We thank two reviewers and Dr. Gloria Manney for their comments and suggestions for improving the paper. Our point-by-point responses to the reviewers' comments are given below in blue text, and the revisions are shown in the version of the manuscript with track changes.

REPLIES TO Referee #1 Comments

In their manuscript, Kuttippurath et al. investigate Arctic stratospheric ozone loss during the exceptional winter 2019/2020 from a range of satellite and ground-based observations. Their analysis is thorough and the results are sound. It is, however, less clear to me, what the main message of this paper is. Previous studies, correctly cited in this manuscript, have already come to similar results. So it would be good, if Kuttippurath et al. could spell out a bit clearer what this study adds, that is not already known from these previous studies.

I have one concern with the claims made here that the exceptional ozone loss in 2019/2020 is a sign of climate change. As far as I am aware of the current literature, most climate models do not show any increase in Arctic ozone loss due to climate change. Can the authors rule out that 2020 was not just an extreme winter within the current range of variability? And related to that point: the Arctic did experience in March 2020 ozone hole conditions, as this study demonstrates. Is there evidence of an "exposure of nearly 650 million people and ecosystem to unhealthy ultra-violet radiation levels" (quoting from the first sentence of the abstract)? Or do the authors suggest that future Arctic winters could show even larger ozone depletion? And if so, on which basis? The authors should try to make these points clearer.

Thanks for the comments

We would like to emphasize that we wanted to describe the polar processing and ozone loss in the winter 2020, as agreed by the reviewer that "Their analysis is thorough and the results are sound". We agree that there are studies on this winter, as mentioned. However, it is important that we need multiple studies using different data and model simulations on different as aspects the winter, which would help assess the winter to assess the impact of climate on winters and aid modelling and forecast of such winters in future (please also see our replies to referee #2). We have removed the statement on climate connection now.

The paper is overall generally well written, but could be made clearer at several points. See my specific comments below. I recommend the manuscript for publication in Atmos. Chem. Phys. if the authors can address my general and specific comments.

Thank you.

Specific comments:

P1, l13: "Severe vortex-wide ozone loss in the Arctic would expose nearly 650 million people and ecosystem to unhealthy ultra-violet radiation levels." The number of 650 million people does not appear in the body of the manuscript and is not backed-up by any citation. So I suggest removing this explicit statement here from the abstract.

Done. We have removed the sentence in the abstract as suggested.

p1, l22: "the very colder Arctic winters in the near future will experience even more ozone loss": do you mean Arctic winters in the near future will become colder? On what basis is this claim made? Or do you mean the coldest Arctic winters in the near future within the current range of variability? But why should they experience very likely even larger losses?

Done. We meant that it is very likely that the colder winters might get more colder. We have removed the climate connection statement and "cold winters colder with more ozone loss" speculation in lines 21-22.

p1, l22: language: "Our study suggests that the very colder Arctic winters in near future would also very likely to experience even more ozone loss and encounter ozone hole situations, provided the stratospheric chlorine levels still stay high there." -> "Our study suggests that colder Arctic winters in the near future would likely experience even more ozone loss and encounter ozone hole situations, as long as stratospheric chlorine levels remain high."

Thanks. Done. Rephrased as suggested, lines 21-22.

p1, l30: why did the Antarctic ozone loss peak in the late 1980s when polar stratospheric chlorine loading peaked around the early 2000s

There were already sufficient chlorine in the Antarctic stratosphere to reach the loss saturation. Therefore, even if there is more chlorine in the stratosphere that wouldn't affect the ozone loss saturation anymore, but the ozone loss will be modified by the inter-annual changes in meteorology of the winters. This is mentioned in line 30.

p2, l42: "e.g., > 1.5ppmv of loss" seems arbitrary. Please motivate this value

Done. This is the average ozone loss value taken from different literature that have multi-year analyses. Please find the revised statement with references in lines 42-43.

p2,l43: 25-30% in which metric? The statement on 1.5ppmv above clearly refers to loss at a certain altitude. On a given altitude, previous Arctic winters (such as 1999/2000 or 2010/11) experienced losses far greater than 25-30% (e.g., Sinnhuber et al., 2000; Sinnhuber et al., 2011). Please be specific which metric this refers to: column loss with the vortex, column loss poleward of a certain latitude, local loss,

Done. These are mostly from satellite (Livesey et al., Kuttippurath et al., Manney et al.), sondes (Rex et al.,) and ozone column from the ground (e.g. SAOZ). The source and respective reference with rephrased statements can be found in lines 43-48

p2, l46: "short-lived" is what sense?

Done. The Arctic vortex is mostly in tact from December through mid-February or early March. This is what we meant by short-lived. This is short as compared to December through end of April vortex this year. This is mentioned in lines 50-51.

P2, l50: "ozone loss is found to be proportional to the timing of the major warmings": I think I know what you mean, but this statement is not very clear

Done. We mention that the vortex longevity is a factor for prolonged ozone loss, although 1997 is an exception. This is rephrased in lines 57-58.

P2,l53: "The occurrence of extreme events is a signature of climate change and so are the extreme cold winters with large loss in ozone (e.g. IPCC, 2007)" Sorry, it may be true that under a changing climate the occurrence of extreme cold winters may increase, but it is not at all clear if there is a trend towards more extreme events in Arctic stratospheric temperatures and whether or not it is related to climate change! This statement is not backed-up by IPCC, 2007.

Done. As mentioned earlier, we have removed the climate connection statement, as it was confusing. The IPCC report was quoted for the occurrence of extreme whether events as polar meteorology is a key element for sustained ozone loss. Please find the revised statement in lines 21-22, 64-65.

p2,l62: would be good to have references for the data sets

Done. Please find them in lines 107, 112, 114 and 115

p2,l64: latitude and longitudes swapped for Alert

Sorry for this. Corrected in lines 80-81.

p2,l65: Do the 5-10% refer only to the sondes, or also to the satellite profile data?

This is mentioned for the satellite measurements here. This is mentioned in line 81.

p3, l70: GOME -> GOME-2 ?

Done. Please find it in line 86.

p3, l74: "and other trace gas profiles": which?

Done. Other trace gases are ClO, N_2O, and HNO_3. This is mentioned in line 99.

p3, l75: does OMPS provide temperature profiles?

This is mentioned here for MLS and for OMPS. We have used the ozone profiles and column value provided, mentioned in line 92.

p3, l80: what is poisson_grid_fill ? Reference?

Done. We are Sorry. This is a normal linear interpolation procedure done with Python software. We have removed it now since it is a function in python for the data processing.

p3, l84: if the precision varies so strongly, maybe better to give percentage uncertainty?

We have mentioned the uncertainty in absolute values because there are biases as well, as suggested by the validation papers. Thank you.

p3, l87: not clear to me how well justified this extrapolation is. Does this extrapolation takes into account the tropospheric N2O VMR?

Done. Yes, it accounts for tropospheric values too, mentioned in line 104.

p4, l114: "longest winters"? You mean latest vortex break-up? Or coldest winters?

Done. We meant the winters with long-lasting vortex up to April, mentioned in line 130.

p4, l129: “PSC area” -> “potential PSC area”. Please make clear, that this is not area of observed PSCs but area of temperatures cold enough for formation of PSCs.

Done. This is rephrased in line 149.

p5 , l133: “in 40 years”: where there colder temperatures before, or are these the coldest ever observed?

Done. Yes, as per the MERRA-2 data as shown in Figure 1.

p5, l133: “the largest ice PSC “: This is likely not a single cloud, but an area of temperatures cold enough for the formation of ice PSCs

Done. Yes, corrected in line 153.

p5, l143-150: This general discussion of the relation between wave activity and vortex strength can be moved to the introduction.

Done. This is now in Introduction in lines 51-58.

p5, l159: “occupied the entire polar region”: how do you define polar region? North of 60N? Or entire vortex?

Entire vortex, mentioned in line 173.

p6, l165-168: can be removed, redundant

Done. Removed.

p6, l188: didn’t Rex define APSC and VPSC as the temporal integral of PSC area and volume, respectively?

Done. This is mentioned in line 208.

p7, l196: From Fig. 3: I don’t see a gradual descent of loss from the middle stratosphere to the lower stratosphere: I see some (small) loss above 600K in December and much larger losses in the lower stratosphere (below 600K) beginning in December as well and intensifying during January. Or does this statement refer to earlier winters?

In fact, there will be ozone loss at higher altitude in December (below 0.5 ppm) and then progresses to lower stratosphere as the winter/spring advances. This is common to all winters, but there will be differences in values of ozone loss. This is what we mentioned in lines 217-218

p7, l200: chlorine activation does not require sunlight, but high levels of ClO do

Yes, this is rephrased for clarity in line 220.

p7, l216: the high levels of ClO in air masses with low PV are very surprising. The high ClO suggests that the reductions are not only “dynamically driven”? Would be great to have a bit more discussion at this point.

Done. We have added a small description of the process in lines 230-234.

p7, l220: citations seem out of place

Done, corrected in line 256.

p9, l264: "chlorine activation and ozone loss is limited to the winters with very low temperatures in December–February" this statement is somewhat incorrect

Done. Yes, ozone loss can always be there, but the loss is significant or not is the point. This is rephrased in line 301.

p9, l268: "ozone loss in the winter 2011 was about 1.0 ppmv (or 30–40 DU),": I don't understand what these numbers refer to. E.g., Sinnhuber et al., 2011, derived maximum ozone loss in Arctic winter 2010/11 of more than 2ppm at 19km and more than 120 DU column loss. Is this what is meant in the next sentence: "which is higher than that of other Arctic winters (about 2.1–2.3 ppmv or 100–100 DU)"? (100-100DU is a typo anyway, I guess.) Is the first sentence then referring to loss before February only?

Done. These measurements are mostly from satellite (Livesey et al., Kuttippurath etal., Manney et al.), sondes (Rex et al.,) and ozone column from the ground (e.g. SAOZ). The source and respective reference with rephrased statements can be found in lines 42-48, 302-307

p9, l77: "undoubtedly" is a strong word. I suggest to remove.

This is removed now.

Fig. 5: I couldn't find for which period in the given years the ClO profiles are shown. Are these maximum values or temporal averages? Any idea why ClO is so much higher above 550K in the Antarctic in 2019 compared to 2015 – keeping in mind that 2019 was a rather warm and disturbed Antarctic spring?

Done. The shown profiles are selected such that they represent the maximum observed ClO in each winter. Then we have averaged the profiles three days (max day +/-1 day) to avoid any error in single day measurements. This is mentioned in lines 828-829.

The ClO profiles have a broader peak in the Antarctic; it is not only for 2019, but for all Antarctic winters, because of the meteorology and strong Chlorine activation there. This can be seen in Kuttippurath et al. (2015), who have done the analyses for 10 Antarctic winters here. More detailed analysis is needed for this Antarctic winter. We will do that in a separate paper. Thank you.

Section 3.5, Fig. 6: When discussing the maximum ClO amounts in the past winters, it would be interesting to put this into context of the EESC (or similar metric): By how much has total chlorine (or EESC, …) decreased between 2005 and 2020?

Done. Its 246 ppt/year. This is mentioned in lines 343-346.

p11, l346: how exactly is saturation ("complete loss of ozone") defined here? In reality ozone is of course never completely gone. Okay, I see further down at l359 that you define this as below 200ppbv with a reference to Smit et al. (2007). I believe it would be good to include a brief justification here, why this is a useful definition for loss saturation.

Done. The definition is depending on the detection limit. This detection limit is 10-20 ppbv. An explanation is given in lines 398-399. The reference is also given there.

p11, l347: Again, I don't understand the meaning of "ozone loss normally happens only up to 25–30% in the Arctic winters". Local loss in previous cold Arctic winters was clearly larger than 30%.

Done. These are taken from Gautail et al., and Pommereau et al. Yes, these are from the ground-based spectrometer measurements. This is the only long-term ozone loss estimate available in the Arctic, which is why these are mentioned. This is rephrased in lines 434-435

Fig. 5: Please indicate the dates for the sonde profiles.

Done. Please find the dates in figure caption (8 April and 10 April 2020), line 833.

p12, l362: "the loss saturation suggests that the Arctic has entered an exigent climate change scenario": again, it is not self-evident, why this is a sign of climate change and not just an extreme winter within the range of variability. Same comment applies to l403.

Done. We have removed the sentence.

p12, l380: Just for curiosity: why are GOME measurements more restricted in latitude than OMI or OMPS? I thought all three use similar wavelengths ranges?

It depends on the orbit and elevation of satellite.

p12, l389: contradiction: in the previous sentence it is stated that a column loss of about 90-120 DU occurs in extreme winters such as 2005 and 2011 and in the next, the largest observed loss was 100 DU in 2011. Sorry, but these small contradictions are very confusing and make for a tiresome reading.

Done. Sorry for that. This is rephrased and corrected now Griffin et al., and Livesey et al. have complied the loss estimates for different winters. Average values from those studies are shown here. . Please find it in lines 436-437.

P14, l437: "Extreme weather events are harbingers of climate change": See comments above on extremes and climate change.

The sentence is removed.

Technical corrections

p2, l61: ->"We have used two satellite ozone profile datasets."

Done. Please find it in line 77.

p2, l77: ER5 -> ERA5

Done. Please find it in line 92.

p2, l78/79: active and passive voice changes

Done. Please find it in lines 94-95.

p6, l186: ozone AND N2O

Done. Please find it in line 205.

p7, l227: present IN all

Done. Please find it in **line 263.**

p9, l286: Wohltmann

Done. Please find it in **line 324**.

p12, l369: by Nash et al.

Done. Please find it in **line 416**.

p13, l404: not present continuously

Done. Please find it in **line 453**.
