

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 #2

Received and published: 15 October 2020

This manuscript deals with gravity waves detected using an airglow imager and discusses their effect on the background wind, combining collocating meteor radar network data and satellite data as background information. Their effort to make full use of the obtained data during the limited campaign period is much appreciated: expanding their analyses to waves with horizontal scales larger than the field of view of the imager. However, the overall analysis methods often lack detailed explanation and hard to follow, and some part of them even seem inadequately done. The authors are advised to revise the manuscript substantially by addressing the following comments.

[Printer-friendly version](#)

[Discussion paper](#)



Major comments:

The sequence interval of the imager observation is 10 min, and the exposure time is mostly 2 min. This means that possible aliasing should be taken into account in the analyses although it is not described in the manuscript. Because of the 2 min exposure, any structure with a period shorter than 4 min will be smoothed out and not be resolved, but those with a period between 4-20 min can easily alias into slower period motions, and significantly affect the results.

In detecting wave structures from wind data, a high-pass filter with a cut-off of 5 hr is used. However, the detected wave periods are around 4 and 8 hr, which are close to or longer than the cut-off. The wave amplitudes and also phase structures will be largely affected. Even a spectral analysis is done using the filtered wind, and fluctuation components with periods longer than 4 hr are discussed (Fig 7), leading to the subsequent analyses of those components based on the filtered wind as shown in Figures 8 and 10. Although the authors mention these peaks as 'leakage' (line 190), I am afraid that such data treatment is not acceptable at all. What the authors can remove before the spectral analysis in the present case would be only the background wind (mean wind) and trend. Since the imager data is unfiltered, wind values should be treated in a similar manner in comparison.

The details of 2D spectral analysis method shown in Fig 9 are not clear. The keograms shown in Figure 8 are thought to be used, of course, but how is the calculation done? The borders of the keograms can significantly affect the results if miss-handled. Isn't there a possibility that the evening twilight seen around 5 pm affects the results? In other words, what kind of spectral window is applied in the analysis?

Minor comments:

Abstract

Line 1 'large and small'



Not quantitative. More specific description is wanted. The same comments apply to the other 'small' and 'large' in the manuscript.

Line 6 'flux divergence estimations'

Momentum flux is estimated, but the divergence does not seem estimated, instead MF is just assumed to be deposited around the mesopause.

Line 11 Mean MF, total MF and number of detected events

585.96/362 is not 0.88. How is the mean estimated? Is the detected number 362 the number of independent wave events, or multi-counted number? The same comment to the lines 285-286.

Lines 17-18

The lidar data in the upper stratosphere is not actually analysed in the manuscript, but only qualitatively mentioned. This statement does not seem appropriate in the abstract.

The main body of the manuscript

Line 52 'modes'

Meaning not clear.

Line 58 'large amplitude filtered wind fluctuations'

Meaning not clear.

Line 84 'long period oscillations'

The wave structure is not very clear. Even one cycle is hard to distinguish, especially for the Nov. 06-07 case.

Line 91 'time difference'

Is this done using pairs of the adjacent 10 min separated images? The detail should be written.

[Printer-friendly version](#)

[Discussion paper](#)



Interactive comment

Line 116 'smaller' > shorter

Line 116 'simultaneously the back. . . .'

'with' after 'simultaneously'

Line 118 'Fig. 1c'

Meant to be Fig. 1d?

Section 3

The definition of 'small' and 'large' should be described at the beginning of Section 3.

Sub-section 3.2

The meaning of 'fluctuation' at line 182 is not clear although it is inferred later at lines 190-191. It is confusing. The filter characteristics also should be written in the caption of Figure 6.

Lines 210, 211, 219, 220, 224, 234, 235 316, 317, 320 and 321

Why are the uncertainly of all these values '+-1'?

Line 244 'amplitude'

Since the background wind is not thought to be a wave, rewording will be appropriate; something as magnitude.

Line 245

Absorption occurs at a critical level, but reflection occurs at a turning level. Different wind structures are responsible, perhaps as you know.

Lines 283-284 'toward the west of $-0.36+-1.51 \text{ m}^2/\text{s}^2$ '

Very confusing description.

Lines 286-288

[Printer-friendly version](#)

[Discussion paper](#)



The finding of a small number of waves carrying a large portion of MF is a good point. I suggest that the authors include discussion on GW intermittency and its importance in model atmosphere studies.

Lines 289-294

This can largely affect the obtained results. Hopefully more discussion wanted. Isn't there any possibility that the layer thickness is related, or is it already taken into account in the analysis? It seems from Figure 4 that O2 layer is the thinnest.

Line 296

Yes, if the acceleration or deceleration occurs all along the same latitude circle, to my understanding.

Lines 339-340

I think this is a nice point. In the radar studies, a long-lasting topic has been which period range of GW is most responsible for MF deposition. Short period GWs were thought to be responsible before, but recent studies suggest that longer period waves are responsible. One example I know is Sato et al, 2017 (<https://doi.org/10.1002/2016JD025834>). There can be more. Including such preceding studies will be beneficial for even more fruitful discussion.

Figure 2 '> 5 hours' should be '< 5 hours'?

Figure 5

Are these dots independent wave events with each other?

Figure 6

Meaning of 'fluctuation' should be described clearly. High pass filtered?

Figure 7

I am afraid that a spectral analysis after applying a filtering is not acceptable.



Figure 8

Image data and wind data should be treated in a similar manner; detrend, background removal or whatever.

Figure 9

The way of 2D spectral analysis should be described in the manuscript.

Figure 10

If all the figures are processed with the high pass filter with the cutoff of 5 hr, reprocessing is suggested, at least, for the November 6-7 case.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2020-896>, 2020.

[Printer-friendly version](#)

[Discussion paper](#)

