentence, but I can't suggest a correction because I don't know what you mean to say. We have deleted the word "exemplarily". Now it should be clear.
Page 5, line 8: This sentence is not very clear. What does "They" refer to? It would also be

better to say “using a correction” rather than “in terms of a correction”. Both points have been changed!
Page 7, line 8: Suggest adding “and references therein” to the Millán and Manney reference. Added!
Page 11, line 7: “is showing” should be “shows”. Changed!
Page 11, line 16: “is in large part reflecting” should be something like “reflects in large part” or “to a large degree reflects”. Changed!
Page 13, line 29: “five weeks” should be “five-week”. Changed!
Page 15, line 3: “in” should be “on”. Changed!

Thank you very much for your support!