Dear Editor and Reviewer:

Thank you for your letter and for the reviewers' valuable comments and questions concerning our manuscript entitled "First Observations of Solar Zenith Angle Modulation of Diurnal Tidal Structure in the MLT Region". These comments are all valuable and very helpful for revising and improving our manuscript, as well as the important guiding significance to our researches. We have studied the valuable comments from you and the reviewers carefully, and tried our best to revise the manuscript. These changes in the revised manuscript have been marked in the track changes version manuscript, as well as the point to point responses have listed as following:

Response to Reviewer #1

Comment 1: Lines 50–61 and Section 3.2 (lines 288–323): The authors might want to be more precise in framing the novelty of the reported SZA dependence. The general influence of solar heating, and hence of seasonal variations in solar illumination, on atmospheric thermal tides is well established. Therefore, the novelty of the present study should not be presented as the discovery that solar forcing affects diurnal tides per se. Rather, the authors should clarify that their contribution is the global meteor-radar-based observational documentation of an SZA-related modulation of the vertical amplitude and phase structure of locally observed diurnal tides in the MLT region, including its hemispheric asymmetry and its incomplete representation in SD-WACCM.

Response: We fully agree that the influence of solar heating on atmospheric thermal tides is well established, and that the novelty of our study should not be framed as demonstrating that solar forcing affects diurnal tides per se. We have therefore revised the Abstract, Introduction, Section 3.2, and Conclusions to clarify the specific contribution of this work.

In the revised manuscript, we emphasize that the novelty lies in providing a broad, multi-station meteor-radar observational characterization of the SZA-related seasonal modulation of the vertical amplitude and phase structures of locally observed diurnal tides in the MLT region. In particular, our observations reveal that this modulation is most evident at Northern Hemisphere low and mid-latitudes, is substantially weaker in the Southern Hemisphere, and is only partly reproduced by SD-WACCM. We have also revised the relevant wording throughout the manuscript to avoid implying that SZA or solar heating alone determines the observed local tidal structure, since single-station radar measurements represent a superposition of migrating and nonmigrating tidal components and are additionally influenced by background circulation and other dynamical processes. These revisions more precisely distinguish the established role of solar heating in tidal excitation from the observational advance provided by the present study.

Comment 2: Lines 114–117: The manuscript refers to a "global climatology" and emphasizes the global distribution of the meteor-radar network. While the use of fifteen radars indeed provides a very valuable and unusually broad ground-based perspective, the spatial sampling is not globally uniform. In particular, some latitude and longitude sectors are much better represented than others, with several Northern Hemisphere stations concentrated in East Asia. The authors may wish to slightly qualify the wording where appropriate, or briefly discuss how the non-uniform station distribution could affect the interpretation of hemispheric differences and the representativeness of the inferred “global” patterns.

Response: We agree that, although the fifteen meteor radars provide unusually broad latitudinal coverage from the Southern to the Northern Hemisphere, their spatial distribution is not uniform in either latitude or longitude. In particular, several Northern Hemisphere low- and mid-latitude stations are concentrated in East Asia, whereas the Southern Hemisphere and other longitude sectors are more sparsely sampled.

We have therefore revised the wording in the manuscript to avoid implying uniformly distributed global coverage and now describe the observations as providing a broad ground-based perspective across a wide latitude range where appropriate. We have also added a discussion of this limitation, noting that the inferred hemispheric differences should be interpreted in the context of the available radar sampling. Because single-station tidal observations can also contain longitudinal variability associated with nonmigrating tides and other regional effects, part of the apparent interhemispheric difference may reflect differences in longitudinal sampling in addition to genuine hemispheric-scale behavior. Nevertheless, the geographically extensive radar network still provides a valuable observational basis for identifying large-scale common features and hemispheric contrasts in the seasonal structure of diurnal tides. These limitations are now explicitly acknowledged in the revised manuscript.

Comment 3: Lines 150–156 and Fig. A1: The authors state that the choice of fitting window length has only a minimal effect on the analysis of seasonal tidal variations and therefore adopt a 7-day window with a 1-day step. However, Fig. A1 appears to indicate that the 11-day window leads to somewhat stronger smoothing of the time series compared with the 5-day and 7-day windows. While this may not affect the climatological seasonal patterns emphasized in the manuscript, it would be useful to mention this smoothing effect explicitly and to clarify that the conclusion of “minimal effect” refers mainly to the seasonal-scale variability considered here, rather than to shorter-term tidal variability.

Response: We agree that the 11-day fitting window produces stronger smoothing of short-term tidal variability than the 5-day and 7-day windows, as can be seen in Figure A1. Our original statement that the fitting-window length has only a “minimal effect” was therefore too general. We have revised the manuscript to clarify that this conclusion applies specifically to the seasonal-scale climatological variability considered in this study, rather than to shorter-term tidal variations. The 7-day window with a 1-day step is retained as a compromise between temporal resolution and fitting stability.

Comment 4: Section 3.2 (lines 288–323): The reported relationship between SZA and the vertical structure of the diurnal tide is one of the central results of the manuscript. At present, however, this relationship appears to be mainly inferred visually from the seasonal-altitude plots. It would strengthen the paper if the authors could quantify this relationship more explicitly, for example by providing correlation coefficients, lag correlations, or a simple metric describing the seasonal variation in the altitude of the amplitude maximum. This would make the evidence for the proposed SZA modulation more robust and easier to assess.

Response: We thank the reviewer for this constructive suggestion. We agree that a quantitative assessment provides stronger support for the SZA-related modulation than visual comparison alone. In the revised manuscript, we therefore added a simple metric to characterize the seasonal variation in the vertical structure of the diurnal tide.

As shown in Figure 4, we further compares the altitudes corresponding to zero phase of the zonal and meridional diurnal tides observed at BJMR, MCMR, WHMR, KMMR, and FKMR with the SZA. Strong correlations are found at the midlatitude stations BJMR, MCMR, and WHMR, while relatively high correlations are also evident at the lower-latitude stations KMMR and FKMR. These results provide quantitative support for a pronounced SZA-related seasonal modulation of the vertical structure of diurnal tides in the MLT region, consistent with an important solar-heating imprint on the observed tidal variability.

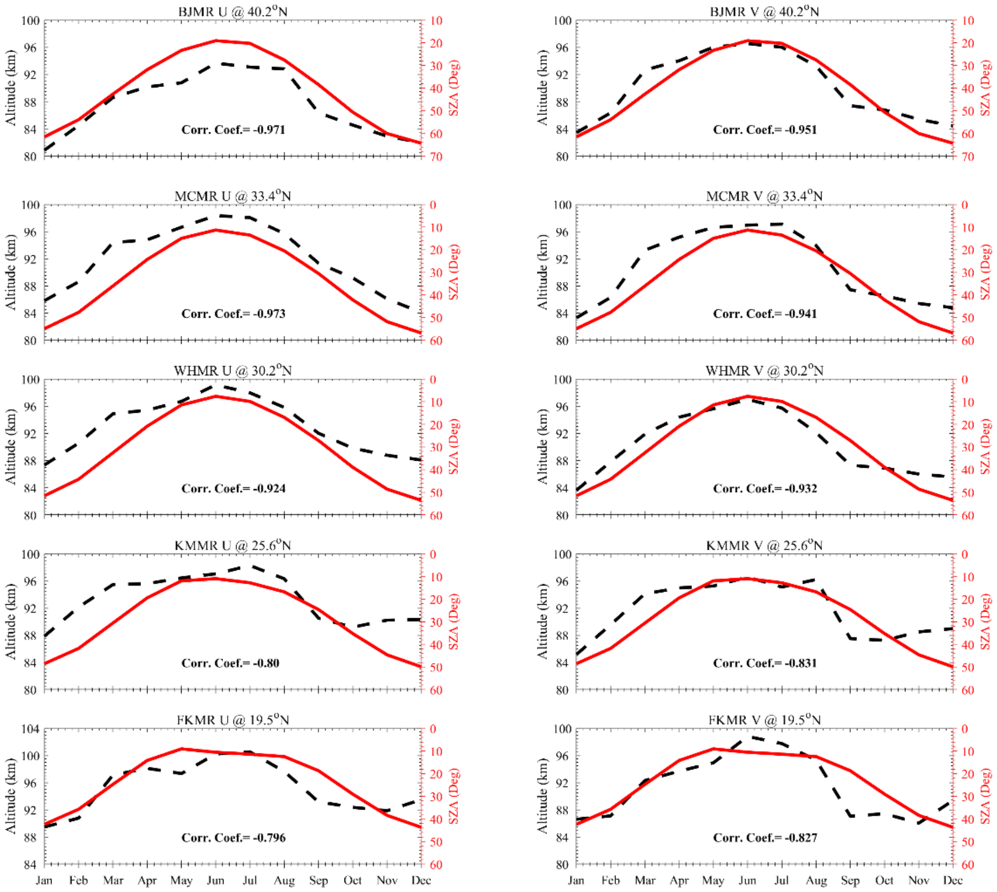

Figure 4. Comparison of the altitudes corresponding to zero phase of the zonal (left column) and meridional (right column) diurnal tides observed at Beijing, Mengcheng, Wuhan, Kunming, and Fuke with the solar zenith angle. The corresponding correlation

coefficients are marked in each panel.

Comment 5: Lines 337–351: The authors correctly note that single-station meteor-radar observations represent a combination of migrating and nonmigrating tidal components. This limitation should be more explicitly connected to the interpretation of the SZA dependence. Since nonmigrating tides may be influenced by zonally asymmetric heating, topography, latent heat release, and wave interactions, the authors should be cautious not to imply that the observed local tidal structures can be attributed to direct solar forcing alone. A short clarification would help distinguish between the established role of solar heating in tidal excitation and the more complex interpretation of locally observed composite tides.
Response: We thank the reviewer for this important comment. The SZA-related variations identified from single-station meteor-radar observations should not be interpreted as a direct response to solar forcing alone. As already noted in the manuscript, the diurnal tide derived at an individual radar site represents a local superposition of migrating and nonmigrating tidal components. While solar radiative heating provides the fundamental excitation for migrating thermal tides, the locally observed composite tide can also be affected by zonally asymmetric heating, latent heat release, topography, background winds, and interactions with planetary waves and gravity waves.

We have therefore revised Sections 3.2 and 3.3 to make this distinction more explicit. In the revised manuscript, SZA is used as a representative indicator of the seasonal variation in solar illumination rather than as a direct measure of the total local tidal forcing. Accordingly, the observed correspondence between SZA and the vertical amplitude and phase structures is now described as an SZA-related seasonal modulation or imprint on the locally observed composite diurnal tide, rather than as evidence that the local tidal structure is directly controlled by solar heating alone. We have also softened the corresponding causal wording in the Conclusions.

Comment 6: Lines 352–363: The authors refer only generally to “data from the European Centre for Medium-Range Weather Forecasts (ECMWF)” when discussing surface net solar radiation and surface latent heat flux. However, the Data Availability Statement suggests that ERA5 reanalysis data were used. This should be stated explicitly in the main text, including the specific data product and variables used, and the appropriate reference should be added, i.e. Hersbach et al. (2020) for ERA5.
Response: We thank for pointing this out. In the revised manuscript, we now explicitly state that the ERA5 reanalysis produced by the European Centre for Medium-Range Weather Forecasts (ECMWF) was used, and specify the two variables analyzed in this study: surface net solar radiation and surface latent heat flux. We have also added the recommended reference to Hersbach et al. (2020) and revised the Data Availability Statement accordingly.
References: Hersbach, H., Bell, B., Berrisford, P., et al. (2020). The ERA5 global

reanalysis. Quarterly Journal of the Royal Meteorological Society, 146, 1999–2049. https://doi.org/10.1002/qj.3803.

Comment 7: Conclusions (lines 422–449): The manuscript focuses on climatological seasonal patterns, but I wonder whether the authors could make better use of the multi-year nature of the radar data set by briefly discussing interannual variability. This aspect may be relevant because the individual radar records have different lengths and cover partly different years. Although the authors compare results from shorter and longer subsets at Davis to assess the robustness of the climatology, it would still be useful to know how large the year-to-year variability of the diurnal tide amplitudes and phases is at selected stations, and whether it could influence the inferred seasonal structures or hemispheric differences. I do not necessarily suggest a full additional analysis, but a short discussion, or one representative supplementary figure, would help readers assess the robustness and representativeness of the reported climatological patterns.

Response: We agree that interannual variability is an important aspect of diurnal-tide variability and may contribute to differences among the climatologies derived from radar records of different lengths and covering different periods.

The primary objective of the present study is to characterize the climatological seasonal structure of diurnal tides across the available meteor-radar network rather than to investigate their interannual variability in detail. To assess the possible influence of record length on the climatological results, we compared the diurnal-tide amplitudes and phases derived from the long-term DVMR record with those obtained from a 5-year subset (Figures A2 and A3). The comparison shows that, although some differences in magnitude are present, the principal seasonal–altitude structures remain very similar. This suggests that the major climatological features emphasized in this study are relatively robust to differences in record length.

Following the reviewer’s suggestion, we have expanded the discussion to explicitly acknowledge that year-to-year tidal variability may affect the magnitude and timing of individual seasonal enhancements and may contribute to some of the station-to-station and hemispheric differences, particularly because the radar records do not cover identical periods. We have therefore added this limitation to the revised manuscript rather than introducing an additional interannual-variability analysis, which is beyond the scope of the present climatological study.

As a follow-up to the present work, we are currently investigating the long-term variability of diurnal tides at middle and high latitudes, with particular emphasis on their responses to the QBO, ENSO, and the solar cycle. These interannual-to-decadal variations will be examined in detail in a separate study.

Comment 8: Conclusions (lines 422–449): The comparison with SD-WACCM is discussed in Section 3.2, but it is not clearly taken up again in the Conclusions. Since the model comparison is an important part of the manuscript and helps demonstrate the value of the observational data set, the authors should briefly summarize the main outcome there. In particular, the Conclusions should state that SD-WACCM reproduces some aspects of the observed seasonal diurnal-tide variability, especially at higher latitudes and partly in the phase structure, but does not fully capture the SZA-related amplitude modulation and the observed low- and mid-latitude seasonal patterns. This would also strengthen the statement that the radar climatology provides useful constraints for model evaluation.

Response: We thank the reviewer for this helpful suggestion. We agree that the comparison with SD-WACCM is an important component of the manuscript and should be more explicitly summarized in the Conclusions.

In the revised manuscript, we have added a brief summary of the main model–observation comparison. Specifically, we now state that SD-WACCM reproduces some broad features of the observed seasonal diurnal-tide variability, particularly the seasonal amplitude variation at higher latitudes and part of the phase structure at Northern Hemisphere mid-latitudes. However, the model does not fully reproduce the observed low- and mid-latitude seasonal amplitude patterns and generally fails to capture the SZA-related modulation of the vertical amplitude structure.

Technical corrections

Lines 211–214: The sentence "However, the seasonal morphology of diurnal tides two conjugate 30° latitudes…" is a bit unclear and appears to be grammatically incorrect.

Response: We thank the reviewer for pointing this out. We agree that the original sentence was unclear and grammatically incorrect. We have revised it for clarity as follows: "However, the seasonal morphology of the diurnal tides at the approximately conjugate mid-latitude stations ALOMR (30.3°S) and MCMR (33.4°N) shows a clear hemispheric asymmetry. Both exhibit enhanced amplitudes during February–March, but this period corresponds to autumn at ALOMR and spring at MCMR. "

Line 233: "The Meridional DT amplitudes" -> "The meridional DT amplitudes"

Response: We have corrected "The Meridional DT amplitudes" to "The meridional DT amplitudes" in the revised manuscript.

Line 434: "vertical altitude structure" is redundant, use "vertical structure" or "altitude structure"

Response: We have replaced "vertical altitude structure" with "vertical structure" in the revised manuscript and checked the manuscript for similar redundant wording.

Response to Reviewer #2

Comment 1: ALOMR is assigned to be a Genesis LTd manufactured radar, but it is an ATARD multistatic setup. Please correct.

Response: We thank the reviewer for raising this point. We rechecked the provenance of the ALOMR data used in this study. The reviewer is correct that the Andes Lidar Observatory region currently hosts the ATRAD-based multistatic CONDOR meteor-radar system. However, the ALOMR data analyzed here are from the earlier SKiYMET meteor radar operated near the Andes Lidar Observatory during 2009–2014, of which the 2009–2013 interval is used in this study (Liu and Franke, 2020).

The following sentence has been added to the revised manuscript: "ALOMR data used in this study were obtained from the earlier SKiYMET meteor radar operated near the Andes Lidar Observatory during 2009–2014 (Liu and Franke, 2020), rather than from the ATRAD-based multistatic CONDOR system deployed in the region since 2019 (Stober et al., 2022)."

References:

Liu, A.& Franke, S. (2020). SKiYMET Meteor Radar Horizontal Wind [Dataset]. Zenodo. https://doi.org/10.5281/zenodo.4267128

Stober, G., Liu, A., Kozlovsky, A., Qiao, Z., Kuchar, A., Jacobi, C., Meek, C., Janches, D., Liu, G., Tsutsumi, M., Gulbrandsen, N., Nozawa, S., Lester, M., Belova, E., Kero, J., and Mitchell, N.: Meteor radar vertical wind observation biases and mathematical debiasing strategies including the 3DVAR+DIV algorithm, Atmos. Meas. Tech., 15, 5769–5792, https://doi.org/10.5194/amt-15-5769-2022, 2022.

Comment 2: Tidal fits and climatology

It is recommended to add a sentence on how the climatologies are obtained from the 7-day tidal fits. It might also be worth mentioning that when tidal amplitudes from different studies are compared, some deviations might be simply due to different data processing.

Response: we thank the reviewer for this helpful comment. We agree that the 11-day fitting window produces stronger smoothing of short-term tidal variability than the 5-day and 7-day windows, as can be seen in Figure A1. Our original statement that the fitting-window length has only a "minimal effect" was therefore too general. We have revised the manuscript to clarify that this conclusion applies specifically to the seasonal-scale climatological variability considered in this study, rather than to shorter-term tidal variations. The 7-day window with a 1-day step is retained as a compromise between temporal resolution and fitting stability.

The corresponding text has been revised in the manuscript as follows: "As shown in Figure A1 in the Supporting Information, we tested multiple fitting-window lengths (5, 7, and 11 days). Although the 11-day window produces somewhat stronger smoothing of short-term variability than the 5- and 7-day windows, the main seasonal variations in diurnal tidal amplitudes and phases remain broadly consistent among the three window lengths. Therefore, the influence of fitting-window length is relatively small for the seasonal-scale variability emphasized in this study, although shorter-term tidal variations are more sensitive to the choice of window. Based on this assessment, we adopted a 7-day window with a 1-day step as a compromise between temporal resolution and fitting stability."

Comment 3: Image quality

The image quality is a bit hard to read and needs to be improved. When looking at the overview images, it is hard to read the station names.

Response: We apologize for the poor image quality in the uploaded version. Because the original high-resolution figures had relatively large file sizes, their resolution was reduced during preparation of the uploaded manuscript. In the revised manuscript, we have improved the figure quality and enlarged the font sizes.

Comment 4: The discussion of the solar zenith angle as a proxy for the tidal excitation needs to be expanded. The temporal variability and interhemispheric differences of trace gases play a crucial role in the tidal excitation and need to be discussed in a bit more detail. Exothermic chemical reactions, primary, secondary, and tertiary ozone might also have a more pronounced effect on the tidal amplitude and phases.

Response: We thank the reviewer for this important comment. We agree that the SZA-related variations identified from single-station meteor-radar observations should not be interpreted as a direct response to solar forcing alone. As already noted in the manuscript, the diurnal tide derived at an individual radar site represents a local superposition of migrating and nonmigrating tidal components. While solar radiative heating provides the fundamental excitation for migrating thermal tides, the locally observed composite tide can also be affected by zonally asymmetric heating, latent heat release, topography, background winds, and interactions with planetary waves and gravity waves.

In the revised manuscript, SZA is therefore used as a representative indicator of the seasonal variation in solar illumination rather than as a direct measure of the total local tidal forcing. We have also expanded the discussion of atmospheric composition and chemical heating, noting that the temporal and interhemispheric variability of trace gases, particularly the primary, secondary, and tertiary ozone maxima, as well as exothermic chemical reactions in the mesosphere, may modify the magnitude and vertical distribution of tidal heating and thereby affect tidal amplitudes and phases.

Accordingly, the observed correspondence between SZA and the vertical amplitude and phase structures is now described as an SZA-related seasonal modulation or imprint on the locally observed composite diurnal tide, rather than as evidence that the local tidal structure is directly controlled by solar heating alone. We have also softened the corresponding causal wording in the Conclusions.

We have revised Sections 3.2 and 3.3 accordingly to make this distinction more explicit.

Comment 5: WACCM

The reviewer recommends adding a paragraph describing the details of the run used for the analysis. Meanwhile, there are so many versions of WACCM, WACCM-X available that the results might differ between different runs. The paragraph should contain some general information on the grid resolution, temporal resolution, and GW parameterizations.

Response: We thank the reviewer for this helpful suggestion. We agree that the original description of the SD-WACCM simulation was too brief. We have therefore added a paragraph in the Section 3.2 the specific model configuration used in this study, including the model version, horizontal and vertical resolutions, temporal resolution of the output used here, specified-dynamics configuration, and gravity-wave parameterizations.

The following paragraph has been added in revised manuscript: “The simulation is nudged toward MERRA meteorological fields produced using the Goddard Earth Observing System Model, Version 5 (GEOS-5) data assimilation system (Rienecker et al., 2008). It employs a horizontal resolution of 1.9° latitude × 2.5° longitude and the high-vertical-resolution lev144 configuration with 144 vertical model levels, providing a vertical resolution of approximately 0.5 km in the MLT region (Zhou et al., 2022). In the specified-dynamics configuration, winds and temperatures in the lower atmosphere are constrained toward the reanalysis fields, while the model runs freely in the MLT region (Cen et al., 2022). Hourly zonal and meridional winds from the model are used in the present tidal analysis. SD-WACCM reproduces the seasonal variation of DT amplitudes at high latitudes, with maxima in summer and minima in winter (Stober et al., 2021a).”

Comment 5: Tidal momentum flux

The section about the tidal momentum flux is a bit confusing. The estimated momentum flux describes how the tides would interact with the mean flow. It is not clear to the reviewer how this quantity can be used to justify that background winds modulate tidal amplitudes and phase, affecting the vertical propagation. This needs to be clarified. However, this parameter seems to be a valuable diagnostic to demonstrate the importance of the diurnal tidal forcing on the mean flow. The values appear to be rather large compared to GW forcing, which would underline that the tides play an important role as well as gravity waves. This also deserves some discussion and is a major contribution of the study.

Response: We thank the reviewer for this helpful comment. We agree that the interpretation of the tidal momentum flux needed to be clarified. In the revised manuscript, we have expanded this discussion to emphasize that the meridional flux of tidal zonal momentum, ($u'v'$), is used as a diagnostic of tide–mean-flow interaction rather than as direct evidence that background winds alone control tidal amplitude, phase, or vertical propagation. We have also clarified the possible role of background-wind Doppler effects in modifying tidal phase relationships and momentum transport.

In addition, we have added a discussion noting that the relatively large tidal momentum flux highlights the potentially important role of diurnal tides in the MLT momentum budget. We also point out that a direct quantitative comparison with gravity-wave momentum fluxes should be made with caution because the quantities represent different components of momentum transport.

Comment 6: Figure A1

Looking at Figure A1, there appears to be a mismatch of the y-label. The Figure title says ‘phase’ plots, but the y-label describes amplitude.

Response: We have corrected the y-axis labels of the phase panels in Figure A1 from “Amplitude” to “Phase”.