Response to referee#1's comments for manuscript

We'd like to thank the Reviewer for his/her positive feedback and valuable comments. Here we clarify the comments of the Reviewer, with his/her comments in black and our responses indented in blue. To clarify the results we introduced the following changes:

- a case study comparing WACCM-X(SD) and all observations plus MERRA2
- all figures are now provided in altitude
- we added sections comparing ozone and water vapor with WACCM-X(SD)
- identical tidal analysis for WACCM-X(SD) and meteor radars.

This study presents the analysis of observed high-latitude tidal and trace gas (ozone, water vapor) variability in response to SSWs, in combination with modeled trace gas and heating rate variations. The authors seek to leverage the simulated heating rates to quantify the impact of ozone and water vapor radiative perturbations on tidal variations in the MLT. By highlighting the role of trace gases, the authors aim to present the first comprehensive attempt to explain mesospheric tidal variability in the polar region. While the interpretation of the results is built upon a well-researched literature framework, the connection between known mechanisms of tidal variability and the result of the current work is done qualitatively.

Since the main aim of the paper is to extend previously published results on ozone and water vapor coupling to mesospheric tidal variability, by specifically quantifying the contribution arising from polar trace gas variations, my main concern is that, in my view, this relationship is not in fact quantified. The paper remains at the stage where observations are argued to be explained by a possible mixture of dynamical and radiative effects, falling short of an actual quantification of the isolated radiative effects that are central to this work. I am further concerned that the relationship between SSW-driven ozone perturbations (and the corresponding radiative effects) and tidal variations might be hardly quantifiable, due to the lack of sunlight at these latitudes (upwards of 65 degrees North) during wintertime when SSWs occur. While the authors demonstrate that stratospheric and mesospheric altitudes receive some sunlight at these latitudes (even though the exact dates of the individual SSWs are not specified in the text; with some presumably occurring during the polar night), the modeled 0.5K/day perturbation is undoubtedly of very low intensity. While some context is provided for the modeled 0.5 K/day heating rates, the actual tidal forcing components of these rates are not discussed.

To support the author's conclusion that this work provides a deeper understanding of the mechanisms driving tidal variability during SSWs, I therefore think that as a first step, the tidal forcing terms should be much more rigorously quantified. But even then, I would be surprised if the tidal forcing will be significant enough to result in an observable perturbation, given also the large number of other mechanisms and forcing terms involved in driving tidal variability during SSWs.

Given the tools used by the authors, possible suggestions for quantifying the tidal forcing perturbations could be, for example, to repeat the SD-WACCM-X experiments with specified ozone concentrations (preferably only past 65 degrees North), and/or to calculate Hough mode tidal forcing terms (or even simply 24, 12, and 8 hour tidal harmonics at the radar sites) from the 3-hourly SD-WACCM-X heating rates.

1) **Regarding the explanation of a mixture of dynamical and radiative effects:**

> In the revised manuscript, we have focused on the radiative effects of trace gases by analyzing 3-hourly variations. We have clarified that the delayed enhancement of the diurnal tide is primarily related to the radiative effect, as evidenced by the strong correspondence between the diurnal ozone cycle and the shortwave heating rate variations at high-latitude stations. Additionally, we have clarified that the enhancement of the semidiurnal tide during SSWs is linked to the secondary ozone layer above 90 km. In

addition, the ozone double-layer structure in the WACCM-X(SD) ozone anomalies that form at the onset of the SSW and last for about two weeks, results in two layers of substantial UV heating. The superposition of these two diurnal tidal waves at the mesosphere may effectively amplify the SDT at high latitudes due to a 12-hour phase offset caused by the different vertical distances both waves have to travel, considering the typical vertical wavelengths of 30-50 km for semidiurnal tides at this latitude.

2) **Regarding the concern about the limited sunlight at high latitudes during wintertime:**

We have presented shortwave heating rates from 3-hourly WACCM-X(SD) simulations during the 2018/2019 SSW event as a case study (with a central SSW date of January 2, 2019). Our analysis reveals a clear diurnal forcing signature in the shortwave heating rate (QRS) approximately 20–30 days after the SSW onset. This response is primarily driven by the modified ozone volume mixing ratio (VMR) in the stratosphere. These results demonstrate that, despite reduced sunlight during winter at high latitudes, stratospheric altitudes still receive sufficient solar radiation to modulate shortwave heating rates, contributing to diurnal tidal variability. We also added one table to show the calculated altitude during winter at three stations.

3) **Regarding the exact dates of the individual SSWs:**

Our study focuses on major SSW events occurring in mid-winter. The WACCM-X(SD) simulations span 2015–2023 and include four SSWs (February 12, 2018; January 2, 2019; January 3, 2021; and February 16, 2023). The meteor radar analysis covers a broader period from 2004 to 2023, encompassing 10 major SSWs, all occurring in January or mid-February.

More general comments are given below:

The abstract mentions that the water vapor perturbations have a negligibly small impact on tidal changes. As a suggestion, I would therefore consider changing the title to "New insights into polar ozone and water vapor, and the connection between ozone radiative effects and tides in the mesosphere-lower thermosphere during major sudden stratospheric warming events". I would also consider changing line 19 to read "...transport of radiatively active ozone is important for explaining the observed tidal variability", rather than "transport of radiatively active gases is important…".

Reply: Thank you for the suggestion. We have decided to keep the role of water vapor in the abstract and title, as it plays a part in the overall analysis. The revised Figures clarify the impact of the trace gases as proxies for the QRL and QRS.

L6: 'polar latitude' to 'polar latitudes'.

Changed

L10: 'TDT tide' to 'TDT amplitudes'.

Changed

L50-51: I would suggest adding information that the intensified tides and quasi-two-day wave amplitudes observed by Lima et al. (2012) were observed in the low-latitude summer hemisphere.

Added

L76-79: The authors note that this study, for the first time, quantifies the impact of ozone and water vapor responses on tidal variations in the MLT. Do the authors mean to say the ozone and water vapor responses specifically at polar latitudes? The impact of ozone perturbations on MLT tidal variations has been investigated in numerous other studies, as also referred to in the introduction. It is also not clear to me how the paper presents a quantification of the total radiative forcing during SSW events; maybe total radiative forcing above 65 degrees North is meant, based on the results showing averaged data between 65-90 degrees North?

Reply: Yes, we specifically investigated the ozone and water vapor responses at polar latitudes. While previous studies have examined the effects of ozone on tides in the MLT, they primarily focused on tropical regions. To our knowledge, no studies have investigated the combined effects of ozone and water vapor on tidal variations in the MLT at polar latitudes during SSW events. Regarding the quantification of radiative forcing, we refer to the total radiative forcing above high-latitude and polar stations (67°N, 70°N, 79°N). Our analysis is based on the shortwave heating rate and ozone anomalies from WACCM-X(SD) simulations at these three stations during SSW events. The total radiative forcing (QRL) is primarily driven by ozone VMR (accounting for approximately 99% in the stratosphere, as shown in Figure 4.24 of Brasseur and Solomon, 2005). Therefore, we quantified the radiative effects on tidal amplitude during SSW.

L144: Perhaps it is enough to mention that SD-WACCM-X can be run in a fully coupled mode. Currently it is not clear whether the model is run with an active or prescribed ocean, which in a way distracts, since this is not strictly necessary information for understanding the results of this work.

Removed

L152: I would suggest rephrasing “photochemical rate constants” to “photochemical absorption and quantum-yield data”, or alternatively “photochemical molecular data” since currently it could be interpreted as if the photochemical reaction rate coefficients (J-values) are kept constant, but clearly these vary as a function of solar zenith angle and trace gas concentrations.

Changed

L154: Could it be specified which years exactly the climatological SD-WACCM-X simulations span? Given that short-wave radiation absorption is central to the paper, could the (E)UV absorption scheme and relevant chemical species from WACCM-X also briefly be discussed?

Added: WACCM-C(SD) simulation is from 2015 to 2023.

L169-L170: A table showing the central SSW dates between 2004-2022 would be highly beneficial, given that insolation can be very different between an event occurring on the 24th of March (in 2010) or 5th of January (in 2004). Could it also be specified which model from the cited NOAA page is used as reference (even though I would think this is MERRA-2 based on its relation to SD-WACCM-X)? It would also be helpful if the exact SSW onset criterion is specified in the text, given the large variety of definitions used in literature.

Reply: The SSW events are already presented in a table in our paper, which we have cited accordingly. In this study, we focus specifically on major SSWs that occur in mid-winter (January and February) and do not consider events with onsets such as March 24th, 2010. Generally, SSWs occurring in early spring are referred to as final stratospheric warming (FSW) events rather than major SSWs. Additionally, we confirm that the reference model from the cited NOAA page is MERRA-2.

L174-177: To my knowledge, not all major SSW events are associated with an Elevated Stratopause (ES). Looking at the NOAA database, there are 8 major SSWs between 2004-2013. However, Limpasuvan et al. (2019) identified only 5 ES SSWs during this time period (their Figure 1). Please discuss the notion of not all SSWs having an ES, and how this affects the interpretation of your results, as ES SSW characteristics are assumed to be present as a general feature in the modelled and measured composite SSW response later on in the text.

Reply: here, we have clarified the distinction between major SSWs and Elevated Stratopause (ES) SSWs. A major SSW is defined by two criteria: A reversal of the zonal-mean zonal wind at 10 hPa (~30 km) and 60°N from westerly to easterly. A significant increase in stratospheric temperatures, particularly in the mid-to-upper stratosphere. Major SSWs occur predominantly in winter and are classified into displacement and split events based on the vortex morphology.

Elevated Stratopause (ES) SSW is a specific type of major SSW that leads to an exceptionally high and rapidly reformed stratopause after the event. While many major SSWs are associated with elevated stratopause, not all exhibit the pronounced mesospheric and lower thermosphere effects seen in ES-SSWs. ES-SSWs often have a stronger and more prolonged impact in the mesosphere and thermosphere due to enhanced gravity wave forcing and residual circulation changes.

L180: The link between planetary-wave activity and the observed oscillations in the meridional winds is unclear to me. While I understand that planetary wave-mean flow interactions can induce low-frequency oscillations in the mean meridional winds, the analysis described in Section 2.1 states that the time-frequency analysis also includes longer period waves such as stationary planetary waves, in addition to a mean wind. How is it possible to differentiate between low frequency (quasi-stationary) planetary waves and mean wind oscillations at a single station without additional longitudinal information?

We are analyzing local observations in the European sector. The zonal and meridional mean winds, which we obtain from the adaptive spectral filter represent a low pass filtering corresponding to a daily mean. These low-pass filtered winds, therefore, show signatures of planetary wave activity. Planetary waves show stronger and more clear signals in the meridional components as the mean (temporal) meridional wind is close to zero. Our analysis is not separating quasi-stationary waves from the mean flow. However, previous studies have shown a close correspondence of the planetary wave activity in meteor radar winds with global satellite data (https://doi.org/10.1016/j.jastp.2011.10.007,www.atmos-chem-phys.net/18/4803/2018/, https://doi.org/10.5194/angeo-35-711-2017).

L188: At this point naming the station coordinates becomes a bit repetitive. My suggestion would be that here "the three radars" is simply enough.

Changed

L192: The interpretation of the %-changes in tidal amplitudes and mean winds would benefit from a comparison to climatological values. Could the climatological values between for example January and March be included as figures in the appendix? And similarly for the long- and short-wave heating rates? Given the spread in SSW onset dates (4th of January to 24th of March), I would expect there to be a considerable spread in event-to-event QRS rates and the associated deviations from climatology, given the rapid fall off of insolation during winter. It would probably also be helpful to discuss the event-to-event variability in QRS in the paper, based in part on the climatological figures.

We clarify that the onset dates considered in this study are in January and February, as we focus on major SSWs and do not include final stratospheric warming (FSW) events that occur in early spring. The climatological values at these stations have been previously published (Stober et al., 2020). While insolation during winter decreases significantly, it can still reach the stratosphere, troposphere, and even the surface. For reference, please see the added Table 1 in the revision.

L195: Possible reasons for the observed TDT variations are suggested here. Would this be more appropriate for the discussion section? Alternatively, I would suggest moving certain discussion points, for example that the TDT may be contaminated by gravity waves in the analysis technique, to this section. With this in mind, could the authors comment on why the TDT enhancement 10 to 20 days after the SSW onset is so sporadic? For example, at the Tromsø site, the meridional anomaly is around + 2 m/s on day 30, then falls to zero, and then returns to + 2 m/s on day 40. How does this variability fit in with the kind of fluctuations that could be expected from GW contamination?

We put less focus on the TDT variations and moved all details to the discussion. Due to the short window used in the adaptive spectral filter, TDT amplitudes and phases can be biased by gravity waves that fall into the filtering window, which is the case for some inertia gravity waves. Some of the variability can be explained by gravity wave source variability and is not necessarily related to TDT tidal short-term changes.

L201: Please be more specific about how exactly the TDT variability observed in the current work aligns with previous studies that used GCMs and satellite measurements to discuss the solar heating, nonlinear interactions, and gravity wave-tide interaction excitation mechanisms for the TDT. This also ties in with the above comment.

A detailed discussion of the gravity wave and TDT tidal variability is beyond the scope of the paper and requires additional work on the methodology. Separating gravity waves and terdiurnal tides is challenging from point source data. Currently, we are working on tomographic meteor radar network analysis to improve our tidal diagnostic. However, as the Hunga Tonga eruption did show, our available domain size is still within the range of large scale gravity wave perturbations and, thus, there will be some ambiguity remaining. However, this would go beyond the scope of the paper.

L200: Do the authors here mean to say “TDT amplitude anomaly”?

Changed

L213: Personally I do not see a clear sign of the STD showing an enhancement 20-50 days after the SSW onset (albeit weaker than during SSW onset time), at least not in the data presented in the current work. In the current work the STD shows considerable variability also during times without SSWs, while there is no commonly identifiable pattern between the three meteor radar stations between days 20-50. If anything, I would argue that only Sodankylä shows a local maximum between days 40-50.

Removed

L234: It is not clear to me what Figure 5 shows. From the context I would guess that these are daily averaged 3-hourly heating rates? Or are they amplitudes of the 24 hr variations in the (3-hourly) modeled heating rates? This is crucial information for understanding discussion points later on.

Yes, Figure 5 presents the daily-averaged 3-hourly heating rates. In the revision, we have updated this figure to show 3-hourly heating rates instead of daily averages to better capture the variability. Furthermore, the new Figures clearly show the diurnal cycle in the QRS and ozone VMR.

L254: This is largely a repeat from an earlier comment, but I fail to see a clear SDT enhancement at the three stations 20-50 days after SSW onset. It is therefore difficult to connect the modeled QRS rates to SDT amplitude anomalies 20-50 days after onset. As a suggestion, could a time-series of, for example, 90 km altitude SDT amplitudes and 50 km altitude QRS rates be shown in a single figure? This may help to demonstrate a more clear relationship between the two.

Reply: In the revision, we have clarified that the SDT enhancement is primarily associated with the secondary ozone layer in the thermosphere, above 90 km, which aligns with the observed SDT enhancement at these altitudes. Stratospheric ozone layer (20-50km) plays a key role in the delayed DT enhancement. QRS is mainly contributed by the ozone layer UV absorption. This double-layer structure is mainly responsible for the semidiurnal tidal activity during the winter months at these latitudes. The superposition of the in-situ forced tide at the secondary ozone layer is out of phase with the stratospheric tide excited around 50-60 km altitude that needs to propagate upward 30-50 km. This coincides with the vertical wavelengths that are found in climatological analysis (Stober et al., 2021b) for these stations. During the SSW, this balance is disturbed, and the secondary ozone layer together with the elevated stratopause provides favorable conditions to amplify the SDT. The QRS shows a pronounced enhancement that coincides with the time of the semidiurnal tidal amplification.

L255: I do not quite follow the line of reasoning where 1) the findings from Siddiqui et al. (2019) and Limpasuvan et al. (2016) that SSWs are followed by rapid increase of ozone between 20S to 40N arising from equatorial upwelling and cooling, and a decrease poleward of 40N, and 2) the subsequent ozone enhancement at mid to high latitude as shown in Figure 6 happening immediately after SSW onset. Based on the first point, I would expect a rapid decrease in ozone centered on the SSW onset date in Figure 6?

Reply: We have added the ozone variation at the mid-latitude station in Bern (47°N) in Figure B2. As expected, there is a decrease in ozone at this station around the SSW onset. At polar latitudes, SSW events, characterized by abrupt warming and the weakening or reversal of the polar wintertime westerly circulation, lead to extreme ozone variability. This allows ozone-rich air from lower latitudes to enter the polar region, contributing to the observed ozone enhancement in the polar stratosphere shortly after the SSW onset.

L256: I would suggest adding that these heating rates are for wintertime. I would further expect these heating rates to be quite different between January and March SSWs, given the high latitude of the stations, and considering that heating rates approach 14 K/day during summer (Brasseur and Solomon, 2005, Figure 4.25). As mentioned above, I think it would therefore be highly beneficial if climatological heating rates between, say, January and March could be added to the appendix.

Please see below: reply with L257.

L257: It is argued that the QRS change is mostly due to ozone increases following the SSW onsets. The QRS anomaly falls roughly between 0.1 to 1 hPa (Figure 5), while the ozone anomalies extend between roughly 50 to 0.1 hPa in two largely separate patches (Figure 6). Why wouldn't the ozone anomaly below 1 hPa, i.e. the bottom patch, contribute to the QRS anomaly? Is this because the stratosphere does not receive any sunlight at these latitudes during winter? A comment at this point in the manuscript would be beneficial. The lack of QRS perturbation between 100-10 hPa seems to conflict with the stratosphere receiving sunlight as argued for in section 3.3.

Reply L256 and L257: We have added the absolute values of heating rates in Figure 15, where the QRS rate reaches up to 15 K/day after the SSW onset, highlighting the significant variation in QRS heating rates. This addition helps illustrate the variability in heating rates during wintertime SSW events. The stratosphere does receive sunlight at these stations during winter, as discussed in Section 3.3. For further clarification, please refer to Table 1 in the revision.

L286: Could the double-layer structure of the ozone be discussed or clarified in more detail? Does this refer to the anomaly in Figure 6 appearing to show a two-layer structure? I would think that a two-layered anomaly would not necessarily imply a double-layered structure of the underlying layer. Could the upper anomaly (centered on 1 hPa, or 50 km) simply be an extension of the stratospheric ozone layer?

Reply: we have clarified that the double-layer structure is the stratosphere ozone layer (20-50km) and secondary ozone layer (90-100km).

L286-300: It is hard to imagine a 0.5 K/day heating rate (if these indeed would amount to 0.5 K/day) to provide significant tidal wave energy. In my view, isolated model experiments and a more detailed discussion of the tidal forcing terms would be a necessary step here. As a first estimate, what is the amplitude of the 12 hr component in the heating rates?

Reply: We added one section: a case study during SSW 2018/2019 to show the heating rate variation, which reveals a clear signature of a diurnal forcing starting 20-30 days after the SSW.

The stratospheric diurnal tide is further a largely vertically trapped mode, so it is unclear to me how the amplitudes at stratospheric altitudes (40-50 km) translates to amplitudes at meteor radar altitudes (80-100 km). The superposition of two 24 h waves will also always result in a 24 h wave, no matter how in or out of phase they are (so long as their frequencies are both 24 h). So it is also unclear to me why the superposition of two diurnal waves may effectively amplify the SDT at meteor radar altitudes? Regardless, I think the impact of travel time and vertical distance should be explained in more detail.

Reply: The ozone double-layer structure in the WACCM-X(SD) ozone anomalies that form at the onset of the SSW and last for about two weeks, results in two layers of substantial UV heating. The superposition of these two diurnal tidal waves at the mesosphere may effectively amplify the SDT at high latitudes due to a 12-hour phase offset caused by the different vertical distances both waves have to travel, considering the typical vertical wavelengths of 30-50 km for semidiurnal tides at this latitude Stober et al., 2021b, 2020).

L298: I can't find reference to the (in-situ) diurnal heating rates causing pronounced diurnal tides at the latitude and altitudes relevant to the current work based on the cited work by Schranz et al. (2018), which appears to discuss only the diurnal cycle in ozone and not the winds. Could this be clarified?

Reply: In the stratosphere, there are strong diurnal heating rates, as shown in Figure 2, primarily contributed by ozone. Schranz et al. (2018) highlighted the diurnal cycle of ozone. However, the diurnal heating rates due to ozone give rise to the formation of diurnal tides.

L310: Here the observed SDT response is primarily attributed to changes in zonal mean wind and ozone heating at mid-to-low latitudes, even though the contributions of these effects are not quantified, and these are therefore difficult to place into context with the ozone mechanism described in the paper. The double-layer ozone structure is further argued to contribute to the immediate STD response on line 315, while on line 312 it is argued to likely contribute to the observed STD variability during the recovery phase, and not during the onset phase. This seems contradictory.

Reply: We have clarified that the secondary ozone layer above 90 km plays a key role in the SDT amplitude.

L309-L324: I struggle to see the connection between the discussion points in this paragraph and the placement of the results within the literature. I think this largely stems from the contributions of the different mechanisms (propagation conditions, mid-to-low latitude ozone forcing) not being quantified in the context of the observational data, even though observed characteristics are attributed to these mechanisms. When the aim of the paper is to quantify the contribution of polar trace gas perturbations, I think a more careful quantification or discussion of the other effects is also warranted, given that the net observed tidal response is shaped by the complex interplay of all the different mechanisms. Further, as mentioned above, while the modeled 0.5 K/day heating rates fall within the range of stratospheric diurnal temperature variations, the actual diurnal components of the heating rates are not discussed.

Reply: In the revision, we have clarified the following points: The delayed DT enhancement is associated with diurnal heating forcing, primarily resulting from ozone UV absorption at high and polar latitude stations. The SDT enhancement near the SSW onset is linked to the secondary ozone layer, and the double-ozone layer also plays a significant role in the SDT amplitude. The interaction of these two diurnal tidal waves in the mesosphere may effectively amplify the SDT at high latitudes. This is due to a 12-hour phase offset caused by the different vertical distances both waves travel, considering the typical vertical wavelengths of 30-50 km for semidiurnal tides at this latitude.

Please check the spelling of Tromsø in the figure sub-titles (sometimes spelled as Tromose).

Changed