

## RESPONSE TO THE EDITOR AND REFEREE COMMENTS

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**General comments:** The track changes document looks incomplete. There seem to be several new pieces of text which are not highlighted in it. In addition, omitted pieces of text are not included in the track changes. This makes the review of the revision complicated and makes it very difficult to judge if the concerns risen by the referees were considered. Please provide complete track change information on the next revision:

Thank you very much for your time, review and comments on MS. Please find answers to specific comments below. **Please note that we have given the line numbers of the answers or changes in the MS for each query to identify the respective responses.** Please find the response to queries and the corresponding change in the MS in **blue typeface**. We do hope that the Editor and referees will find the revised version more interesting and recommend a publication very soon.

We have again made the track-change version of MS and all changes shown are in blue typeface. Please note that there were some typos, grammatical corrections and rephrasing sentences, which are not shown in track-change mode.

**Content:** Referee #1 has asked about your mentioning of the influence of ozone depletion on precipitation. This is mentioned in the abstract, but it does not seem to be discussed in the introduction. Please add with references.

**Done.** Yes, there are studies discussed about the precipitation and ozone hole connection. This is discussed now in the revised MS. For instance, please see the references Kang et al. (2011), Kang et al. (2013), Thompson and Solomon (2002) and Gillett and Thompson (2003). We have mentioned this and cited the articles in the introduction. Please find **lines 49-55**.

I agree with the referee that your usage of the term "observed ozone loss" is confusing as you derive it from an observed and a modelled quantity. So there is no direct observation of ozone loss here. Please use a more appropriate term such as e. g. "derived" or "inferred". Line 18 in the first page, it is not correct to say "quantifies the ozone loss.. using satellite measurements" because the authors also use the passive ozone from the REPORUBUS model.

**Done.** This is rephrased as suggested, "inferred ozone loss". Please find it in **line 84**.

L 69: Please add the URL that you used in your response to the referee to the manuscript or a reference to a peer-reviewed publication to justify the values mentioned

**Done.** We have added the link with the description in the Data and Methods section in **line 75**.

L 202: check reference: the author's name in the text does not agree with the list of references. The citation is incomplete.

**Done.** We have made the correction in **line 205**.

Table 1: there does not seem to be a reference to the new Table 1 in the text. Please discuss the information presented in the table and reference it.

Done. Please find the reference to Table 1 in **line 216**.

Figure 4: In response to a comment from Referee#1 you omitted the tracer simulations up to 10 June 2018 and 20 July 2019 in the respective figure panels. 1 What was the reason for the choice of date? How about the initialisation phase for the other years? How about the year 2020 when ozone loss numbers presented in Fig 4 also looked very different at the beginning of the season? The new Figure 4 has a modified color scale but this feature is still well visible but not commented on. What is the purpose of the isolines highlighted in black in the new version?

Done. We have noticed that the model tracer simulation was not initialised on 1 July (as it should be for all winters) and therefore, there is a bias in the tracer simulations. However, the simulations looked reasonable after these dates in all years. The bias and initialisation problems were identified after comparison with the measurements (measured and simulated ozone, and simulated tracer). Therefore, we did not use the tracer simulations for ozone loss computations in these years. In 2020, the tracer was initialised on 1 April 2020, and was too early. So, we have corrected the simulations with respect to the measurements, and presented the results here. Except these years, tracer simulations for other years are correct and validated. This has been stated in the MS in **lines 82-87, 340-341**.

The black lines drawn only for comparison reasons among the winters. The horizontal and vertical lines are also drawn for the same reason and are the periods of maximum ozone loss and peak chlorine activation days and altitude range. This is mentioned in the figure captions, **lines 323, 341-342**.

Data availability statement: please add doi information or URL for all publicly available data

Done. We have added the url in the Data availability statement, **lines 253-255**.

I don't see where/how you addressed Referee #2's question about mixing up MERRA-2 and ECMWF analysis data. In Lines 64/65 it is stated that MERRA-2 meteorology is used while lines 72/73 state that the REPROBUS model is driven by ECMWF operational analysis. Referee #2 has asked why you did use the meteorology that drives the model also for your analysis and I do not see that question answered, neither in your response nor in the revised manuscript.

Done. Please note that REPROBUS is standard, stabilised, widely-used and well-established model and driven by ECMWF data. The model has been running with the data for years, and is stabilised. We used the MERRA-2 data for meteorology because all required parameters (fluxes, waves, temperature and winds) are available in high resolution, which was not the case for ECMWF (operational data that used in the model). Therefore, to make a comprehensive analysis on the meteorology of the winter, including the status of PSCs, we had to use MERRA-2. Please note that many previous studies also use the MERRA-2 data for meteorological analysis in the polar winters, which would also facilitate the comparison of our results with the previous analysis. Therefore, we used the MERRA-2 data. Hope, the editor and referees agree to that. This is mentioned in **lines 67-70**.

In fact, we have taken the PSC area calculated from MERRA as available from <https://ozonewatch.gsfc.nasa.gov/>. We apologize for the confusion. This is mentioned in **line 75**.

I don't see a sufficient discussion of the mixing up latitude with equivalent latitude raised by Referee #1

Methods. Since the loss calculation is based on the equivalent latitude (**Line 95 in page 4**), the authors still use the geographic averaged latitude to do other calculations (for example, temperature, PSC etc.). I would suggest the authors use the same criteria to re-make the figures.

Done. Please note that these types of analyses are mostly performed for polar cap temperature, winds, etc, which is why we have presented the analysis this way. These are also needed to check major and minor warming criteria, as we presented in **lines 104–107**. In addition, this is also needed to compare with previous studies (e.g. **lines 106–107**). Therefore, we have kept the original analyses for Figures 1 and 2. However, we respect the referee's comment and we have done the temperature and wind analysis inside the vortex and it is presented in Figure S1 and related text in **lines 140–146**.

choice of words regarding "temperature growth" etc.: please provide an accurate track of changes made for assessment.

Done. We have removed those words. Please see the track-change version of MS. Thank you.