Review of “Dynamical and chemical processes contributing to ozone loss in exceptional Arctic stratosphere winter-spring of 2020” by Smyshlyaev et al.

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This paper aims to analyze and distinguish dynamical and chemical contributions to the evolution of Arctic ozone in the 2019/2020 winter. Results for dynamical diagnostics from reanalysis data are presented along with an analysis using reanalyses temperatures to estimate chemical ozone loss from ozonesonde data; this is followed by trajectory modeling that is used to diagnose chemical ozone loss from assimilated ozone data from the ERA5 reanalysis. Finally, the authors present analysis of chemistry transport model runs with different scenarios aimed at showing the relative contributions of various chemical and dynamical processes to the evolution of ozone in the 2019/2020 winter. The paper as it stands is not suitable for publication in ACP, for the following major reasons:

(1) Dynamical results from reanalysis data: The dynamical diagnostics shown from reanalyses and the discussion thereof are almost all things that have already been published in existing papers on the 2019/2020 winter; in addition, the authors are unclear about which reanalyses are used where and why -- in particular, the NCEP / NCAR reanalysis is deprecated for all stratospheric and polar processing studies and should not be used; and it appears that for any given diagnostic or model calculation, one reanalysis (though often it is not stated which) is used -- while comparing multiple reanalyses for each calculation is highly desirable and enhances the robustness of the results, using different individual reanalyses for different calculations does the opposite, since one cannot even evaluate the results as a whole knowing that they are based on the same representation of the atmosphere. Further, the construction of and/or interpretation of some of the diagnostics is unclear or inconsistent.

(2) Use of ERA5 assimilated ozone for quantitative estimates of ozone loss: Because ERA5 ozone is an assimilated products based on ingesting several datasets (including different datasets at different times), extensive validation of this product would be needed before using it to derive quantitative estimates of ozone changes, especially on the daily temporal and relatively localized (e.g., where zonal means are inappropriate) spatial scales that are important for polar stratospheric ozone loss. While doing so is a highly valuable undertaking, I am not aware of any study that has done this already.

(3) Trajectory modeling: The initialization of the trajectory model on a single latitude circle makes all of the results highly suspect, and makes interannual comparison virtually impossible, since any latitude circle will be in different parts of the vortex at different times and especially in different years. Without relatively uniform sampling (to guarantee which one would have to initialize parcels relatively uniformly throughout the vortex, e.g., a procedure similar to that described in Manney & Lawrence, 2016, ACP), you cannot even compare results on different dates in one year, much less do fair interannual comparisons.

(4) Chemistry-transport modeling: There is inadequate description of the details (e.g., initialization dates and fields, boundary conditions, etc.) of the set up of the model runs.

Some of the interpretation of the results is unclear or inconsistent. It is not obvious that the model has been well-validated, nor that the agreement with observations shown here is adequate.

Because of these issues, I cannot recommend publication in ACP unless / until the authors focus the paper clearly on the results that are new and clearly acknowledge and describe existing literature where they are not; clearly describe all of the reanalyses and datasets that are used, which ones are used for what diagnostics, and justify why those data products are chosen; resolve the inadequacies in the description and formulation of the model simulations and in the interpretation of the results; and enact substantial improvements to clarify the writing and English usage. The specific comments below give further details on these issues that should provide guidance if the authors choose to revise the paper.

Clarification issues that are needed throughout:

You should be careful about using (as you currently do even in the abstract where being precise is especially important) terms like "ozone loss", since that is usually taken to refer to chemical "loss". Also, for the most part, dynamical factors tend to *increase* ozone in the lower stratosphere, so saying they contribute to chemical "loss" (or to ozone decreases to use a term that does not imply chemical loss) can be confusing. Finally, whether and which dynamical processes contribute to decreasing ozone depends on whether you are talking about column or vertically-resolved ozone -- for example, column ozone is lower in cold regions because of the direct impact of lower temperatures on density at a given pressure, and this can be a substantial portion of the appearance of very lower column ozone values in the coldest portions of the vortex; in many places in the paper it is not made clear which you are talking about, and in some places it is not clear how calculations of one relate to the other.

Similarly, you often use the term "ozone anomalies" when you specifically mean low ozone in the winter polar lower stratospheric vortex (or equivalently low column ozone) that is related chemical loss. The are / can be many other kinds of "ozone anomalies", including winter/spring seasons (such as 2015 in the Arctic) with anomalously high ozone, as well as other kinds of low ozone anomalies (such as "mini-holes" in column ozone, which are entirely dynamical in origin and typically appear outside the polar vortex, but often at high latitudes near the vortex in winter). If you are going to use the term anomaly, you should define exactly what it is an anomaly from; however, it appears to me the way you use it means unusually low ozone relative to climatology that arises at least partially from chemical loss -- if that is the case, I would suggest using different terminology that is more precise. (E.g., page 2, line 30, instead of "...significant ozone anomalies are observed in the Arctic less…" it would be clearer to say something like "...extensive chemical ozone depletion occurs less often in the Arctic than in…"; on line 34, for the Antarctic, it makes sense to simply say something like "the Antarctic ozone hole was one of the deepest / most extensive on record…")

Specific Comments (in order of appearance, not importance):

(Where I suggest references, I have tried to provide the DOIs if they are not already cited in this manuscript.)

Introduction, overall: While I'm providing a number of comments below about particular statements and the literature cited for them in the introduction, I question whether this detailed a review of well-known impacts of stratospheric ozone loss is needed or appropriate for this paper. For example, possible (though as yet still controversial) effects on precipitation or weather seem as best peripheral to this paper. I believe much of the material that is not directly related to setting the context for interannual variability and interhemispheric variability in stratospheric vortex dynamical and chemical conditions and chemical ozone loss could / should be condensed or deleted.

Page 1, lines 28-29: This is one of many places where there is a very incomplete list of references, some of which are not the most appropriate ones. In cases like this where it is a general, well-known point, adding a recent review paper (such as Domeisen et al 2019, https://doi.org/10.1029/2019JD030923 in this case) or at least simply adding "e.g.," before or "and references therein" after would convey the information that these are only examples of some of the literature on the subject. Simply adding "e.g.," beforehand would probably be sufficient in this case.

Page 2, lines 30--31: Smyshlyaev et al (2016) is not a key reference here, I would suggest some earlier papers that were among the first to focus on disentangling chemical and dynamical effects on ozone (e.g., Manney et al, 1995, JAS, https://doi.org/10.1175/1520-0469(1995)052%3C3069:LTCUDP%3E2.0.CO;2; Manney et al 2011, Nature -- especially the SI for details on chemical and dynamical effects on column ozone -- and references in the latter). WMO reports are always good references, in this case the 2006 one has a particular detailed section on diagnosing chemical and dynamical effects on column ozone. This is a case where "and references therein" is definitely appropriate.

Page 2, line 33: As you note on line 36, there was also a strong SSW (arguably stronger in terms of abrupt changes than that in 2002) in the SH in 2019; Wargan et al (2020, JGR) should be cited in both places for that; and it should be mentioned with the 2002 one (reorganizing this paragraph to talk about them together would be helpful. Solomon et al (2014) is not an appropriate reference for the 2002 SH SSW -- the most appropriate ones would probably be Allen et al (2003, GRL, doi:10.1029/2003GL017117) and/or Hoppel et al (2003, GRL, doi:10.1029/2003GL016899) -- the first peer-reviewed papers on that SSW -- and Shepherd et al (2005, JAS -- the preface to the special issue on that SSW).

Page 2, line 35: More appropriate references for the depth of the 2015 Antarctic ozone hole would be Ivy et al. (2017, GRL, doi:10.1002/2016GL071925), Stone et al. (2017, JGR, https://doi.org/10.1002/2017JD026987), and/or the 2018 WMO report.

Page 2, line 37: Need to specify whether by “largest decrease” (should be “decreases”) you mean in vertically-resolved or column ozone.

Page 2, line 41: Should cite Bernhard et al (2013, ACP, https://doi.org/10.5194/acp-13-10573-2013) for anomalously high surface UVI in 2011.

Page 2, line 45: These results of Chubarova et al (2020) are questionable, given that the three methods used in that paper to estimate UV trends resulting from changes in cloudiness and ozone agree very poorly (their Figure 13).

Page 2, line 52: Manney & Lawrence (2016, ACP, cited elsewhere in this manuscript), should be cited here.

Page 2, lines 61-62: Need references for this sentences; Lawrence et al (2020) is good for the temperatures (also several other papers in the JGR/GRL special issue on the 2019/2020 Arctic vortex, including Wohltmann et al, 2020, which you cite elsewhere); DeLand et al (2020) should be cited here (as well as where you do later on) since it discussed observed PSC activity.

Page 3, line 65: Reference should be Dameris et al (2021). Other published papers that discuss the low column ozone and diagnose its chemical origins should be cited here, including Wohltmann et al (2020), Inness et al (2020, JGR), and others from the aforementioned special issue.

Page 3, line 77: The fact that it was exceptionally long-lived, which you don’t mention, was also critical (e.g., Manney et al, 2020; others). Because the results in the paragraph this sentence ends are all from published papers, I believe this should be greatly condensed with appropriate references to those papers.

Page 3, line 82: If you are going to discuss “the El Nino-South [sic] Oscillation effect”, you need to define what that is. A reference to the review by Domeisen et al (2019, Rev. Geophys, https://doi.org/10.1029/2018RG000596) could be helpful. However, I am not sure that this paragraph contains any information that is necessary / directly relevant to the current manuscript, since you do not analyze any relationships to these SST patterns.

Page 3, lines 86--92: This discussion of the early 2019 major SSW is peripheral to this paper and does not add anything. Further, if it is included, the radiative / dynamical interactions leading to a slow recovery after many strong, early-season SSWs should be discussed (e.g., as in Hitchcock and Shepherd, 2013, JAS, DOI: 10.1175/JAS-D-12-0111.1).

Page 3, line 95: Add “e.g.,” before Rex et al reference, since there are numerous papers on this.

Page 4, lines 101--110: This discussion could be condensed to a sentence with appropriate references, or deleted entirely. However, taking this as it is: Saying there were “regular...ozone holes in Antarctica” by “the end of the twentieth century” is misleading given that there were annual ozone holes by the early 1980s. Even more importantly, there has not been anything that could be unequivocally called an "ozone hole" in the Arctic, even through 2020 (see, e.g., Solomon et al, 2014; Wohltmann et al, 2020; and the online discussion for Dameris et al, 2021). If you are going to talk about the impacts of the Montreal protocol, it would be best to cite some of the several "World Avoided" papers that address this topic in detail (e.g., Newman et a, 2009, ACP, https://doi.org/10.5194/acp-9-2113-2009; Chipperfield et al, 2015, Nature Comms, DOI: 10.1038/ncomms8233).

Page 4, lines 110--114: There are numerous other references so at least add an "e.g.," before Weber et al. Also, if you cite Ball et al (2018) it is also important to cite some following papers that update and / or call those results into question (e.g., Wargan et al, 2018, GRL, https://doi.org/10.1029/2018GL077406; Chipperfield et al, 2018, GRL, https://doi.org/10.1029/2018GL078071; Ball et al, 2019, ACP, https://doi.org/10.5194/acp-19-12731-2019, 2019).

Page 4, lines 115--124: Again, this paragraph could be greatly reduced or deleted. Also: on line 116, saying the 2019 ozone hole was "lowest" is very confusing, I'd suggest "shallowest" or some other such wording; line 120, neither Butler et al 2020 nor Wargan et al 2020 discuss the 2020 Antarctic ozone hole (and Butler et al 2020 is about two NH winters), so neither is an appropriate reference here. Further, the statement on lines 118--120 that the Antarctic is showing increasing interannual variability is entirely speculative and no evidence is given to back it up (two contrasting years does not make a trend).

Page 4, lines 126--128: It would be good here to make a clear statement about what is new in the paper that goes beyond the papers that have already been published.

Page 5, lines 138--139, and Section 2.1: It should be stated which reanalysis or reanalyses are used for each diagnostic shown in the paper. As per the major comments, need to justify using and/or showing different reanalyses for different diagnostics. A very strong justification is needed for using the NCEP/NCAR (aka NCEP-R1, or just NCEP as you call it) reanalysis, which has long been deprecated for any polar processing studies (e.g., Manney et al, 2005, MWR, https://doi.org/10.1175/MWR2926.1; Manney et al, 2005, JGR, doi:10.1029/2004JD005367; Lawrence et al, 2018, ACP, https://doi.org/10.5194/acp-18-13547-2018; and references therein).

Page 5, line 140: Throughout this subsection, need to say which reanalysis or reanalyses was used to calculate each of the diagnostics and why the same one (or, much better, more than one) wasn't used to calculate all of them.

Page 5, lines 141--144: This is not a useful diagnostic since it is neither related to the polar vortex nor expected to capture the actual minimum in high-latitude temperature in all conditions. While you may argue that this region was inside the polar vortex most of the time during 2019/2020, you cannot make that case for all of the winters you focus on, much less all Arctic winters. Even if this region was in the polar vortex, the location of minimum temperatures (which isn't always inside the polar vortex either since the cold region and vortex are often not concentric in the Arctic) varies a lot both within one season and in between years, so you are almost certainly not comparing the lowest high-latitude temperature at different times in one year or in between years. In the list of years compared, 1996-1997 stands out as being the one that had only modest chemical ozone loss (with the low column ozone in spring 1997 being largely related to dynamical effects including the direct effects of the late period of low temperatures in that winter (e.g., see discussion of and references on 1996-1997 in the supplementary information of Manney et al, 2011, Nature). This distinction is important, particularly when discussing column ozone changes. Finally, the relationship of temperatures in the lower stratospheric vortex in 2019/2020 to the other years with the most ozone depletion, to climatology, and to those in the Antarctic winter, has already been more completely and correctly discussed (accounting for the full region of low temperatures), most completely in Lawrence et al (2020) and Wohltmann et al (2020), so a brief statement citing those papers (as well as DeLand et al, 2020 for the relationship of temperatures to PSC observations) would be quite sufficient and more accurate than including these diagnostics.

Page 5, lines 145: Need to give some references in relation to the effects of wave propagation as diagnosed by the Plumb or other formulations of 3D EP fluxes (e.g., Nishii et al, 2011, J Clim, DOI: 10.1175/JCLI-D-10-05021.1, and references therein.)

Page 5, lines 150-155: It is not clear (here or later) how the discussion of this diagnostic goes beyond that in Lawrence et al (2020). Also need references on the calculation of the NAM index from geopotential height anomalies (e.g., Cohen et al, 2002, Baldwin and Thompson, 2009). If the NAM index is indeed calculated as described here (which description is consistent with the papers mentioned above and with Lawrence et al, 2020) then the range of values in the figure you show later does not appear to make sense (see comment on that figure). (Again, this is a case where it is not clear that the analysis you show of this diagnostic goes beyond or adds anything to that in Lawrence et al, 2020.)

Page 5, line 159--Page 6, line 2: Need to say something about the validity of trajectories as long as 120 days for this purpose. Typically individual trajectories are not considered reliable (even in the lower stratosphere where radiative time scales are long) for more than a couple of weeks, so lengthy trajectories are used only to diagnose large scale motions by using very large ensembles of parcels. It is not at all clear that this purpose -- because you interpolate ozone to individual locations, thus assuming that those locations are relatively precise -- is consistent with that type of usage. Also, as mentioned elsewhere, because the ERA5 ozone is an assimilated product based on combining numerous datasets, one would need to either cite or perform detailed validation before its usage could be considered appropriate for this quantitative usage.

Page 6, Section 2.3: How is “inside” the vortex determined (and which reanalysis is used to do that)? How far inside must the data be for the “well isolated” assumption to be valid? There are many more complete references for effects of descent on ozone in the vortex than Braathen et al (1994; note that you have a typo in that citation), e.g., Tegtmeier et al, 2008, GRL, https://doi.org/10.1029/2008GL034250, as well as numerous other papers cited in the WMO reports (again, the 2006 report has a special focus on distinguishing chemical and dynamical effect in the Arctic vortex). How are the descent calculations done? To be robust, they would have to follow the motion of the air sampled at the time and location of each ozonesonde measurement, since descent is by no means uniform throughout the vortex. If you are calculating a vortex-averaged descent rate that is used with vortex-averaged ozone, for that to be even roughly an accurate estimate, you would have to demonstrate that you have uniform and consistent coverage of the vortex in both the ozone profiles that go into the average and the diabatic descent (which in the latter case includes demonstrating that the descent rate is a reasonable approximation of all the descent conditions the parcels in the ozone measurements experienced). Why are temperatures from JRA (55 presumably) used with an offline radiation code instead of using diabatic heating rates provides with the reanalyses (ERA5, MERRA-2, and JRA-55 all provide these, and it would seem to make more sense to take those from whichever of these reanalyses you use to determine vortex characteristics for the sonde analysis)?

Page 6, line 184: This seems to be very coarse resolution, especially in the vertical. What is the actual vertical resolution in the lower stratosphere where you are focusing on the results? Is this adequate to capture the expected vertical variations / motion?

Page 6, lines 186--187: I am no expert on this, but I would like to see some justification of why it is appropriate / adequate to treat PSC formation as STS during a winter such as 2019/2020 when temperatures were low enough that larger solid HNO3 containing particles were present (and even at some time ice PSCs).

Page 7, lines 194--195: Why north of 64N? This does not encompass the entire vortex, nor the entire region of lowest temperature, except perhaps on some individual days when the vortex is unusually pole-centered -- thus it does not encompass the full region in which PSCs might be expected.

Page 7, lines 202--210: This has been covered more completely in already published papers including Bernhard et al (2020), Inness et al (2020), and Dameris et al (2021). Simply describing this briefly with citations of those (and potentially other) papers would be more appropriate than presenting this as if it were a new result.

Page 8, lines 226: More like the last approximately 60 years, see Lawrence et al (2020) and Matthias et al (2016, GRL, doi:10.1002/2016GL071676).

Page 9, lines 229--237: As mentioned in relation to the methods section, this has been covered more completely and precisely in Lawrence et al (2020), Wohltmann et al (2020) and others. It would be more appropriate to include a brief statement citing these papers rather than presenting this as if it contained new results.

Page 9, lines 242--243: As mentioned already, 1996-1997 did not have severe ozone loss. Moreover, not only did 1996-1997 not have a strong polar vortex (which is by no means synonymous with a cold one) but rather an exceptionally weak one until spring, but also

2004--2005 was notable for being cold and having substantial ozone loss, but having a rather weak vortex that allowed considerable mixing (e.g., Manney et al, 2006, GRL, doi:10.1029/2005GL024494; Schoeberl et al, 2006, JGR, https://doi.org/10.1029/2006JD007134; Lawrence et al, 2020).

Page 9, lines 246--247: What are the implications of this? And what does this add to what has already been shown by Lawrence et al (2020)?

Page 10, lines 256--261: What is new here that goes beyond what was shown by Lawrence et al (2020)?

Page 11, lines 265--286: Again, it is not obvious what this adds to what has already been shown / discussed by Lawrence et al (2020). Also, Figure 4 shows NAM index values up to about 10, whereas Lawrence et al (2020) show values near 5 at the same time and place (their Figure 4a); the values shown by Lawrence et al are typical of those shown in previous work calculating that index based on GPH anomalies. Yet your description of your calculation in the methods section sounds like it is the same index used in these previous papers. Please explain this apparent inconsistency.

Page 12, lines 299--301 (Figure 4 caption): How significant are the differences in “Plumb” fluxes in (b) from climatology, compared to those during similar length time periods in other individual years or at other times in 2019/2020? That is, how unusual is this behavior?

Page 12, line 306 to Page 13, line 311: The “SSW event” you describe was very minor and affected only the upper stratosphere. Although it could be the case that it resulted directly from the enhanced upward wave propagation, you have shown nothing to demonstrate this. You have also shown no evidence that a Rossby wave breaking event occurred in the troposphere nor that if it did, it was associated with the enhancement of wave activity. You have not shown potential vorticity at all, so the reader cannot know if / where it was low. The situation described by Coy et al (2009) was in relation to a major SSW that affected the entire stratosphere for weeks -- there is no reason to believe that the very brief minor event in the upper stratosphere in 2020 that you describe is analogous in any way. (In addition, it is not clear in any of the accompanying discussion, why this minor event that showed no evidence of significantly affecting the lower stratosphere is relevant to the analysis in this manuscript.)

Page 15, line 306 (Figure 6 caption): Why 50-70N?

Page 15, lines 331--344: Please see major comment (3), as well as previous comments on inappropriate initialization locations, need to justify the length of the trajectories for this purpose, and the need to demonstrate (or cite literature that did so) that ERA5 assimilated ozone is appropriate for this purpose. Also, choosing a different initialization date in each year apparently just because that latitude circle happened to be within the vortex is further degrading the ability to make interannual comparisons and the dependence of the results on details of vortex shape, position, and evolution. Again, you do not say how you determine what is inside the vortex.

Page 17, lines 358--359: Do you mean you ran the model with ERA-Interim, or you ran the model with ERA5 degraded to ERA-Interim-like resolution? How large are the differences? If they are large enough so as to make the results highly uncertain, then this points to the need to do something more (typically driving the model with several different reanalyses) to determine whether the results can be considered robust at all. There are numerous papers (eg, in the S-RIP special issue of ACP/ESSD, https://acp.copernicus.org/articles/special_issue829.html) that show substantial differences in results of trajectory analysis and / or chemistry / transort model results from using different reanalyses to drive them. (Just because ERA5 is the newest, does not mean you can automatically assume without testing that it is better for all types of analyses.)

Page 17, line 363: How do you determine where the vortex centre is?

Page 17, line 364: How did you select this PV value? Is this what you use to define the vortex edge previously in the paper, and, if so, what values did you use for the other levels that are shown / discussed? Is the same value appropriate for each of the reanalyses that you use?

Page 17, line 266: See previous comments regarding how representative an average of measurements at a small number of stations is of the entire vortex.

Page 17, line 370: “ozone losses were the lowest on record” -- you must mean “ozone values were the lowest on record” or “ozone losses were the largest on record”.

Page 17, line 372: There are quite a few other papers in addition to Manney et al (2020) that also show this, including Wohltmann et al (2020). In addition if on line 370 you were implying that chemical ozone loss was larger in 2020 than in 2011, then it is not really consistent with those papers, since they estimate chemical ozone loss amounts to be very similar in 2020 and 2011 (but indeed peaking at lower altitudes in 2020).

Page 17, lines 377--378: Why these two winters? What was 2004-2005 “much colder” than (your wording could be interpreted as saying it is colder than 2019-2020, which obviously is not the case)? Since there are several studies (e.g., Kuttipurrath et al, 2010, ACP, https://doi.org/10.5194/acp-10-9915-2010; Livesey et al., 2015, ACP, https://doi.org/10.5194/acp-15-9945-2015) that provide chemical ozone loss estimates for a wide range of years in the past decades, why not compare with all of them. Especially, why not compare with 2011 since it was the previous year that unequivocally had the largest ozone loss?

Page 17, line 380, the terminology “ozone anomalies” is imprecise and potentially misleading, since you are not talking about just any ozone anomaly (which could occur anytime or anywhere for many different reasons), but specifically anomalously low ozone in lower stratospheric vortex that is related to unusually cold conditions there and thus partly to chemical ozone loss. I recommend not using this wording (here or elsewhere in the paper).

Page 17, line 381: There needs to be more information given on the chemistry transport model and its set up (though this would probably be best in the Methods section). How is the model initialized (especially ozone and other important trace gas fields)? What are the boundary conditions for trace gases? What is(are) the initialization date(s)? Why is MERRA-2 used to drive this model whereas ERA5 is used to drive the trajectory model?

Page 19, 393--395: This doesn't look to me like it agrees very well with the OMI data shown earlier. The minimum values in Figure 9 appear much higher than those in Figure 1 on the first three days shown; there appear to be significant differences in morphology, especially, the OMI data on the second day shown suggest that the lowest column values are actually within the polar night, whereas your model results show them to be well away from there, with no low values immediately surrounding the region OMI cannot observe.

Page 19, lines 398--399: There are, in fact, satellite measurements in 2019/2020 that observe in darkness (e.g., MLS), though you would have to compare profiles rather than column (but profile comparisons are a necessary part of validating model results. But in any case, per my immediately previous comment, since the model and OMI observations appear to agree very poorly (in morphology as well as in values) going into the polar night, I fail to see how you can argue that your model results provide useful information in polar night.

Page 19, lines 399--404: This does not make sense to me. First, the OMI data show minimum values abutting the gap in polar night -- meaning the actual minima are inside the polar night. Second, the results of the chemical reactions are transported throughout the vortex, so there is no reason to expect ozone to be lowest in the sunlit regions. Third, in order to interpret these and understand how direct dynamical effects of the low temperatures may be involved we need to know where the vortex is and where the cold region (which is not necessarily concentric with the vortex) is.

Page 19, lines 408--412: How well do these results agree with the PSC observations described in DeLand et al (2020)? And / or with other PSC observations?

Page 19, lines 413--416: As per two comments above, the region of PSCs and that of chemical ozone loss are not expected to coincide since chemically processing air is rapidly transported throughout the vortex and does not remain only in the region with PSCs.

Page 20, Figure 10: It would be helpful to know where the polar vortex is, and where the cold region is to interpret this figure.

Page 22, lines 427--428: How this is calculated should be explained further (probably in the Methods section), and something should be said about how the processes included compare with those in well-validated models, as well as how the chemistry in this model was validated.

Page 22, lines 428--429: Why would the maximum rate of ozone destruction be at the edge of polar night? I would think it would be wherever chlorine is activated (which, when fully activated, is the whole vortex) and there is the most time in sunlight. Since activated chlorine is expected to be quickly transported throughout the vortex, and since the region just outside the edge of polar night receives only a little sunlight compared to regions farther into daytime, I would think the edge of polar night would be rather low in ozone destruction?

Page 22, lines 429--434: Since all of these processes are going on inside the polar vortex, the main effect of polar vortex position is how much of it experiences sunlight, and that is only a significant factor until early March. You have not mentioned here descent (which increases ozone in the vortex and is one of the most important dynamical processes changing ozone), nor have you mentioned the direct dynamical effects by which low temperatures are associated with low column ozone. In addition, none of your statements here have been demonstrated since you never show where the vortex is or where the cold region is.

Page 22, lines 441--449: These lines appear to be an exact repetition of the statement in the Methods section. Delete.

Page 23, line 463: It is my understanding that at the altitudes where ozone contributes most to the column (below ~20--25 km), gas-phase chemistry is very slow (much slower than dynamical time-scales), so I don't understand how it plays such a large role?

Figures 12--15: Why do you not show OMI / model differences for 2019/2020? Why do you suddenly bring in SBUV data to show a climatology rather that deriving that from OMI data for 2005 through 2019? How do OMI and SBUV data compare, are there significant differences? Comparing to a climatology rather than directly to observations in the same year seems an indirect and potentially inaccurate way of assessing what processes are needed to reproduce observed fields. Also, it would help with interpretation of the results if you could show a timeseries indicating where each station is with respect to the polar vortex during the period shown.

Page 27, first two paragraphs of conclusions, and line 555: In the paper, you have not made the case that the very minor SSW event (which affected only the upper stratosphere significantly, hence the absence of any mention of it in the many papers already published on the 2020 lower stratosphere conditions and ozone loss) significantly affected the lower stratosphere. In fact, temperatures in mid-March 2020 were already rising, but did so at a much slower rate than in the vast majority of years, many of which also had only very minor SSWs during this period. While nothing rules out a small effect of this minor SSW on lower stratospheric temperatures, you haven't demonstrated that there was one either -- given the many variables that affect temperatures and the seasonal cycle, the timing of the temperature increases could have been coincidence. Thus, it is not justified that this event should feature so prominently in your conclusions.

Page 28, lines 558--560: Per previous comments, this has not been demonstrated because of the inappropriate choices for initialization of the trajectories.

Page 28, lines 561--563: This has been shown for 2020 in previous papers, with which you should compare your results.

Page 28, lines 564--569: Per specific comments, this has not been demonstrated.

Typos / Minor Points:

For the most part, I am not including details here of improvements that would be needed in the English usage, as the revisions needed to the scientific content are sufficiently major that much of the structure of the writing will be changed. The following are thus just a few things that happened to catch my eye:

Abstract, line 15, “year” should be “years”

Page 2, line 32, “warm” should be “warmer”.

Page 2, line 49, “statically” should be “statistically”

Page 3, line 79, delete comma

Page 3, line 86, “the main SSW” is not appropriate wording, particularly since SSWs are quite common in the Arctic. Perhaps you mean “a major SSW”.

Page 3, lines 91-96, this sentence is very long and convoluted and nearly impossible to parse correctly.

Page 6, line 164, “has” should be “have”.

Page 9, line 245 “strongest weakening” is extremely confusing, please reword.

Page 15, line 340, saying ozone losses were “higher” could be confusing (if the reader thinks of higher ozone values), I’d suggest a wording more like, e.g., “...more ozone loss occurred…”