

Response to RC1: 'Comment on egosphere-2023-2381', Anonymous Referee #1, 21 Jan 2024

We are very pleased with RC1's comments. The reviewer comments are given in italics and the our respective responses in purple font.

"I appreciate the effort of the authors to address the comments from this reviewer; I realize it wasn't easy. I remain concerned of the use of the model output to infer climatological behavior in a region that is not informed by observations and is potentially affected by lid-dynamics. The attempt to 'validate' the model in their rebuttal (figure R1) addresses only a static behavior of the model, not its variability. I only ask that the authors include in their conclusions a cautionary statement that the behavior illustrated in their study might be influenced by compounded effects of the sponge layer dynamics and unconstrained model climatology. After that, the study can proceed to publication."

: In response to your concerns regarding the model output potentially being affected by the sponge layer, we have included a cautionary note in our manuscript. This can be found in lines 694–703 as follows:

Lines 694–703: "Results of SD-WACCM may depend on the extra damping above the middle mesosphere in the GEOS-6 model (Fujiwara et al., 2017) used to produce the MERRA-2 data. The damping may have harmful effects on the results for the upper mesosphere in the EXP75, where the dynamics is still specified above the middle mesosphere using the MERRA-2, but comparison with observations shows that the zonal asymmetric structure of mesospheric temperature in the EXP75 is reasonable for the time periods of our interest (Fig. S5). However, the activity and variability of mesospheric PWs in the MERRA-2 and SD-WACCM need to be further examined for the longer time periods and evaluated against other observations to support the reliability of results obtained in this study, which should be a topic of continuing research."

We believe that this addition adequately addresses your concerns about the potential influence of sponge layer dynamics on our results.

Response to RC2: 'Comment on egosphere-2023-2381', Yosuke Yamazaki, 26 Jan 2024

We appreciate the comments by Dr. Yamazaki. We repeat the reviewer's comments are provided in italics and the our repective responses in purple font.

"I have read through the revised manuscript and response letter. The authors have properly addressed my previous comments.

The authors have re-evaluated the amplitude of the Q10DW derived from the meteor radar (MR) observations. The Q10DW amplitudes have been corrected from 10-25 m/s to 1.2-3.0 m/s. The amplitude in the WACCM simulation (EXP75) is in the range of 4-10 m/s, which is much larger than the revised amplitude values. The Q10DW amplitudes estimated with MLS geostrophic winds are also greater than those from the MR observations. The authors argue that the Q10DWs from WACCM and MLS are overestimated. The authors explained in the response letter why this might be the case. Although I have no objection or counter evidence to the authors' viewpoint, it is worth acknowledging and discussing the disparities among the results obtained from MR, MLS, and WACCM in the paper. For the moment, it is not entirely clear which among these three sources most accurately represents the true nature of the Q10DW. The MR technique is not perfect, involving only two stations at different latitudes. More validation studies are needed in the future, which may lead to the improvement of the technique as well as the establishment of a method for uncertainty estimate.

I suggest that the authors include one sentence in Results and Discussion addressing the possible underestimation of the Q10DW amplitude by the MR technique, and another sentence in Summary that explicitly address the discrepancies among the MR, MLS, and WACCM results and the need for further studies."

: In response to your suggestions, we have added contents to our manuscript discussing the potential reasons for the discrepancies in the amplitudes of Q10DW among MRs, MLS, and SD-WACCM data as follows:

Lines 276–280: "It is important to note that the amplitudes of Q10DW are systematically lower in MRs compared to the MLS results. These discrepancies might be attributed to the accuracy of estimated geostrophic winds from the MLS data, or the inherent limitations of MR analysis, which in our case involves only two stations located at slightly different latitudes."

Lines 673–678: "In addition, our study shows the Q10DWs from the MLS appear to be consistently overestimated compared to those from MRs. These discrepancies can be due to both errors in estimating winds from the MLS and uncertainties in results obtained from two MR stations alone. Further investigation is required for more reliable estimation of the amplitude and phase of Q10DWs from observations."

These additions to our manuscript provide the possible reasons of discrepancies among the MR, MLS, and model results and emphasize the need for further studies.

Minor comments:

1. I. 97 "(2022a, 2022b)"
"(2020a, 2020b)"

: We have changed it in line 97.

2. I. 303 "generally larger"
"much smaller"

: We have updated this in line 289.