Response Letter

This letter is to respond to the comments given by referee #2.

The authors give thanks to referee #2 for the priceless comments leading to the improvement of the paper.

The referee's comments are noted with C#X, and the response is given in blue color, noted with the corresponding R#X.

Major comments:

C#1. The authors used NRLMSISE-00 temperature data, which is not useful for background analyses of individual cases. The author did not mention the role of thermal ducting due to the presence of mesospheric temperature inversion which plays a vital role in the ducting of GWs. Therefore, the statistical observations of free propagating, ducting, or evanescent waves are biased.

C#2. Model temperature cannot record any of these events. The authors should use SABER temperature data for this purpose.

R#1,2: Following the reviewer's comment, we reanalyze the vertical propagation characteristics by using temperature data measured by TIMED/SABER for Equation 2. We updated the results related to m^2. Total events of ducting and partial ducting are 9 for each, as shown in Table 3. Among those events Doppler vs. thermal ducting cases occurred with a ratio of 5:4. The advantage of using SABER temperature is to be able to specify either thermal or Doppler ducting, although the number of wave events is reduced down to 95 (from 111 previously) due to the limitation of the SABER coverage. The majority of vertical propagation is still in the freely propagating mode.

C#3. The seasonal results of propagation direction are similar to previously reported from the mid-latitude location. However, a serious question arises from the low number of data set for statistical analysis. On average, the observation day is only 13% of the total days of 3 years which is very small for statistical analyses. Can the authors justify this?

R#3: To some degree we agree with the reviewer's comment that the percentage of observation day is rather small for the seasonal statistical analysis, especially for fall season that we have already mentioned in the text. However, the total number of wave events we found is 150, more than the number for the 10% statistical uncertainty. Given that the optical observation is highly dependent on weather conditions, other studies (Espy et al. (2004, JGR) and Yang et al. (2010, JGR)) with all-sky camera have published the results with this level of number statistics.

C#4. In previous studies, the neutral instability has been attributed to the in-situ generations of ripples structure (<10-15 km). Figures 5a & 5d show the wavelength data in the range of 10 km. How are they sure that those small-scale waves are not ripples? In this regard, the authors are suggested to calculate the Richardson number to analyze dynamic instabilities.

R#4: Following the reviewer's comment, we calculated the Richardson number and found that only one event is satisfied with a shearing condition of $Ri<0.25$ in the direction of wave propagation. It is rather difficult to evaluate the instability from the MR measured wind data that were derived due to coarse temporal and spatial resolution (1 hr and 2 km altitude and few tens km horizontal bin resolution). Thus, we cannot claim any dynamical instability based on the Richardson number from the MR wind data.

C#5. Line 170-171: "…. the spring/summer the westward wind is dominant in the middle 171 atmosphere, whereas in the fall/winter the eastward wind is dominant …". The observation is during nighttime. Is the seasonal wind pattern mentioned above during nighttime? Authors are suggested to provide mean wind map or profiles of MERRA-2 over Mt. Bohyun observatory season-wise to support the conclusions in lines 321-324.

R#5: Following the reviewer's comment, we added the seasonal variation of nighttime winds for 2017-2019 from MERRA-2, as in Figure 6.

C#6. Why did the authors use interquartile range (IQR) instead of mean and standard deviation?

R#6: In the real situation, there are some cases not well fitted to the Gaussian distribution because the range of parameter distribution is so wide. For this case, IQR results are often used. We calculated the mean and standard deviation for the analysis result. However, intrinsic parameters frequently have large standard deviations, even greater than the mean value. In that case it is not meaningful to have the mean value. Thus, we prefer to use IQR with the median value, instead the mean and standard deviation. For more information, we provide additionally the mean and standard deviation in Table 1.

C#7. Is it possible to detect the nature of GWs (ducted, evanescent, or free propagating waves) based on airglow imager alone?

R#7: No. The airglow image provides only horizontal information. The vertical wave number can be estimated from the gravity wave relation (Eq. 2) with background wind and temperature information.

Minor comments:

Line 17: Please mention the bandwidth of the 557.7 nm filter.

R: The OI 557.7 nm filter has a central wavelength of 557.81 nm with a full-width half maximum of 1.53 nm as added in Line 96-97.

Line 217: The authors did not include the first and second derivative terms in the GWs.

R: The wind profile obtained from meteor radar is derived from the collected echoes for 1 hour with a 2 km height resolution. The wind data are interpolated to produce the wind profile of a 1 km resolution for the use of this study. In case that the first derivative and second derivative terms were added in the m^2 formula, the result would increase uncertainties seriously. Thus, we have to use the simplified version without those terms, as in Eq. (2).

Line 225: Please correct the scale height (Hs) and recalculate those profiles.

R: The scale height has been updated with using SABER temperature.

Line 252: What is the reason behind the observation of evanescent waves during spring only?

R: We do not think that evanescent waves are particularly common or unique in spring, as shown in our new results in Table 2. Using the SABER temperature information, we found the evanescent waves in summer and winter, too.

Figure 6: The X-axis limits of all subplots in the left (wind speed) should be same and also applicable to right subplots (m^2). It will be easy to compare.

R: The wind velocity is to be compared with the phase velocity of gravity wave, but not with other wind velocities. The m^2 is also the matter of change of negative and positive regions through the altitude. If the magnitudes are set with the same maximum from Figure 7a-d, the altitudinal variation would not be distinguishable to the reader.