Short (well, rather long actually!) comment on "First description and classification of the ozone hole over the Arctic in boreal spring 2020" by Dameris et al.

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

Other comments on / reviews of this paper have also already discussed the claim of "First" and the lack of citations of other papers in review and published on this exceptional winter, and I agree overall with their remarks. I do have additional "philosophical" comments on this subject:

I am happy to see numerous papers submitted on the exceptional 2019/2020 stratospheric winter, and made publicly available as preprints, regardless of what journal they are published in (the special issue is simply a vehicle to encourage and facilitate publication of as comprehensive work as possible on this winter) -- this includes, in particular, seeing the full range of datasets and models that are available studied, which points to what I see (and the authors mention in their brief reply to Ingo's review) as a strength of this paper, namely, the use of TROPOMI data along with the GOME-type Total Ozone Essential Climate Variable (GTO-ECV) to describe the evolution of total ozone in relation to previous winters. I believe we as a scientific community will learn the most (always our ultimate goal) about this winter if we all study and cite each other's work, and discuss / collaborate with each other in analysis and modeling of what we see in the observations. The move towards open access to preprints of submitted papers -- strongly supported by both AGU (in making preprints available on ESSOAr immediately after submission) and EGU (in journals with open review such as ACP/ACPD) -- allows us to do this much more effectively than in the past. Thus:

(1) As one with possibly a strong claim to leading the "first" peer-reviewed paper on Arctic ozone loss during 2019/2020 (the Manney et al GRL paper was submitted in late May, available on ESSOAr by early June, first published in GRL online on 17 July), I firmly believe that **none** of us should be claiming to be first when all of us have been working since it became apparent that this past Arctic winter was going to be exceptional to produce and disseminate the best science describing / explaining the observations!

(2) In this collaborative spirit, I hope that not only will the authors of this paper cite other submitted papers that are openly available wherever it is appropriate to support or show consistency with the material in this paper, regardless of whether they have received final acceptance or not, but also that those papers (e.g., for the GRL/JGR special issue) that are still at a stage prior to having completed their final revision will in turn cite this preprint wherever it is appropriate to support or show consistency with their results.

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

Major Scientific Comments:

The biggest issue that has not been raised in other comments / reviews at the time I'm writing this, and that I believe must be addressed before peer-reviewed publication, is the comparisons of 2019/2020 with 2010/2011 and 1996/1997, and the failure to communicate the very large differences in polar processing and ozone loss in 1996/1997 compared to the other winters studied. In the context of comparing superficially similar springtime lower stratospheric vortex conditions in 1996/1997 and 2010/2011, the very large differences in polar chemical processing in those two winters have been extensively highlighted, first in detailed discussion in the supplementary information (SI) of Manney et al. (2011, Nature), and in numerous later publications culminating in a detailed summary / synthesis in the WMO 2014 Scientific Assessment of Ozone Depletion (section 3.2.3.3), which provides further references. In short, for numerous reasons (very late lower stratospheric, LS, vortex development and late drop of temperatures below PSC thresholds, smaller altitude region of low temperatures, weaker LS vortex throughout the winter, little/no denitrification, etc), chemical ozone loss was much less in 1997 than in 2011 (and hence than in 2020, which saw as much or more chemical loss as in 2011, eg, Manney et al, 2020; Wohltmann, et al, 2020; Grooß and Müller, 2020). Moreover, dynamical conditions led to frequent ozone mini-holes (e.g., Coy et al., 1997) and higher tropopause altitudes (e.g., Manney et al, 2011, Nature, SI) in spring 1997 that contributed to lower column ozone via dynamical processes than followed other winters with comparable chemical ozone loss. This is an important distinction that it is essential to address for the comparisons in this manuscript to provide accurate information on the similarities (a few) and differences (many) between 1997 and the other two winters considered. Statements such as (to pick only one example, page 10, lines 8-9) "...all three years showed particularly strong ozone depletion..." are scientifically inaccurate. This also folds in with the inadvisability of lightly using the term "Arctic ozone hole", as 1997 is a classic case of a situation that looked superficially similar to the Arctic winters, 2011 and 2020, with the most chemical ozone loss and in some ways "Antarctic-like" conditions (see WMO 2014, Section 3.2.3.2; Manney et al, 2020; Wohltmann et al, 2020), but which in fact had chemical processing that was in no way comparable to that in the Antarctic.

I also have concerns with the description of the dynamical conditions in relation to previous winters. The dynamical situation is described using only 10hPa zonal mean winds and 50hPa temperatures.

Zonal mean winds in the middle stratosphere (10hPa as opposed to the levels around 50hPa where chemical processing maximizes) are virtually irrelevant to the state of the lower stratospheric vortex, because:

(1) Vortex strength, size, and geometry vary strongly with altitude in different ways in different winters -- we have seen winters (such as 2010/2011) where the vortex was exceptionally strong in the lower stratosphere but not in the middle stratosphere, and winters (such as 1997) where the vortex was for much of the winter fairly typical in the middle stratosphere but exceptionally weak in the lower stratosphere.

(2) The Arctic vortex is rarely close to symmetric or pole-centered, even in the coldest and/or most dynamically quiescent winters (see, e.g., Figure 1 in Manney et al, 2020, or any of numerous other publications in the past ~20 years), and its size, shape and position vary dramatically both intraseasonally and interannually. Thus, zonal means, even were they examined at altitudes in the range where LS polar processing occurs rather than at 10hPa, provide very little information on characteristics of the polar vortex such as size, location, and strength.

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

(1) The vertical structure/location/extent of the region with temperatures conducive to PSCs varies strongly interannually and within seasons; this is one of the reasons why one of the most useful measures of polar processing / ozone loss potential (both day-to-day and as a measure of total ozone loss potential in a given winter) is V_psc, the area below the PSC or chlorine activation threshold integrated over all lower stratospheric levels.

(2) Because the LS vortex varies strongly in size, shape, and position, while the high-latitude minimum associated with the polar vortex is usually north of 50N in dynamically quiet winters, this may not always be the case, and is certainly not always the case in winters with strong SSWs during the cold period (Dec--Feb).

Furthermore, in relation to column ozone and its relationship to the LS vortex and low temperatures, because low column ozone is strongly spatially correlated with low LS temperatures by dynamical processes (see, e.g., discussion and references in SI of Manney et al, 2011) and the region of low temperatures in the LS is often not well-correlated with the lower stratospheric vortex (see, e.g., Manney et al, 1996, GRL; Mann et al, 2002, JGR; SI of Manney et al, 2011; Lawrence et al, 2015, ACP; and references in those latter two works), in absence of strong chemical depletion, the shape / extent of the region of low column ozone is not expected to be correlated with the shape / extent of the polar vortex. Therefore, in the maps of column ozone in Figure 8, we cannot judge whether the morphology of the low region is consistent with strong ozone loss unless we know how it relates to the morphology of the LS vortex. E.g., one of the most commonly used metrics for this is a contour or contours of potential vorticity (PV) on an isentropic surface somewhere in the LS (somewhere between about 450 and 550K is typical, commensurate with the approximate levels where ozone contributes most to the column), with value(s) such that it is (they are) in the region of strong PV gradients bounding the vortex. (Similarly, the strength of PV gradients along the vortex edge is a common and valuable metric of vortex strength -- while maximum windspeeds at an appropriate level would also be informative of vortex strength, zonal mean winds are not.) PV on isentropic surfaces is readily available in all modern reanalyses including the ERA5 reanalysis used herein.

Please note that Lawrence et al (2020; as I write this in late August, nearing completion of minor revisions for JGR, and available on ESSOAr since mid-June) in their section 3.4 (submitted version) provide a detailed discussion of LS vortex strength and chemical processing potential in 2019/2020 in comparison with the record from 1979 through 2019 and in particular compare with 1997, 2011, and 2016 (2016 is of interest because, while low temperatures and chemical

processing ended much earlier, it still is the record cold winter in January and February, and had overall greater polar processing potential and more chemical loss than that in 1997; e.g., Manney and Lawrence, 2016, ACP; WMO 2019; and references therein), using widely accepted diagnostics / methods with uncertainties quantified. Because this paper for the AGU Special Collection provides an overview of the dynamical conditions during the winter, it is designed to provide this "foundational" material in a thorough way so that other papers on this winter can start with that comprehensive description as background. [While not as relevant to this paper and the 2019/2020 ozone loss, Lawrence et al (2020) also describe thoroughly in their Section 3.1 the evolution of 10hPa zonal mean zonal winds in 2019/2020 versus climatology (their Figure 1) and in the context of the time series since 1959 (their Figure 2).]
[The points above for which I have not provided references are well documented, e.g., in the past ~4 WMO Assessments and references on Arctic polar ozone and ozone loss therein. The relevant aspects of the meteorological situation are also discussed in Lawrence et al (2020), Manney et al (2020), and/or Wohltmann et al (2020).]

Other Scientific Comments / Questions:

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

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

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

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

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

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

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

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

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

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

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

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

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

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