Authors response:

We are thankful to both reviewers for their thorough comments. We have added a few paragraphs to the introduction to better clarify previous studies that are relevant to this manuscript and to elucidate remaining research questions that this study focuses on. A few graphs have been updated to have higher resolution and to include statistics, as suggested by the reviewers. We have used more clear language to highlight the novelty and main findings of this study. We applied most of the reviewer's suggestions and provide justification for those that we did not include. We believe that the quality of this manuscript has been greatly improved by these reviews.

Our detailed responses are given below, as well as comments on the full manuscript.

To facilitate reading of our responses and tracking changes in the full manuscript, we have labeled each of the reviewer's comments using the following labels:

List of labels:

reviewer 1 (Kris Wargan) :

- General comments (GC+#)
- Specific comments (Spc+#)
- Technical corrections (Tc+#)

reviewer 2 (Simon Chabrillat):

- General comments (GCC+#)
- Tables and figures comments (TF+#)
- Minor Comments and typos (MC+#)

We also used different fonts, sizes and colors: Reviewer comments are in black in Calibri font, our written responses are in blue in Calibri font, and the updated text in the manuscript is in cyan in Time New Roman font using a small font size.

Review of the manuscript "*Analyzing ozone variations and uncertainties at high latitudes during Sudden Stratospheric Warming events using MERRA-2*" by Shima Bahramvash Shams et al.

This study examines the evolution of polar ozone during six recent major sudden stratospheric warming events (SSW) using the MERRA-2 reanalysis. The analysis is preceded by evaluation of the MERRA-2 ozone using independent observations and focusing on the region and periods of interest, which provides a nice complement to previous validation studies that looked mainly at the global picture. The authors find that the impacts of SSWs on ozone were largest in 2009 and 2018 ("elongated vortex" cases) compared to the other events ("displaced vortex") and identify vertical advection as the key mechanism responsible for the formation of these positive ozone anomalies during all the SSWs analyzed in this study.

This paper is interesting and certainly suitable for publication in ACP subject to some minor revisions as delineated below. It's nice to see a detailed study of a number of recent SSWs in one place along with an ozone budget analysis for each of them. It certainly adds to our understanding of the role of transport during these events. It's also great to see another paper that demonstrates the value of reanalysis ozone data. It is only recently that our community began to take advantage of the global coverage, high resolution and dynamical consistency that these products offer. The additional validation of MERRA-2 ozone, focused on the region of interest is especially valuable. While I ended up having a rather large number of comments and suggestions, none of them are serious objections, and I believe all of them can be easily addressed. The most important ones concern the need to highlight the novel aspects of this work in the context of other similar studies (some of them not cited) (general comment #1), the choice of these particular six events and a possibility of adding 2021 to the list (general comment #2), and terminology (#3). I hope my comments will be useful.

General comments

GC1. Can you place your results in the context of previous papers discussing the role of dynamical ozone resupply, e.g., Tegtmeier et al., 2008; Strahan et al. (2016)? It would help if you clearly delineated the novel aspects of your study against the backdrop of the existing literature of the subject.

Response:

We have added multiple paragraphs in the introduction to provide better connect to previous studies, which now focus our manuscript on key research questions. Also, some sentences are added to the abstract to provide better focus on the key findings: the average shape of polar vortex before SSW, EPV changes to the geographical extent, timing, and magnitude of ozone changes, and, finally, the magnified vertical advection in the elongated vortex shape.

"From 2004 to 2020, six major SSWs persisted (persistent easterly winds at 60ºN 10hPa) for more than two weeks with each of these events having significant impacts on Arctic ozone. Since 2004, the number of stratospheric observations has increased, and various studies have focused on individual SSWs, their evolution, and their impact on trace gases. For example, Siskind et al. 2007 investigated trace gas (CO) descent from mesosphere to the upper stratospheric layers during the SSW event in 2006, using the Navy Operational Global Atmospheric Prediction System–Advanced Level Physics, High Altitude (NOGAPS-ALPHA) model, along with observations from the Sounding of the Atmosphere with Broadband Emission Radiometry (SABER). Manney et al. (2008a) investigated the evolution of the SSWs in 2004 (minor) and 2006 by focusing on the transport of traces gases, including CO, H_2O, and N_2O using Microwave Limb Sounder (MLS), SABER, and ACE-Fourier Transform Spectrometer (ACE-FTS) at Eureka Canada. The evolution of the 2008 SSW and its associated changes in ozone and water vapor over northern Europe and, specifically, Bern, Switzerland was studied using the ground-based microwave radiometer and ozone spectrometer measurements, as well as MLS and Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO) measurements and meteorological data from reanalysis systems (Flury et al. 2009).

Manney et al. (2009b) used MLS and GEOS-5 data to discuss the dynamics and evolution of trace gas transport (CO, N_2O, H_2O) during the 2009 SSW event with a split polar vortex and compared it to the 2006 SSW with a displaced vortex. They confirmed a more rapid changes in trace gases during the split vortex event compared to displaced vortex, similar to a previous study by Charlton and Polvani (2007). Tao et al, (2015) showed the significant impact of dynamical forcing in variability of N_2O and O_3 during the SSW in 2009, using chemical Lagrangian Model of the Stratosphere (CLaMS) simulations and tracer-tracer correlation.

Using CALIPSO and trace gas data (N_2O, HCL, HNO_3, CLO, and O_3) from MLS, and MERRA meteorological fields, Manney et al. (2015) showed that during the 2013 SSW, the persistent spring vortex, after it split in the lower latitudes and was exposed to sunlight, caused record ozone depletion in the Northern Hemisphere. Schranz et al. (2020) investigate the impact of the SSW in 2019 on ozone and H_2O over Ny-Ålesund, Norway, in particular, and the northern hemisphere, in general, by analyzing the ground-based microwave radiometers, MLS measurements, MERRA-2 and climate simulations.

de la Cámara et al. (2018) analyzed the climatological impact of SSWs and their associated changes in stratospheric transport using ERAI reanalysis and WACCM simulations. They showed the associated changes in residual circulation and isentropic mixing and emphasized the impact of mixing on atmospheric composition in the lower stratosphere. The composite mean ozone changes during SSWs and associated chemical and dynamical conditions is also discussed by de la Cámara et al. (2018b).

While the above summarizes the studies that have looked at individual or composite SSW events the relative magnitude and extent of these events and their specific impact on ozone have not been compared to each other. How do the observed changes in Arctic ozone during each of the SSWs compare with the simulated climatology? If there are major differences associated with these events, do they fit into certain categories? What physical parameters modulate the different impacts of SSWs on Arctic ozone?

To our knowledge, no previous study has investigated these questions. Therefore, this study investigates the dynamical variability and ozone variations at northern high latitudes (between 60°N and 80°N) using the MERRA-2 dataset, both in the zonal average and within a specific geographical region during six persistent, major SSWs. We show that the magnitude, geographical extent, and timing of ozone changes are connected more closely to the averaged polar vortex shape before the SSW event rather than the final form of the vortex after breakdown (split vs displacement). We also show there is strong correlation between changes in average equivalent potential vorticity (EPV) and ozone column changes during these SSWs at high northern latitudes."

As Tegtmeier et al., 2008 and Strahan et al. 2016 both focus on springtime impact on ozone when chemistry is significant modulator of Arctic ozone in the middle stratospheric, we added these articles in our methodology section and discuss why chemistry is not significant modulator of ozone changes during winter over polar night.

"The contribution of dynamical and chemical drivers of ozone anomalies varies throughout the year. During springtime, both dynamical resupply and chemical depletion strongly modulate ozone changes. Assuming an isolated polar vortex and neglecting isentropic mixing, a previous study showed a similar magnitude of influence from chemical ozone depletion processes and dynamical ozone supply during the springtime (Tegtmeier et al. 2008). However, Strahan et al. (2016) used a chemistry and transport model to show that dynamical processing affects ozone changes by a factor of two more than chemical processing during March. However, chemical processes are not significant drivers of ozone changes in the middle stratosphere from November to February in the Arctic because of the polar night (de la Cámara et al. 2018b). Moreover, it has been shown that during years with SSWs, Arctic ozone depletion is significantly diminished (Strahan et al. 2016). However, if prior to or during the SSWs, the polar vortex moves outside of the region of the polar night (to lower latitudes), ozone depletion will occur as shown in the 2013 SSW by Manney et al. (2015). By limiting our analysis to latitudes between 60ºN to 80ºN, this impact is minimized in our analysis."

GC2. Butler et al. (2017) as well as the SSW Compendium (https://csl.noaa.gov/groups/csl8/sswcompendium/majorevents.html) list several more SSWs than those discussed in the paper: 2007, 2008, and 2010. I assume there's a good reason for the selection discussed in this study to be what it is, but it needs to be explained, especially since the definition of SSW used here is the same as that applied in the Compendium. In addition, ideally, I would like to see 2021 added to the analysis. Seven events are better than six. Note that it would likely be the first paper that talks about ozone during the 2021 SSW.

Response: In this study, we focused on persistent SSWs with at least 16 days. As shown by Lee and Butler (2019), the average duration of SSWs is 12-13 days. The SSW in 2008 has been part of our analysis as the least strong SSWs with 16 days of easterly days. The SSW in 2007 exhibit only 4 days of easterly zonal mean zonal winds. The easterly zonal mean winds last for 9 days in Feb 2010. Moreover, the increase in stratospheric temperature in 2010 that occurs in mid-January is not synchronized with the wind reversal. Thus, in this study, we focused on six persistent SSWs between 2004 and 2020. Even 2008 shows a very small impact in our analysis, thus, to not overwhelm our plots and analysis, we focus on these six events that are also among the top ten strongest events since 1979. However, to show that our conclusion about the EPV change and ozone changes is valid for weaker events, these two events are added to the regression plot (Fig 6). This plot also emphasis that these two events are the same level of influence as SSW in 2008 with regard to EPV change and ozone changes.

Addition text to sec 3:

> "This paper focuses on six persistent mid-winter (December-February) major warmings in this period that exhibited persistent easterly zonal mean zonal winds with a duration of at least 16 days (Table II). Table II includes the duration, magnitude of the easterly zonal wind, and the duration of polar vortex recovery for each SSW; all information is derived from MERRA-2 data. It should be noted that the duration of the easterly wind shown in Table II is not necessarily consecutive. Two major SSWs during the 2004-2020 time period are not included in the main results of our study because they did not meet the persistence criteria. The major SSW in 2007 exhibits only 4 days of easterly zonal mean zonal winds, while the major SSW in Feb 2010 exhibits only 9 days. However, SSWs in 2007 and 2010 are included in the regression analysis for Figure 6 for more robust statistics which also shows that they had some of the lowest impact on ozone."

Additional text to sec 5:

> To increase the robustness of the regression analysis, SSWs in 2007 and 2010 are also included here (Fig. 6).

We believe that by using the suggestions and feedback by both reviewers we have clarified the novelty of the work using six persistent events between 2004 to 2020. We briefly looked into the SSW in 2021 and considering its duration of 15 days of easterly winds with winds only reaching -10 m/s, it was a weak event compared to our original set of events. Thus, we decided to keep the scope of this study between 2004 to 2020.

GC3. There is a terminological confusion regarding the use of the word "model" where you really mean either data assimilation system, assimilated data, or reanalysis. I tried to catch those instances in my specific comments below. I think it's important to remember that a reanalysis is **not** a model simulation. Rather, it is fundamentally a data-driven product. Calling a reanalysis a "model" is equivalent to calling a retrieved satellite data set "a radiative transfer model". The role of the general circulation model in a data assimilation system is to propagate information from observations in time and space over a period of several hours. That's all it does. Things do get a bit muddled in places/periods devoid of observations, but your study looks at regions well constrained by data, so that's not an issue here.

Response: All occurrences are changed to data and system as commented in the text.

GC4. You use the terms lower and middle stratosphere somewhat loosely. Please define them somewhere in the methods section.

Response: In methods section, we added a sentence to emphasis the middle stratospheric layers that will be the focus of the further analysis:

> "In further sections, analysis will focus on middle stratospheric layers between 15 and 30 km."

For lower stratosphere, in the section 4, we defined the UTLS in the layer description (p11, l27):

> "5 to 10 km includes the upper troposphere-lower stratosphere (UTLS)"

GC5. You choose to exclude the lower stratosphere from this analysis citing the high uncertainty of MERRA-2 ozone there. While it's completely OK to make that choice I have a problem with the stated motivation. It's certainly true that the uncertainties get larger closer to the tropopause, but I don't think one can say, based on the validation that you did or the results in Wargan et al (2017), that there's no useful information down there, i.e. that the true variability is completely obscured by uncertainty. The ozonesonde comparisons in Fig. 3 suggest difference standard deviations of about 25%. Below I plot the time series of MERRA-2 ozone at Eureka at 150 hPa along with 25% and 50% envelopes. Clearly, the dynamical variability around the 2009 SSW is still very much discernible in the sense that the magnitude of the large jumps exceeds the uncertainty. Additionally, note that Albers et al. (2018) found useful ozone information in MERRA-2 in the lowermost stratosphere and Knowland et al. (2017) as well as Jaeglé et al. (2017) successfully studied stratospheric intrusions into the troposphere using MERRA-2 ozone. Again, I agree that any results in the LS would have larger uncertainty, but I can't agree that they would be worthless, and the present wording in the paper seems to imply that. I'm not suggesting extending the analysis. It's just a matter of phrasing things in a more nuanced way.

Response: We agree with the reviewer comment. The text is updated to reflect this comment:

> "The larger uncertainties below 10 km during the five months impacted by SSWs are consistent with larger uncertainties in MERRA-2 in these layers year-round, as seen in previous studies (Gelaro et al. 2017; Wargan et al. 2017). However, large fluctuations within the lower atmosphere ozone are still discernible from MERRA-2 data (Knowland et al. 2017; Jaeglé et al. (2017); Albers et al, 2018).
>
> ..
>
> Because more than 80% of ozone molecules exist in the middle stratosphere (10 to 30 km), the total column uncertainty is dominated by uncertainties in mid-stratospheric layers. In the following section, we discuss ozone variability in the total column and the vertical profile up to 60 km, while our primary analysis is focused on ozone and dynamical processes the mid-stratospheric layers, which contribute most to the TCO and where the measurements are most reliable. "

Commented [1]: Spc23

GC6. In several places the language of the paper implies (or even outright) states causal relationships where causality is not immediately demonstrated. I provide some examples in my specific comments. There are sentences like "*the vortex displacement toward the southeast (Europe) prior to the 4 major SSW as seen in 2006, 2008, 2013, and 2019 (hereafter the displaced vortex SSWs) caused an early positive ozone anomaly*", "*an elongated polar vortex is observed before the SSWs which caused a dominant negative ozone anomaly*", "*SSWs and their impact on ozone*" (the title of a section). I get it that Figures 11 and 12 and the discussion in Section 6 do provide evidence for a causal mechanism, but the reader doesn't know that in advance. I suggest rephrasing these causal statements as something more "relational" (Fig 6 and the wording on P24 L3-5 are very good) and explaining that a dynamical mechanism will be elucidated in Section 6. You can then state your results in causal terms in the conclusions as you do. It's really about streamlining the argument.

Response: We agree with the reviewer comment and fixed the language. Also, the revised manuscript places more emphasis on Figure 6, plus the new categorization of the polar vortex before the event, to identify the impact on ozone. As discussed in the response to GC1 and some parts of the discussion.

Specific comments

Spc1: P2. L19-22. I believe that this "ozone gets made in the tropics and transported to the high latitudes" description is simplified to the point of being incorrect (albeit quite commonly used). Please, take a look at the discussion around Fig. 5.11 in Bresseur & Solomon 2005 (in my edition it starts on page 286 with "It is sometimes stated that ozone is produced where its mixing ratio maximizes..."). This does not affect anything in your paper of course.

Response: In that sentence we referred to ozone accumulation during wintertime over high latitudes that is modulated by BDC (rather than the absolute source of ozone in the Arctic). We rephrased the sentence to avoid any confusion for readers.

> "High latitude ozone accumulation during winter and peak values in the spring are largely controlled by BDC transport of ozone-rich, tropical stratospheric air."

Spc2: P2. L24-25. Would it make sense to cite the recent review paper on SSWs by Baldwin et al., 2021?

Response: It is added to the text.

Spc3: P4. L3-4. "numerical and assimilation models". I struggle with this terminology. I understand that by "numerical models" you mean general circulation models or numerical weather prediction models. I can live with the short-hand version "numerical models". But I think the word model should not be applied to data assimilation systems, which comprise a model component and a statistical analysis scheme. I suggest "numerical models and data assimilation systems". See my general comment #3.

Response: The text is updated:

> "The complexity of altered dynamics of SSWs might introduce extra uncertainties into the numerical models and data assimilation systems."

Spc4:P4. L15-16. At first read this sentence confused me: why focus on zonal structures? SSWs are very non-zonal! But you're actually doing much more than that: the Greenland sector, polar averages, maps. I suggest rephrasing it.

Response: That paragraph is completely revised as discussed in respond to GC1.

Related to this comment, we also In section 7, page 24:

> "The variability in impact of SSWs on high latitude ozone is analyzed, two different patterns are found, and the possible dynamical mechanisms involved, are studied."

Spc5: P4. L17. "assimilation model". See above. Why not just say reanalysis?

Response: It is updated to reanalysis data.

Spc6: P5. L7-9. The description of MERRA-2 needs some rewriting. It's not clear to me what it means that there's a variety of models incorporated in MERRA-2. Do you mean GCM, land model, parameterized chemistry, etc.? There aren't multiple "general circulation model**s**" in it as the text implies. There is one. Also, I don't know what you mean by "extended reanalysis". No other reanalysis is incorporated in MERRA-2. If you mean something like OSTIA (the SST data set that provides boundary conditions for the GCM), it's not a reanalysis, at least not in the same sense as MERRA-2.

Response: The text has been updated to apply the reviewer's comment:

> "A variety of data sets are incorporated into a general circulation model to create 3-dimensional MERRA-2 ozone datasets with a time-frequency of 3 hours (Wargan et al., 2017; Gelaro et al., 2017)."

Spc7: P5. L16. I wouldn't say that it's been extensively used in trend studies. To my knowledge only Wargan et al. (2018) derived trends from (suitably corrected for discontinuities) MERRA-2 (there was a follow-on paper by Orbe et al. 2020, but MERRA-2 ozone is sort of tangential there). Also, please consider adding a very interesting study by Albers et al. (2018) to these citations.

Response: We removed the extensive from the sentence and added Albers et al. (2018) to the citation list.

Spc8: P5. L21. Please, provide a citation for the data collection used in this study. Information on how to cite the MERRA-2 pressure-level output is here: https://disc.gsfc.nasa.gov/datasets/M2I3NPASM_5.12.4/summary under "data citation".

model-level output: https://disc.gsfc.nasa.gov/datasets/M2I3NVASM_5.12.4/summary (I'm providing both in case I misunderstood which one you used; see the next comment).

Response: The data source is now cited (as shown in the next response)

Spc9 :P5. L21. Why use pressure-level output if much higher vertical resolution model-level output is available? But P8 L16 talks about using the model levels. So which collection is really used?

Response: To have the finest vertical resolution for the comparisons with observations, MERRA-2 ozone in the model level is used (GMAO, 2015a). To investigate dynamical mechanisms, using pressure-level data facilitate estimation of variables such as PV and potential temperature and consequently ozone in pressure level is used for associated analysis. We converted the pressure coordinate to geopotential height to estimate the derivatives of further analysis.

> "To have the finest possible vertical resolution for the comparisons with observations, MERRA-2 ozone at the model levels is used (GMAO, 2015a). Other dynamical variables such as temperature, and the northward and vertical wind velocities (v, ω), are extracted from the pressure-level MERRA-2 dataset (GMAO, 2015b), which facilitates the calculation of variables such as potential vorticity (PV) and potential temperature (θ)."

Spc10: P5. I think it should be mentioned that MERRA-2 assimilates MLS ozone down to 177 hPa between 2004 and 2015, then switches to version 4.2 retrievals going down to 215 hPa. This version (and vertical range switch) results in some differences between the pre- and post 2015 periods, whereby the latter has likely more accurate ozone, especially in the lower stratosphere.

Response: It is applied.

> "Total column ozone from the Solar Backscatter Ultraviolet Radiometer (SBUV) (1980 to 2004) and the Ozone Monitoring Instrument (OMI) (since 2004) and retrieved ozone profiles from SBUV (1980 to 2004) and the Microwave Limb Sounder (MLS) (since 2004, down to 177 hPa to 2015, down to 215 hPa after 2015) are used to estimate ozone in MERRA-2 (Gelaro et al., 2017)."

Spc11: P5. L22. "Assimilated/reanalysis models". Why not just say "assimilated products" or "reanalyses"? And what do you mean by "variations in models"? Model uncertainties?

Response. The sentence is updated.

> "In reanalysis products such as MERRA-2, methods of analysis, model uncertainties, and observations cause uncertainties in the products (Rienecker et al. 2011)."

Spc12: P6. L25. Are your results affected by the "ozonesonde problem" identified in Stauffer et al. (2020)?

Response: Only Eureka is among the affected stations as discussed by Stauffer et al. (2020), and the artifact is observed starting from early 2016. As shown in the appendix of the same study, starting in 2018, the differences are back to normal (Fig B of appendix of Stauffer et al. (2020)). None of the SSWs studied here are during the period of artifacts. Thus, our comparisons are not affected by this issue.

Spc13: P8. L21, L24, L26. Model dataàreanalysis data, model profileàreanalysis profile, modelà reanalysis.

Response: The text is updated.

Spc14: P9 L15-16. I don't understand this sentence. Could you rephrase it, please?

Response: It is updated.

> "This study also analyzes the impact of different dynamical transport mechanisms on ozone for each of the major SSWs."

Spc15: P9 L21 The symbol Δ. (the Laplace operator followed by a dot) should be replaced by ∇ · (divergence)

Response: It is updated.

Spc16: P10 L8. Since you previously said that subscripts denote derivatives you can't use M_y and M_z as these are not the derivatives of M. I can't remember off-hand the notation in Andrews et al. but I suggest $M_{(y)}$ and $M_{(z)}$. I can't exclude the possibility that some authors use M_y and M_z but it strikes me as unnecessary abuse of notation.

Response: It is updated to $M_{(y)}$ and $M_{(z)}$.

Spc17: P10 L15-16. The GEOS model used in the MERRA-2 data assimilation system does include chemical ozone production (albeit simplified). Do you simply mean to say that P and L is neglected in your analysis (Fig 11 and 12) but that doesn't lead to significant non-closure of the budget as evidenced in Fig. 11? Please, clarify.

Response: The text is updated:

> "Thus, neglecting P and L below 30 km in further analysis, as chemical production and loss is not an output of reanalysis data, does not lead to significant non-closure in the presented analysis and does not impact our conclusions."

Spc18: P10 L20. "assimilated models"àdata assimilation systems

Response: The text is updated.

Spc19: P10 L20-24. I know what you mean but the way it's written this is somewhat contradictory. You say that the ground-based observations are too sparse, and then, in the next sentence you say that it's a "dense network".

Response: The text is updated:

> "However, the use of ground-based observations to directly study the impact of SSWs is challenging because of the coarse time resolution of ozonesondes, limited clear-sky conditions and sunlight for FTIR measurements, and dealing with one profile per site/launch time for each sensor, and its subjectivity to the site location and time."

Spc20: P12 L2-3. It's not designed to do that. The MERRA-2 DAS does not include bromine chemistry.

Response: The text is updated.

> "The extreme low ozone values near the surface are not represented in MERRA-2 as it does not include bromine chemistry."

Spc21: P12 L11. How is significance evaluated? Also, note that lack of statistical significance does not necessarily mean that a result is not useful.

Response: the text is updated.

> "From 5 to 10 km, a negative mean bias exists at all sites however they are accompanied by a larger the standard deviation."

Spc22:P13 L13. Larger percentage values may also result from low mean ozone concentrations in the denominator.

Response: We expect that the differences of low concentration should be smaller too. Even if we consider a detection limit for low values that is a source of uncertainty at lower levels.

Spc23:P13 L21. It's not clear to me what this means. Do you simply mean 75% of ozone molecules?

Response: The text is updated to ozone molecules.

Spc24:P14 L10. Why were those values chosen? Because they are representative of the vortex edge? Also, I think "105 K..." should be "10^{-5} K..."; please use proper notation for the exponents. Note that 1 Potential Vorticity Unit = 10^{-6} m^2 s^{-1} K kg^{-1}.

Response: We updated the range to fit the standard PV units of 600 and 800 (10^{-6} K m^2 Kg^{-1} s^{-1}). We used these two values of PV that show the extent of the polar vortex. Although these values are not the exact edge of the polar vortex, even using the maximum gradient of PV as polar vortex edge has its own uncertainties (as discussed in Serra et al. 2017). Because we are looking at 15-day averages, the fine structure of polar vortex locations are smoothed and fix contours provide a simple and easy way of estimating polar vortex general intensity and positioning.

Spc25:P14 L11. Approximately what altitude does 850 K correspond to?

Response: ~ 30km, and it is added to the text.

Spc26:P16 L4. Causality cannot be inferred from these maps alone. I suggested changing "caused" to "accompanied by" or something like that. You can explain that a causal mechanism will be established later in the paper.

Response: The text is updated and now uses "accompanied".

Spc27:P16 L3-11. Are the "elongated" and "displacement" events related to the more familiar characterization of SSWs as "displacement" and "split" or wave-1 vs. wave-2 events?

Response: One of the novelties of this study is showing ozone changes due to SSWs are connected to the averaged polar vortex shape before the SSW as we categorize them as elongated or displaced. These averaged polar vortex states are different, though often related to, split and displaced vortex breakdown conditions. As seen during the SSWs in 2018 and 2009, the polar vortex splits and the 15-day average polar vortex before that event is elongated. Other events such as 2013 and 2019 first displace, and then split. However here we considered the displacement as the 15-day average as displaced and not elongated.

Additional text to section 5, page 17:

> "The averaged polar vortex state we refer to in this study is different, though often related to, split and displaced vortex morphology discussed in previous literature (e.g., Charlton and Polvani 2007). As seen during the SSWs in 2018 and 2009, in which the polar vortex split, the 15-day average polar vortex before those events is elongated. Other events, such as those in 2013 and 2019, first displace and then split. However, here we consider them displaced SSWs if the 15-day average EPV prior to the event is displaced and not elongated. Previous studies focused on the connection of the type of polar vortex breakdown to its impact on the speed of trace gas transitions (Charlton & Polvani (2007); Manney et al. 2009b). This study investigates the modulation of the magnitude and extent of ozone changes, and the results show that the average EPV shape before the vortex breakdown is more influential than the final form of polar vortex breakdown."

Additional text to section 5, page 18:

> This result shows that the averaged polar vortex shape before the SSWs is connected to the EPV change and then dramatically influences the magnitude of ozone changes at high latitudes.

Commented [BSS2]: Spc28

Spc28: P16 L12-20 and Figure 6. This is really nice but I would like to see more detail. What latitude is the PV averaged over? What about the TCO? Is it evaluated over some region, latitude band or location? Can you provide a correlation coefficient and maybe draw a regression line? Let me say preemptively that I realize that the sample is small so technically there may be an issue with low statistical significance, but I wouldn't overestimate the importance of the latter, rather arbitrary notion.

Response: The plot is updated to have the regression line and R2. And the text is updated: A correlation between the magnitude of change in EPV and TCO is very strong (R2= 90%). We emphasize this finding more through out the paper and, in the introduction, and discussion.

> "To investigate the connection of polar vortex strength and TCO, the scatter plot of the zonally averaged (60ºN to 80ºN) EPV change at the potential temperature of 850 K versus the corresponding change in TCO (60ºN to 80ºN) is shown in Figure 6. All averages are area weighted, and the ratio of change for each variable is estimated as the average of 15 days after SSWs subtracted by the average of 15 days before the SSWs and divided by the average of 15 days before the SSWs. To increase the robustness of regression analysis, SSWs in 2007 and 2010 are also included here (Fig. 6). The correlation between the magnitude of change in EPV and TCO is very strong (R2= 90%). The elongated vortex SSWs (2009 and 2018) exhibit a higher magnitude of change in both EPV and TCO in this period. This result shows that the averaged polar vortex

shape before the SSWs is connected to the EPV change and then dramatically influences the magnitude of ozone changes at high latitudes."

Also, the updated text in the conclusion, page 25:

"A strong correlation of $R^2 = 0.90$ is observed between the magnitude of change in the averaged EPV around the SSW and the magnitude of TCO change for the same period for all six studied SSWs and including two less persistent SSWs in 2007 and 2010. The regression analysis also emphasized larger changes in both EPV and TCO during elongated SSWs."

page 26:

"This study shows that the averaged vortex shape before the SSWs is an important modulator of the magnitude and extent of ozone changes over high northern latitudes."

Spc29:P16 L24. Are these area-weighted averages?

Response: Yes. We added the area-weighted to the text:

"the variability of area-weighted ozone average over the Greenland sector (60ºN to 80ºN and 10ºW to 70 ºW) as well as the zonal average (60ºN to 80ºN) is analyzed to investigate the similarities and differences of the impacts of SSWs on zonal and regional high latitude ozone"

Spc30:P18 L2-4. I assume the "due to the effects..." bit refers to the 2006 situation. The wording suggests a causal link between the minor warming and the exceptional trajectory of the Greenland vs. zonal TCO in 2006 but no detailed analysis is presented to support that link, at least not until Section 6. Additionally, it looks to me like the increase is similar between the Greenland sector and the zonal mean also in 2013.

Response: We applied some clarification to respond to the first part as mentioned below. For the second part of the comment, that paragraph summarizes the increase rate of ozone during the SSWs. And 2006 is the only case that exhibits lower ozone increase over Greenland compared to the zonal average, and that's why we provided more explanation on that year. 2013, on the other hand, is consistent with other years as the TCO increase is higher over the Greenland sector. During 2013, EPV is higher than the climatology and ozone is dramatically lower than the climatology (-40 to -20 days of the event) over the Greenland sector.

The text is updated:

"The relative increase in TCO over the Greenland sector (blue line) is higher compared to the zonal average (orange line). The Greenland sector is climatologically inside the polar vortex area and has a lower TCO value during strong polar vortex which consequently exhibits higher relative increase after the vortex break down and mixing. However, dynamically disturbed winters such as years with minor SSWs before the major SSWs hinder the higher relative TCO increase over the Greenland sector compared to the zonal average. For instance, in 2006, the polar vortex weakened around 25 days before the major SSW (first column Figure 7, TCO 2006) due to a minor SSW, which coincides with the averaged TCO (solid line) increase compared to the climatology (dashed line) as seen in the second column Figure 7 (TCO 2006). The earlier timing of the positive anomaly caused a lower value in the TCO change after the event. The relative TCO increase over the Greenland sector exhibits a higher value during elongated polar vortex SSWs with 37% in 2018 and 31% in 2009. More details of physical mechanisms that cause variability in ozone during SSWs is discussed in section 6."

Spc31:P18 L15. Is the zonal average taken between 60N and 80N as before? Are these area-weighted averages? Are the anomalies calculated with respect to the climatology?

Response: Yes, the average is area-weighted and are based on climatology of non-SSWs years.

The text is updated:

"Figure 8 shows the temporal evolution of the vertical structure of ozone as a cross-section of area-weighted ozone anomalies for both the zonal average (60ºN to 80ºN) and the Greenland sector from 40 days before to 60 days after each SSW. The anomalies are estimated with respect to the climatology of non-SSW years between 2004 to 2019."

Spc32:P18 L25-27. "...because of". Again, causality is stated but not demonstrated until later.

Comment: The text is updated:

"The impact on ozone with the shortest duration occurred in 2008, which has multiple disturbances in the circulation and the shortest duration of easterlies (Table II)."

Spc33:P19 L15. I don't think that it's the vertical advection that leads to the poleward transport of PV. Isn't it rather due to planetary wave breaking and resulting mixing of low-PV low latitude air into the high latitudes?

Response: Thank you for noticing this. It is now clarified in the text

"The cyclonic polar vortex during wintertime is generated in response to the seasonality of radiative cooling. The intensified wave forcing before the SSW is manifested by both accelerated tropical upwelling and polar downwelling, and by poleward *eddy* transport of low EPV air parcels."

Spc34:P20 L20 The notation M_y/dy is incorrect. If anything it should be $\partial M_{(")}/\partial y$. Same for the vertical derivative. More troubling is the use of "y" (which was never defined, I believe; the equations so far were in terms of φ). These two quantities should be the terms of the del operator acting on **M** in the spherical coordinates, i.e. one would expect something like

$\frac{1}{a\cos\varphi}\frac{\partial}{\partial\varphi}(M_{(y)}\cos\varphi)$.

. Please make sure that the calculation is done correctly and that the notation is also correct.

Response: the text is updated to have the correct syntax of derivation: $e^{(z/H)}((a\cos\varphi)^{-1}\partial(\cos\varphi M_{(y)}/\partial y)$. Please note that My and Mz are already defined in eqs (4)-(5).

Spc35 :P21 L8. Is this connection with "elongated vortex" something that you could explain, or at least provide a viable hypothesis for?

Response: We show that vertical advection is more magnified during elongated vortex cases, which could be connected to stronger wave forcing in these cases. Occurrences of more elongated vortex SSWs in future could shed light on this matter and help us to make more robust conclusions.

In section 6, page 23:

> "This study shows that the larger geographical extent and magnitude of ozone changes during SSWs with elongated polar vortex is tied to greater vertical advection during these events."

Sec 7, page 27

> "The intensified vertical advection during elongated vortex events and abrupt wave forcing in these events is tied to the more intense magnitude and larger geographical extent of ozone changes during these events."

> The intensified vertical advection and abrupt wave forcing in during elongated vortex events is tied to the more intense magnitude and larger geographical extent of ozone changes during these events

Spc36: P22 L5. "*errors in the vertical derivatives over high latitude can be large as cos(φ) gets small)"* Why? I would think that it's the horizontal derivative that would be sensitive to that, but it's very likely that I'm missing something obvious.

Response: Yes, horizontal derivatives such equation in Spc34 is in mind. The text is updated.

Spc37: P22 L24. Again, it's not clear to me how significance is calculated.

Response: Significant bias is defined if the bias value is higher than the magnitude of the standard deviation. The text is edited:

> "a non-significant negative mean bias (bias is less than the standard deviation) exists at all sites (-8% to 15%)."

Spc38: P24 L23-25. As this (true) statement is not substantiated by the results of this study, it would be good to cite something here to support it.

Response: (Randel et al., 2002) is added to reference and support the statement.

Spc39: P25 L8. Even adding just the 2021 SSW would help!

We believe that by using the suggestions and feedback by both reviewers we have clarified the novelty of the work using six persistent events between 2004 to 2020. We briefly looked into the SSW in 2021 and considering its duration of 15 days of easterly winds with winds only reaching -10 m/s, it was a weak event compared to our original set of events. Thus, we decided to keep the scope of this study between 2004 to 2020.

Technical corrections

TC1: P3. L1 Please double check the grammar. "SSWs" is plural

Response: Applied.

> "Sudden stratospheric warming events (SSWs) are the largest alterations of stratospheric circulation during wintertime and the significant factor in the interannual variability of stratospheric transport."

Tc2: P3. L18-19. Grammar. "one of the strongest"à"some of the strongest"

Response: updated.

Tc3: P10 L5. The "a" (earth's radius) should be italicized.

Response: Updated.

Tc4: P10 L10-11. The equation numbering changed from 1, 2,.. to 2.4, 2.5. Please, fix it.

Response: Updated.

Tc5: P9 L22. "in this case ozone mixing ratio ***tendency***"

Response: Updated.

Kris Wargan

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