Response to Reviewer #1's Comments

We are writing to provide additional details regarding the revisions made to our manuscript titled "Quasi-10-day wave activity in the southern high-latitude MLT region and its relation to the large-scale instability and gravity wave drag".

To clearly delineate the structure of our second response, the original reviewer comments are given in bold and italics, our initial responses are in grey, and the specific changes we have made are in regular red font.

General comments:

Q. This study examines the generation and propagation of 10-day waves in the southern hemisphere upper atmosphere, using observations from meteor radar winds at two Antarctic stations, with supporting data from SD-WACCM simulations and MLS data. The main conclusion of this study is the identification of regions of barotropic/baroclinic instability in the upper mesosphere and lower thermosphere that can generate such oscillations

A: The identification of regions of barotropic/baroclinic instability in the upper mesosphere and lower thermosphere as potential generators of oscillations is one of conclusions of our work. However, we would like to emphasize that our study also presents two additional key findings.

1. The first one is the climatology and seasonal variability of the quasi-10-day wave (Q10DW) observed in the Antarctica MLT region. The seasonal variation we present in our manuscript adds to the list of observational reports of mesosphere dynamics that can be used for the validation of model results as well as other observations.
2. The second one is the implication of comparison between EXP60 and EXP75. This comparison suggests that large-scale instabilities due to excessive gravity wave drag (GWD) in the summer mesosphere can make travelling quasi-10-day planetary waves (PWs) overestimated. This finding suggests that the more accurate representation of GWD is required to improve the mesosphere dynamics in high-top models.

Major comments

Q1. The main conclusion of the study is not new. The authors have ignored existing literature which offers corroborating or alternative investigations: doi 10.1002/grl.50373, 1016/j.asr.2022.10.054, 10.1029/2019JD031599 and 10.1016/j.jastp.2014.06.009, and many others published since Chandran's study.

A1: We agree that the studies you mention should be incorporated into our manuscript to provide a comprehensive background, and thus we will add these references to the introduction and discussion sections of our revised manuscript.

However, those studies particularly focus on the variation of Q10DW activity during sudden stratospheric warming (SSW) events. Our study describes the 5-year and seasonal variations of Q10DW over the Antarctic region observed using ground-based meteor radars (MRs) rather than Q10DWs during particular events. As mentioned in the introduction, Forbes & Zhang (2015) reported climatology and seasonal variations of Q10DW activity within +-50 degrees latitude in the altitude range of 20–100 km based on the SABER data for 2002–2013 . Our research also contributes to the climatology and seasonal variations at higher latitudes over the Antarctica for z= 80–100 km, Furthermore, our additional conclusion suggests that large-scale instabilities due to strong eastward GWD in the summer mesosphere can lead to the generation of Q10DW as we mentioned above.

A1': Following your suggestions, we included a detailed review of previous Q10DW studies related to SSWs. This comprehensive overview has been integrated into to the "1 introduction" section from lines 96 to 117.

Q2. MERRA-2 is scientifically valid up to 60 km, the uppermost altitude where MLS data is used. Above 60 km and all the way to the lid (~75 km), it is just a sponge layer. Thus, I cannot associate any usefulness to the EXP75 presented in this study, where SD-WACCM is nudged with MERRA-2 fields all the way to MERRA-2 lid: it is like nudging a model with another model. Certainly, EXP60 is more useful but it is discussed only in terms of a difference from the EXP75 and briefly. If the goal of doing this study was to compare two simulations with higher and lower nudging fields, the authors should then be aware of these extant studies: doi 10.1002/2017JD027782, or 10.1002/2015GL065838.

A2: Since late 2004, the data assimilation system at NASA GSFC GMAO have incorporated the MLS temperature and ozone from 5 hPa up to 0.02 hPa and from 250 hPa to 0.1 hPa, respectively, for the production of the MERRA-

2 data (Gelaro et al., 2017; McCormack et al..2021). We think the assimilation of MLS data gives the mesospheric meteorological analysis with a reasonable quality when compared to other mesospheric reanalysis. McCormack et al. (2021) showed the reasonable quality of the mesospheric part of the MERRA-2 data through the intercomparison of the mesospheric assimilation such as the MERRA-2 and results obtained using the higher-top data assimilation systems. The realism of the MERRA-2 in the upper mesosphere is also supported by the personal communication with Drs. Lawrence Coy and Krzysztof Wargan at NASA GSFC GMAO (8 November, 2023). They told us that there is no evidence that the divergence damping layer (Fujiwara et al., 2017) near the top boundary of the GEOS6 model is unduly interfering with the MERRA-2 results. They also said that while there is a lot of variability in the different (mesospheric) assimilation systems, meaning a lot of uncertainty, MERRA-2 seems to fit well in the mix. In fact, Figure 11.42 in Chapter 11 of the Stratosphere-troposphere Processes And their Role in Climate (SPARC) Reanalysis Intercomparison Project (S-RIP) Final Report (Harvey et al., 2021; https://www.sparc-climate.org/sparc-report-no-10) demonstrates that upper mesospheric temperature in the MERRA-2 compares well with the MLS temperature. As reviewer pointed out, there is the sponge layer near the top boundary of the GEOS6 model, but the use of the sponge does based on the divergence damping in the MERRA-2 not seem to degrade seriously the reliability of the MERRA-2 in the mesosphere. A lot of damping mechanisms (Rayleigh friction, 2nd-order hyperdiffusion, and so on) can be used for the sponge layer (Jablonowski and Williamson, 2011). Rayleigh damping and low-order hyperdiffusion can damp significantly model forecast fields, but the divergence damping is often used to effectively and selectively remove high-frequency (noisy) gravity waves keeping the large-scale circulation and planetary wave structure less changed. Below, we compare the horizontal temperature distribution between MLS and MERRA-2 data. The results demonstrate generally good agreement between the MERRA-2 and MLS above 0.1 hPa (Figure R1), suggesting that the EXP75 experiment is not simply nudged towards model forecasts but towards assimilation based on MLS. We will revise our manuscript to include a more detailed justification of this specified dynamics experiments, addressing potential concerns about the validity of simulation results in the upper mesosphere.

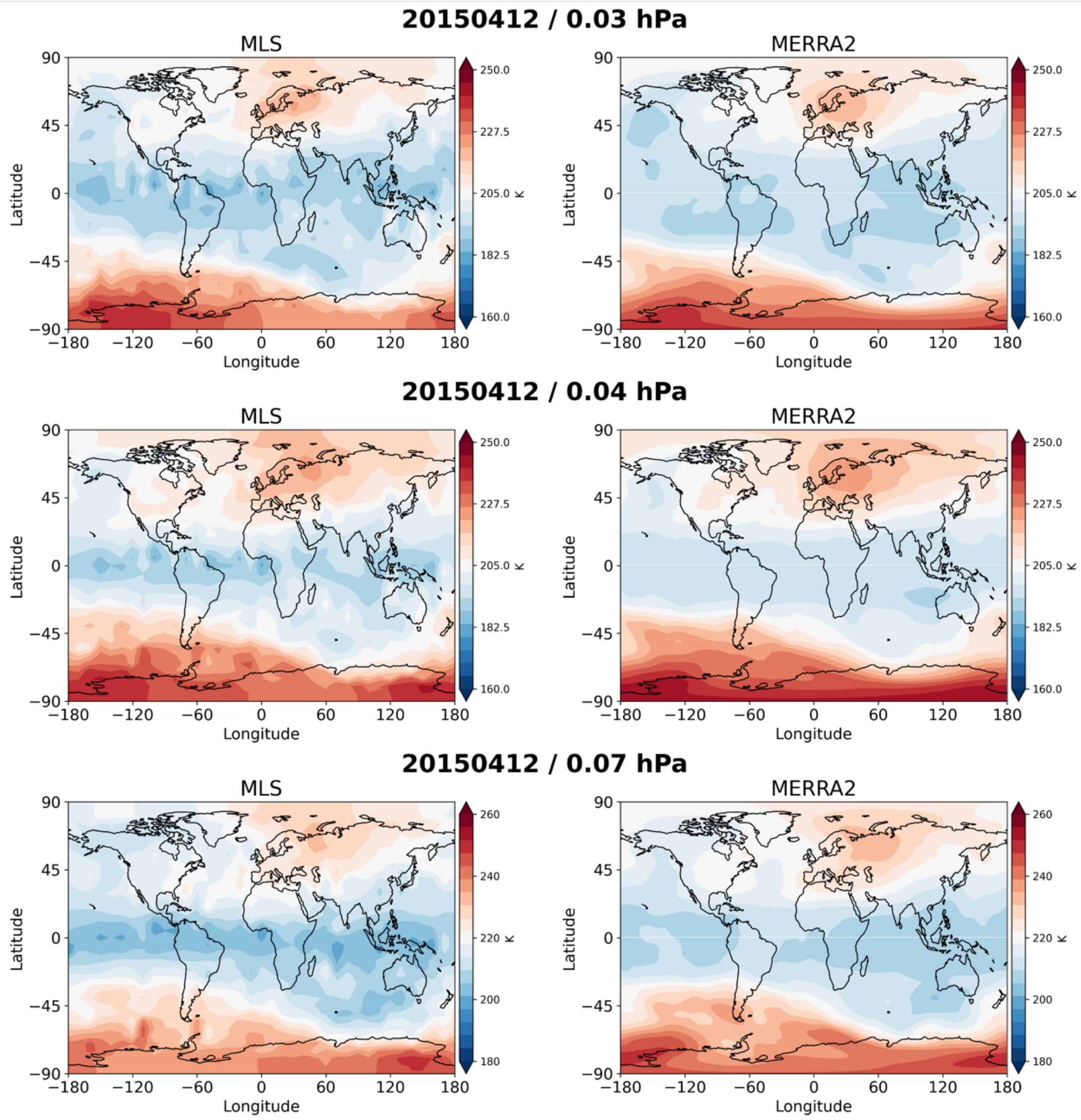

Figure R1 Longitude-Latitude distributions of temperature at three different pressure levels of 0.03 hPa, 0.04 hPa, and 0.07 hPa. For each pressure level, the temperature distributions from (left) MLS and (right) MERRA-2 are presented.

A2’: In order to clarify the top boundary of MERRA-2 reanalysis, we have included the overview of divergence damping layer for the GEOS-6 model and assimilation procedure of MERRA-2 in the 2.2 SD-WACCM section from line 201 to 210.

Q3. MLS data is used in the data assimilation of MERRA-2. I find it odd to attempt to validate the model results that are nudged to MERRA-2 with a dataset that is part of the data assimilation cycle. In other words, an independent data set ought to be used. Why not SABER?

A3: We agree with your opinion. Independent datasets need to be employed for validation. First of all, however, we will check the availability of SABER data in the Antarctic region. Due to the yaw orbit of TIMES satellite, we need to confirm whether SABER data are available in the high-latitude regions for the specific dates and locations of interest. If the SABER observation is available in the Antarctic areas, we will incorporate the analysis with SABER into our manuscript to strengthen of our results.

A3': In accordance with your suggestion, we checked the availability of SABER data for the cases in Figure 6a and 6b of our study. As shown in Figure R2, we found that SABER data was not available for case '05 Feb 2013', but available for case '16 Nov 2016'. However, Figure R2b shows that the local time of observation for high-latitude regions were concentrated between 22 and 02 LT. This concentration of observations within a narrow time window, due to the yaw cycle of the TIMED satellite, makes it difficult to calculate a daily zonal mean zonal wind for specific dates. Smith et al. (2017) did calculate monthly mean zonal mean zonal wind using SABER data covering 60 days to account for the yaw cycle, this approach appears unsuitable for our study, which focuses on investigating large-scale instability on specific dates. Therefore, we concluded that using SABER data for a case study like ours might not suitable.

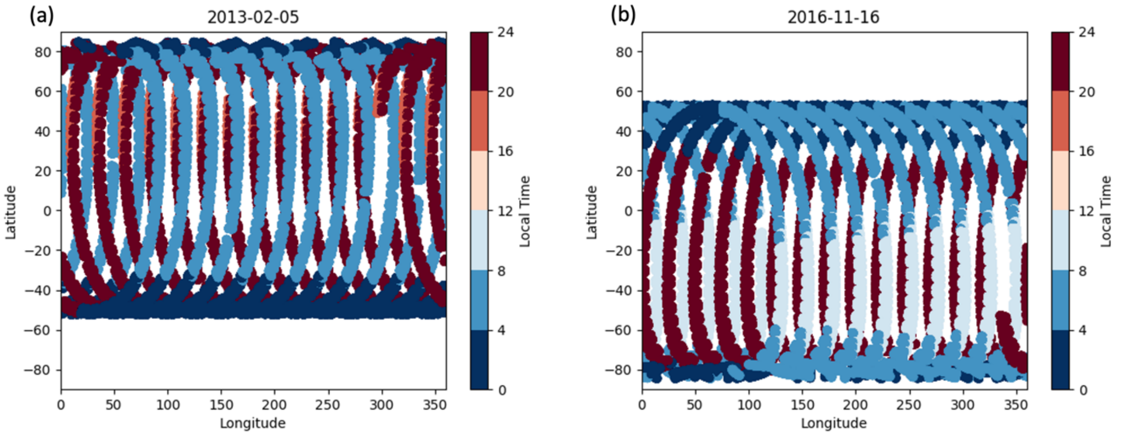

Figure R2 Horizontal distribution of local time sampling of SABER on (a) 5 February 2013 and (b) 16 November 2016.

Q4. The authors identify the occurrence of a 10-day normal mode from the symmetric behavior of MLS geostrophic winds. That is not sufficient: the amplitude structure has nodes, and the phase is expected to be in a specific configuration in order to be 10-day free oscillation. See: DOI: 10.1175/JAS-D-11-0103.1, 10.1111/j.1600-0870.2007.00257.x, and the Salby's papers already cited.

A4: We agree that a rigorous analysis based on Legendre polynomials and Fast Fourier Transform, which provide latitudinal and longitudinal structures of the normal mode, is the optimal method for identifying the theoretical normal modes. However, It is noteworthy that several studies have recognized disturbance with a quasi-10-day period or other periods of gravest normal modes, westward propagation, and zonal wavenumber 1 as normal mode waves in most of the middle atmosphere (e.g., Forbes and Zhang, 2015; Yamazaki and Matthias, 2019). Forbes and Zhang (2015) observed that the normal mode of Q10DW tends to be contaminated above 80 km altitude, the layer on which our study focuses. In their study, the amplitude and latitudinal structure of Q10DW above z = 80 km are modified by the asymmetry of mean winds between the hemispheres and meridional temperature gradient. In addition, they suggested that the dissipation of gravity wave filtered by the Q10DW wind field can generate a secondary Q10DW by momentum deposition. Our supplementary figure (Fig. S3) demonstrates that this latter factor is unlikely to be a significant contributor to the generation of Q10DW in our cases. In our manuscript, we tried to distinguish Q10DW with normal mode-like structure from oscillations that seem to never have the normal mode-like structure using MLS data. As the reviewer pointed out, however, this distinction may potentially cause confusion, and it will be excluded in our revised manuscript. In the light of removing potential confusions, we will carefully define our terminology in the revised manuscript. The Q10DW will be described as "a westward-propagating (travelling) planetary wave with zonal wavenumber 1, potentially related to the Rossby normal mode”. We will incorporate the considerations mentioned here to clarify our analysis and conclusions.

A4’: We have modified the terminology of Q10DW as “Q10DW-W1, which is potentially related to the Rossby normal mode in the MLT region.” In line 254–255.

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