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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Mesospheric gravity wave activity estimated via airglow imagery, multistatic meteor radar, and SABER data taken during the SIMONe–2018 campaign" *by* Fabio Vargas et al.

Anonymous Referee #1

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This manuscript presents a fairly detailed investigation of gravity waves observed in airglow image data obtained during an observation campaign in November 2018, from Northern Germany. It is well-written and provides a good assessment of the effects of GW on the mesosphere, even if it is during a limited period of time. However, the authors should address the following comments:

- It is a little bit surprising that there are no waves with intrinsic period <20 min. Most of the waves should be propagating against the wind, therefore their intrinsic phase speed should be larger than the observed one, and their intrinsic period should be

smaller than their observed one. - lines 255-271: It is complicated to compare the results with Li et al., 2011, because of the time resolution ($\sim$10 min) which does not allow for detecting most short-wavelength, short-period waves. As also noticed in the manuscript, this campaign covered only a few days, while Li et al. results represent the whole year. Same problems with Li, 2011. - lines 284-285: These mean values are very small, especially considering the error. What does -0.36+/-1.51 mean? -0.36+1.51=1.15, so the MF is carried in the other direction? Or you are sure about the wave direction of propagation, and so the minimum possible value is 0? I am not sure how it is possible to measure such small MF values giving the uncertainties in the measurements. - line 286: The total value is very misleading and cannot be used for any future comparisons. With the analysis method used in this paper, GWs are measured for each image, therefore the more images, the larger the total MF. If the imager cycle was 1 min instead of 10 min, the total MF would be 10x larger because 10x more waves parameters would have been measured! An average MF value for each wave and its duration could be more useful to assess its impact. The statistical results (11% of the waves responsible for carrying 50% of the MF...) are still relevant but the authors have to be very careful when giving the "total" MF. - The number of waves detected (362) is also controversial. There were NOT 362 separate GWs propagating over the observation site during these 4 nights! There were 362 wave measurements, which is different. Some of the waves probably lasted for hours and their parameters were measured several times. Of course, these parameters can evolve depending on the forcing and the background atmosphere conditions, but there were still the same GWs coming from the same sources. - The GWs were observed in 4 different airglow layers, at 4 different altitudes. Could you give more information about that? Which waves were observed simultaneously? It would also be interesting to see the difference in MF between the airglow layers. The MF divergence would be very interesting, as well. - You should mention ducted waves. Even if the waves presented in this paper are unlikely to be ducted because of the minimum intrisic periods, these waves exist but do not carry any vertical MF, so they would biased your results. Again, a comparison between the different airglow layers would help. - It is probably beyond the scope of this paper, but given the small number of nights, looking for sources (tropospheric weather, convection, fronts...) would be interesting.

Minor comments: line 12: can you add the number of nights/hours of observation? It would give a better idea of the importance of the MF values. line 26: remove "successfully" line 93: long-period variations Section 2.2: what are the vertical and temporal resolutions of the radar system? line 137: FWHM line 175: "duration" instead of "length" line 177: The MF equation is proportional to Lz/Lx, so it is not that surprising that large horizontal wavelength waves carry less MF. It is more surprising that large vertical wavelength waves carry less MF, though. line 191: You must have done your filtering wrong if the cutoff was 5 hours and you still see that many peaks, especially the ones around 6, 8, and 12 hours! The peak at 8.9 hr is pretty close to the peak at 8 hr. You have to give the resolution of the spectra, otherwise this is not very convincing. line 204: The keograms could be improved by flat fielding the images first. Not sure if this had been done, but it would improve the signal at the zenith compared to the edges. Figure 8a bottom is misleading. It looks like the wave tilt (which is related to the direction of propagation and phase speed) corresponds to the decrease in meridional wind intensity. Not sure how to avoid that. You should add the directions of propagation in Table 3. line 221: What is the error on the horizontal wavelength measurements? line 235: 8 hours, could be a tidal component. line 293-294: The last sentence is enigmatic. If these larger amplitude waves are only seen in the O2 emission but not below (OH, Na) or above (OI), it is quite puzzling. Lines 363-365: Can you rephrase this sentence? It is not very clear. Line 378: The links are missing! Figure 4: (d) Calculated volume emission rates... Figure 7: Can you add a detection threshold?

The comparisons with previous publications are lacking some relevant studies: eg, Bossert et al., 2015, for MF of long period waves, Cao and Liu, 2016, for the impact of large vs small MF GWs, and even on the same topic Hertzog at al., 2012, Plougoven et al., 2013, or Wright et al., 2013. The last 3 describing stratospheric measurements,

though. The comparisons with Li, 2011, and Li et al., 2011, are not very relevant except because they used a similar analysis method.

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