We thank the 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 #2 comments

I struggled with this review because while the paper presents a lot of information, and the authors have done a lot of work, it is difficult to see what is new. The major discoveries announced in the abstract, for example, "Sporadic occurrences of ozone hole values … record-breaking ozone loss of about 2.0–3.4 ppmv … unprecedented chlorine activation … first-ever appearance of loss [near] saturation in the Arctic…" have been made, and published, by Manney et al. (2020) or Woltmann et al. (2020). The title ("on the verge") suggests that the authors think we will see a real ozone hole, or more ozone holes in the future. But they fail to present any dynamical arguments for that. Figure 1a of Woltmann et al. (2020) in fact suggests this, so it would be interesting to see a climate model prediction of the like, or more long-term analysis of trends in dynamical parameters.

Thank you for the comments and suggestions.

The Arctic winter/spring in 2019/2020 is unique in terms of the stratospheric meteorological conditions and its consequences in the very low polar stratosphere ozone, which has also discussed by other studies in JGR/GRL special issue. However, we would like to provide a comprehensive analysis to investigate the polar processing and ozone loss in the Arctic winter/spring 2019/2020 using different observations and reanalysis datasets as well as model simulation, which would help to assess the impact of climate change on winters and aid modelling and forecast of such events in future.

Our study is very important in this context, as we have used six different satellite measurements to describe the chemistry, two different methods to estimate the ozone loss, two different meteorology analyses to explain the dynamics, analysis of the ozone mini-hole situation, multi-satellite measurements of observed very low total column values and observation of saturation of ozone loss. All our results are supported and confirmed by different measurements and methods, though most of the conclusions are consistent with other related studies. We have removed the statement on climate connection.

Instead, the paper waffles back and forth about whether there was an "ozone hole" in 2020, even contradicting itself, e.g., "all the methods, data, and parameters converge to provide an undeniable fact of the first-ever ozone hole" and then "the ozone loss in the Arctic cannot not be called as "an ozone hole"." In any case, this is not an important scientific argument, but a quibble about terminology.

Thanks for the concern. We agree that the "ozone hole" is not an important scientific argument but how to make the definition for the Arctic. However, we have never used any statement that confirms an ozone hole in the Arctic. We have used "?" where the ozone hole is mentioned, even in the title. Also, there is a section on whether there is ozone hole or not. We have clearly stated that the ozone loss in the winter cannot be called an ozone hole. To avoid the confusion, we have removed the ozone hole statements from the conclusion now, Title, Section 3.8, lines 422-423, 452-454, 476-478

Figure 3 is very nice, with a lot of important information, but that information is already in Manney et al. (2020). The ozonesondes, and the degree to which loss saturation was approached, are thoroughly presented in Woltmann et al. (2020). Loss saturation was never reached, in fact: Antarctic ozone hole profiles frequently show ozone below the detection limit of the sondes (~1 ppbv), while the lowest observed last spring in the Arctic was 125 ppbv. Sondes can measure ozone levels below 100 ppbv with good accuracy (as they do in the troposphere).

Thanks. Though a similar information about the time series of observed ClO, HNO_3, N_2O and O_3 from MLS is shown in Manney et al. (2020), it is very important to discuss the changes of these species involving the polar processing in the winter related to ozone depletion, both dynamical situation and chemistry. Figure 1 describes the dynamics and Figure 3 shows the chemistry, including ozone loss. We have used two different method and three different data sets together in this study to analyse the chemical ozone loss, which is the novelty of this study. We believe that we need different types of analysis for each winter to assess the chemical and dynamical situation to assess the changes in the region. Including Figure 3 in the current version will also provide important information for readers to understand these processes.

Done. It has to be noted that we have not seen any comparison or literature showing 1 ppbv as ozonesonde detection limit. However, there are studies claiming the detection limit as 10 ppbv, but we agree that the studies have used 1 to 40 ppbv as detection limit in different studies (Solomon et al., 2005; Vömel and Diaz, 2010). Here, we use 10 ppbv for this and 0.2 ppmv as the saturation of ozone loss, which is the loss incurred only by the loss cycles in the lower stratosphere. This is mentioned in lines 398-399.

I did, however, find the discussion of the ozone mini-holes in December 2019 and January 2020 quite intriguing, especially the observation that they contained high ClO. Mini-holes are generally regarded as dynamic phenomena, so the suggestion that heterogeneous chemistry is occurring is interesting. It might be interesting to explore this further: are they also becoming more common? Do they affect the overall loss of ozone? See also Stenke and Grewe (2003). I noted that in Figure 7 the mini-holes were the only point where TCO fell below 220 DU. That seems worth exploring.

Thanks. Arctic winter 2019/20 is quite interesting including the miniholes in Decemer 2019 and January 2020.

We covered the mini-hole episode in this paper as we wanted to describe the polar processing in the winter thoroughly. In addition, this mini-hole was particularly important as it was slightly modified by the chemistry we have done the trajectory and ClO analysis to complete analyses on this year's mini-ozone hole episode. Anything beyond this demands substantial work on modelling, which is beyond the scope of this study.

Yes, the early winter ozone hole values were related to ozone mini holes, and we have clearly mentioned them. However, as suggested, we have given a brief description of mini-holes that occurred in the past, in lines 236-248.

I also appreciated the long-term comparison with previous years in Figure 6. Perhaps this could expanded, along with an analysis of the long-term changes in vortex temperature, V_PSC, wave disturbances/stability…

Thanks for the suggestion. It is included now. Although the long-term analysis is presented in Figure 1 and Figure S2, we have made another long-term analysis with all dynamical parameters and presented in Figure S1b as suggested. The discussion is given in lines 177-181.

Minor points:

Line 23: "provided the stratospheric chlorine levels still stay high there." I don't think there is much uncertainty about future Cl levels.

Done. We have corrected this in lines 21-22.

Line 34: "because"? Perhaps "possibly because". This is far from certain, or we wouldn't still be producing ozone assessments. In fact a lot of data show the opposite (decline since 1997).

Done. We meant that it is very difficult to estimate or find a trend in the Arctic data as it is very difficult to differentiate the dynamical contribution from the data. We have changed the sentence now. Please find it in line 34.

Lines 58-59: " Here, we show that the Arctic winter in 2020 … met the condition for an ozone hole for the first time". What condition? This disagrees with most other assessments (e.g. Woltmann et al., 2020; Manney et al., 2020; Wilka et al., 2021).

Done. We have rephrased this. We have removed the term ozone hole" and changed it to "very low ozone" values below 220 DU were there for more than 23 days, and the ozone loss was also record-breaking in the lower stratospheric altitudes below 500 K. This is the situation. All three references mentioned in the comments agreed to this situation, but they were not using the word "ozone hole", although Wilka et al. used the word, as we did in this work.

We have reprahsed the sentences that use "ozone hole" now, including title, Please find the revised **Title, Section 3 .8, lines 422-423, 452-454, 476-478**

Lines 60-65: Should indicate where the data were obtained. Uncertainties are quoted but no citation is given.

Done. Data details are given in ***Data availability*** statement. Citation is mentioned **in line 81.**

Line 80: "The missing values in satellite measurements were filled with linear interpolation (poison_grid_fill)." What is "poison_grid_fill"? How does it work? What are the criteria used for filling?

Done. This is a function with python for the linear interpolation. We have deleted it because it is a method for the data processing.

Line 120: A lot of this paragraph is confusing, but this line especially. T_NAT is 195, not 200 K.

Done. Please note that we were not talking about T_NAT, but the temperatures below 200 K in the winters, which were uncommon during the period. However, note that temperatures below 200 K also include temperatures below 195 K, which is why PSC temperatures are also mentioned. We have rephrased this in **lines 132-134** to make it clearer.

Line 133-134: "This is the largest ice PSC ever observed in terms of its area, volume and number of days of appearance (i.e. frequency) in the Arctic and the area is twice that of the winter 2011." So what? This information is never used for anything.

Done. It was showing the extreme meteorological situation in the winter and was also the reason for the large loss in the lower stratosphere as compared to 2011. This is mentioned in **lines 338-339**.

Lines 151-156: This is interesting, potentially, but vague and hand-waving. It could be really valuable to have an analysis that looks at the evolution and variation of the Arctic vortex over the last 20+ years.

Done. This is shown **in Figure S1B** for the past 41 years and described in **lines 177-181**.

Lines 210-212: This analysis might make an interesting paper, if expanded.

Thanks. We have analysed the ozone mini-hole in the winter, its temporal evolution, air mass transport, and analyses with ozone measurements by satellite and ozonesondes. Anything beyond this is modelling of mini-holes, which is beyond the scope of this paper, and warrants a separate study and we would do that. However, we do respect the suggestion, and we have presented a brief description of the mini-holes, in **lines 236-248.**

Lines 360-364: This interpretation is incorrect. The sondes do indeed have an uncertainty of about 10%, but that means that the minima of 0.125 (or 0.200) ppmv would have error bars of ±0.012 (or ±0.020). That is not consistent with zero, or even 0.1 ppmv.

The 5% accuracy is noted from the ozonesonde JOSIE intercomparison project and Smit et al. (2017). We could not find any document stating this 10% uncertainty. We have, however, stated this as 5-10% in **line 406.**

Reference: Stenke, A., and V. Grewe (2003), Impact of ozone mini-holes on the heterogeneous destruction of stratospheric ozone, *Chemosphere, 50*, 177-190, https://doi.org/10.1016/S0045-6535(02)00599-4

Thank you. We have cited this paper, **lines 229, 247.**

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