Review of "Evaluation of record low ozone values over the Arctic in boreal spring 2020" (revised from "First description and classification of the ozone hole over the Arctic in boreal spring 2020") by Dameris et al.

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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.

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.

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.)

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).

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.

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).

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).

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.

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).

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.

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.

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)?

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).

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.

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.

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.

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.

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).

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).)

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).

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.

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%".

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.

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.

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

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.

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".

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.

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

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.

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.

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.

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.

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.

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".

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

Page 11, line 23 through page 12, line 16: Could be condensed, since this information content is already in published 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 point about the relevance to this paper, seem more of a distraction than anything else.

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

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

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.)

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.

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.

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?

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".

Typos / Grammar / Minor Wording:

Page 1, line 27: should be "...(on the order…"

Page 2, line 5: "allow" should be "allows" and "hamper" should be "hampers"

Page 2, line 8: "an altitude" should be "altitudes"

Page 2, line 19: "lower polar" should be "polar lower"

Page 3, line 5: "heavy" should be "large" or "strong".

Page 3, line 13: "have" should be "has".

Page 3, line 21: delete comma after "noteworthy".

Page 3, line 31: "far away" could just as easily mean "far below" as "far above"!

Page 4, line 15: "to" should be "as for".

Page 4, line 17: delete "laid", and "data is" should be "data are".

Page 6, line 11: "50 hPa" isn't really a height "range".

Page 6, line 15: Please say "approximate activation threshold".

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.

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”.

Page 7, line 8: Suggest adding “and references therein” to the Millán and Manney reference.

Page 11, line 7: “is showing” should be “shows”.

Page 11, line 16: “is in large part reflecting” should be something like “reflects in large part” or “to a large degree reflects”.

Page 13, line 29: “five weeks” should be “five-week”.

Page 15, line 3: “in” should be “on”.