Response to reviewers' reports on the paper amt-2019-79 Advanced hodograph-based analysis technique to derive gravity waves parameters from Lidar observations

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We appreciate the reviewers' constructive comments and their positive judgment on our paper. We have taken the reviewers' suggestions into account when preparing the revised version of our manuscript.

However, we would like to make a general comment. This paper is submitted to AMT with purpose to describe a method of analysis. We demonstrate on a data set how this method works. We also demonstrate how to obtain extended set of GW parameters and summarize equations and assumptions used for estimation of different parameters. We do not claim that this data set represents a "typical" situation in polar winter season. Thus, in this manuscript we try to avoid making general conclusions like behavior of momentum flux or vertical wavelength as a function of altitude or any other parameter. We are currently working on another manuscript where a larger data set is analyzed by this method. We will take into account the corresponding suggestions of referees when preparing the next manuscript.

In the following we address the comments of all reviewers point by point.

To Referee 1

1) The current structure of the paper makes it hard to follow the story at some point. This is especially true for section 3 (theoretical introduction) which already presents one part of the analysis method, i.e. hodograph analysis. I suggest to include the content of current section 3 in the next section and place the details about hodograph analysis in the respective subsection.

To address this reviewer's comment we revised the sections 3 and 4 to make the theory and the analysis technique to be clearly separated.

2) Scaling seems to be an essential step of the analysis (section 4.2). Here, you can refer to Wright et al. 2017 who applied the scaling to satellite data. They used a reference altitude in the middle of their observations (41 km). Can you tell if the scaling altitude has an influence on your results? One may question if it's reasonable to scale amplitudes to surface values (z=0) for measurements starting above 25 km.

To address this reviewer's comment we added two notes, in Sec. 4.2 and 4.7, respectively.

In Sec. 4.2 **Scaling of fluctuations**:

Note however, if further analysis requires treatment of fluctuation amplitudes, this scaling must be either taken somehow into account (e.g., by appropriate normalization) or removed (by applying inverse scaling) as we do in Sec. 4.7.

In Sec. 4.7 **Calculation of GW parameters**:

Note, that as mentioned in Sec. 4.2, at this point the fluctuation amplitudes must be rescaled back to their original growth rate with altitude using the derived scaling parameter ς, to legitimate their use for e.g., estimation of wave energy.

Here we give a more detailed explanation which, we believe would disimprove the readability of our manuscript.

The scaling altitude does not affect the final results neither in the analysis used by Wright et al. (2017) nor in the analysis shown in our manuscript. The reason for that is the inverse rescaling applied to the fluctuations before their actual use: Wright et al. (2017): *"This restores the true height-scaling of the measured wave amplitudes, typically exponentially increasing with height"*.

Note, however, that our scaling approach is different to what has been used by Wright et al. (2017). Namely, instead of using $\exp((z-z_0)/(2H))$, we apply scaling $\exp(z/(\varsigma H))$, where parameter ς is individually (and automatically) adjusted to every profile at step 4.2 (scaling) and is further used for inverse scaling at step 4.7 (calculation of GW parameters).

In the approach used by Wright et al. (2017), the choice of z_0 can only influence amplitude of fluctuations if it increases with altitude not as $\exp((z-z_0)/(2H))$, but $\exp(z/(\varsigma H))$. Figs. 1, 2, 3 demonstrate the simulated Wright et al. (2017)'s scaling process.

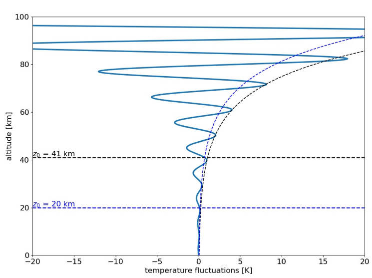

Figure 1. Vertical profile of temperature fluctuations. Black (blue) dashed line was estimated for $z_0 = 41km$ ($z_0 = 20\ km$)

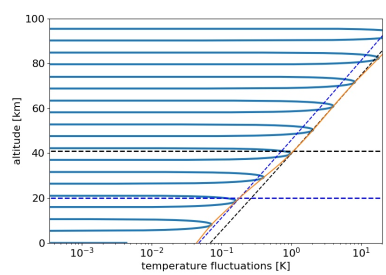

Figure 2. The same as in Fig. 1, but in logarithmic scale. Orange line demonstrated wave amplitude used in simulations.

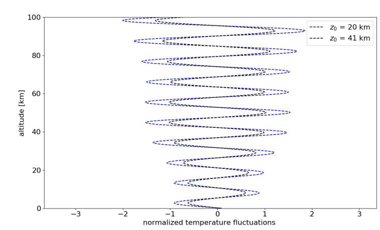

Figure 3. Vertical profile of temperature fluctuations normalized by $\exp((z-z_0)/(2H))$, where $z_0 = 41km$ ($z_0 = 20\ km$) for black (blue) lines

Here we simulated GW with increase of its amplitude with height as $1.0/\sqrt{density}$, where $density$ was taken from the NRL-MSISE00 model. The continuous blue line shows this GW in Fig. 1 and 2 in linear and log scale, respectively. The GW-amplitude increase of $1.0/\sqrt{density}$ as it is derived from the MSIS data is shown by the orange line in Fig 2. Note, that we used MSIS density profile for January because it reveals more pronounced difference between $\exp((z-z_0)/(2H))$ if compared to summer. The dashed blue and dashed black lines in Fig. 1 and 2 show the scaling factor $\exp((z-z_0)/(2H))$ derived for $z_0 = 20$ and $z_0 = 41$ km, respectively. Whereas orange line represents the natural GW-amplitude increase. As it is seen in logarithmic scale (Fig. 2) the both dashed lines (i.e., for $z_0 = 20$ and $z_0 = 41$ km) are parallel to each other and they differ only because the increase of the "natural" (=MSIS in this case) GW-amplitude varies with altitude. This variation produces such altitude-dependent difference between $\exp((z - z_0)/(2H))$ for different z_0. In summer case these both lines will be identical and no difference for different z_0 will be observed.

After applying these two different scalings (derived for different z_0) we get fluctuations shown in Fig. 3 as dashed blue and dashed black lines for $z_0 = 20$ and $z_0 = 41$ km, respectively. These two profiles of scaled fluctuations reveal similar behavior as far as altitude dependence is concerned (Fig. 3).

One may question if it's reasonable to scale amplitudes to surface values (z=0) for measurements starting above 25 km.

This question arise more likely because we use a function $\exp(z/(\varsigma H))$ for normalization (i.e. $z_0 = 0$). We can rewrite $\exp((z - z_0)/(\varsigma H))$ normalization as $\exp((z)/(\varsigma H)) \cdot \exp((-z_0)/(\varsigma H))$. Since we assume, that ς and H are constant at given altitude range, we can rewrite this normalization as $const \cdot \exp(z/(\varsigma H))$. Thus, we can divide our observations by this $const$ and later multiply results by the same $const$. Finally, results will be the same.

3)I don't fully understand how the fitting process of the cosine functions works. Please, try to clarify. What is prescribed in the first guess? Where do the values come from? See comments P8, L3; P8, L27; P9, L15

To address this reviewer's comment we completely rewrote the Sec. 4.4 (Fitting of linear wave theory) to make the description of fitting process better understandable (see revised version of the manuscript).

Minor/detailed comments:

P1, L12-15: It doesn't seem necessary to give/repeat details about the (hodograph) technique here. ...We identified 4507 quasi monochromatic waves. In the vicinity of the polar night jet...

Changed as suggested.

P2, L2: define small scale (horizontally, vertically)

Improved as suggested:

... *waves with horizontal wavelengths typically shorter than 1000 km.*

P2, L7: high resolution numerical modelling is also a useful tool; please remove "only"

Changed as suggested.

P2, L24: again, what is meant by "these small scale waves" P3, L4: I don't think that geophysical meaningful results are enough to justify the capability of the method at this point. I suggest to simply go with "Finally in section 5, the capability of the method is demonstrated with continuous ALOMAR lidar data during a four day period in 2016.

To address this reviewer's comment we made it more specific in the text:

...lidar technology give us new possibilities to study GW experimentally on a more or less regular basis and resolve spatial sales of 150 m in vertical and temporal scales of 5 min

P5, L3: comment shows up: "% begin equation"

corrected as suggested

P6, L7: This means your algorithm doesn't take into account stationary waves because they are assigned to the background. Should be mentioned here.

The sentence that confused reviewer was: "*We define the background as wind or temperature fluctuations with periods longer than 12 hours and vertical wavelengths longer than 15 km.*"

Thus, our algorithm indeed excludes stationary waves if they have vertical wavelength longer than 15 km. Stationary waves with shorter vertical wavelength are not removed. This can be seen in Figures 2-4. Middle panels demonstrate obtained backgrounds and lower panels demonstrate remained fluctuations. Structures defined as background do not reveal something like "stationary waves". On the contrary, remaining fluctuations demonstrate in some places such a behavior. For example, some fluctuations below $\sim$40 km look like stationary waves (especially in meridional wind, lower panel of Fig. 4) To note again, this is the advantage of the 2D-FFT method if applied for the background removal. See also our previous response to reviewers, where we had a detailed discussion about background definition (Appendix B), where different approaches were discussed.

Note also, that we changed the colormap when preparing the new version of manuscript in order to demonstrate fluctuations in more appropriate way.

P6, L13: "...which might only be produced by gravity waves...": I don't think this is true and you already mentioned in your introduction that wave structures must be distinguished from e.g., turbulence. The fluctuations need to follow the GW-dispersion relation which is hard to prove in measurements as one usually lacks either vertical or horizontal information of the wave structure.

To address this reviewer's comment we rephrased the sentence to make it clear that: *After subtracting the derived background from the original measurements we obtain the wind and temperature fluctuations which have periods shorter than 12 hours or wavelengths sorter than 15 km.*

P6, L19: "...skip this step from the analysis.": this conclusion doesn't make sense to me. Don't you need fluctuations of u, v, T for all the analysis?

To avoid such a confusion we rephrased this sentence as follows: *The new technique is not sensitive to the background derivation schemes and may use simpler background calculations like constant values in time.*

P7, Fig. 5: Did you apply zero-padding to the data? You should explain and include the cone of influence of the wavelet analysis. It limits the interpretation of signatures at the edges and the true vertical extent of packages with longer wavelength.

We agree with the reviewer that the wavelet transform would reveal limitations connected to the finite length of data set like edge effects etc. We believe that after significant revision of Sec. 4 (as was requested by the reviewer in his/her major comments above) it should be clear now from the text, that the wavelet analysis is only used for estimating initial guess for the further and more robust part of the analysis. That is, the next after wavelet steps do refine the picture and yield more details than can be inferred from the wavelet analysis.

P8,L3: I can not fully follow the description in this paragraph. You start with a first guess from the scalogram for z0 and vertical wavelength (are you automatically searching for the maxima?). Your fitting reveals intrinsic frequency and propagation direction + corrected z0 and vert. wavelength? The equations for T', u', v' depend not only on z0

and vertical wavelength. Which values are you using for the wave packet width and intrinsic frequency? Propagation direction is calculated afterwards using Eq. A4. Isn't the conclusion then that Eq. A4 is not well performing for intrinsic periods larger than 1h OR the whole fitting process including z0 and vertical wavelength brings some uncertainty?

To address this reviewer's comment together with major comment above we rewrote the section 4.4. In particular, we made it clear in the text, that the uncertainty connected to direct application of Eq. A4 to noisy data can be avoided if we apply next steps in our algorithm instead. Namely, we propose to apply the hodograph method to the extracted wave packets to precisely derive further wave parameters.

P8, L12: "For low frequency GW, i.e. those with periods close to the Coriolis period ($2\pi/f$) the fluctuations reveal a circle." This does not agree with the fact that hodograph method/stokes analysis is especially used and appropriate for gravity waves with intrinsic frequencies close to the inertial frequency (<10f), i.e. showing an ellipse in the hodograph?

If coriolis parameter is equal to intrinsic frequency, i.e. $f = \widehat{\omega}$ in Eq. 2, we get $\widehat{v}_\perp = -i\widehat{u}_\|$. Alternatively, if we use Eq. 7.68 from Holton (2004) we can rewrite wave fluctuations as:

$$u' = |\widehat{u}| \cdot cos(kx + mz - \widehat{\omega}t) \tag{1a}$$

$$v' = |\widehat{u}| \cdot sin(kx + mz - \widehat{\omega}t) \tag{1b}$$

That is, equal amplitudes and phase shift of $\pi/2$ means circle. At ALOMAR location $2\pi/f \simeq 12.8$ h. This means, that GW with intrinsic periods close to ~ 12 h reveal rather circle than ellipse.

P8, L27: "Additionally we calculate a vertical wavelength by requiring the hodograph to close the full $360\circ$ cycle." How is this done? Why all the effort to correct the vertical wavelength in the previous step if you could use this value anyway?

To address this reviewer's comment we improved the description of this procedure: *This correction to the vertical wavelength is found by forcing the hodograph to close the full 360 ° cycle and calculating the additional vertical length resulted from this extra rotation.*

We also note here, that the hodograph technique is very sensitive to the data quality and, in particular, to such specific difficulties like limited dataset or insufficient resolution. That is why we developed this more extensive algorithm of GW-analysis that combines different techniques and uses their advantages when we believe it is more appropriate. Hodograph alone usually fails if its rotation is considerably smaller than 360 °. Also, by iterating wave fitting and hodograph we make another consistency check which improves the robustness of our analysis.

P8, L1: Did you account for the influence of transverse-shear on the axial ratio of the ellipse? *correction given in: Vincent R.A., Allen S.J., Eckermann S.D. (1997) Gravity-Wave Parameters in the Lower Stratosphere. In: Hamilton K. (eds) Gravity Wave Processes.

Vincent et al. (1997) concluded, that this effect is not significant ($\sim 6\%$ in winter). From our data analysis we derived similar conclusion. Moreover, since background wind during our observations was restricted to range of azimuth from $\sim 0°$ to $\sim 45°$, transverse shear was restricted to ranges of azimuth from $\sim 90°$ to $\sim 135°$ and from $\sim 270°$ to $\sim 315°$, where amount of observed waves is minimal. Thus, the quantitative effect of such correction is not significant and only small fraction of detected waves is affected by this correction. For test purposes, we applied such corrections, but in plots, shown in the current manuscript it would be hard to see any differences. On the other hand, Hines (1989) introduced such correction for wind profiles without background removal. Since we remove variable background wind, some effect from vertical displacement due to background wind gradient can be attributed to background and hence, has no influence on the observed ratios. Thus, in order to apply this correction to our data, we have to demonstrate, that correction introduced by Hines (1989) is meaningful for our data analysis. Since impact of such correction (if applied) is negligible, we decided to not include it in our algorithm (i.e., our results are without correction).

P9, L15: "That is, the dominating frequency is used as a zero guess for the fitting of Eqs. 1 to derive exact values of z0 and λ_z.." Now, I am totally confused (see comment P8, L3)

We are grateful to reviewer for the careful reading. This is indeed a typographic error and it is now corrected (removed).

P11, L1: I think you already have demonstrated how the method works with real data profiles. This section now shows "Finally, this algorithm for a single point in time is subsequently applied to all time points of the entire data set shown in Fig. 2, 3 and 4." as you say at the end of the previous section. Maybe you can just shift this sentence to the beginning of this section.

To address this reviewer's comment and to avoid such confusions, we added Section 4 **Reconstruction of 2D fields** where we mentioned explicitly: *Finally, this algorithm for a single point in time is subsequently applied to all time points of the entire data set shown in Fig. 2, 3 and 4. Thereby two dimensional time-altitude fields of GW parameters can be reconstructed, which is demonstrated in the next section.*

P12, L1-8: I recommend to put this paragraph prior to the up/downward discussion of literature.

Changed as suggested.

P12, L6: Any physical explanation for this finding? Enhanced vertical wavelength due to high wind speed?

We appreciate the reviewer's analytical reasoning and agree that a more in depth study with emphasis on physical interpretation is needed. However, as we noted already above, we decided to publish such study in a separate paper and involving a lager data set.

Regarding this particular reviewer's point, we can speculate e.g., that in the regions where background wind is very strong the linear theory used in our study fails to describe GW properly. More specifically, our algorithm does not find GWs in this region. This finding has to be studied in more detail before we start to argue for any particular reason.

P12, L10: No scaling of the amplitudes? To enhance the visibility at lower altitudes compared to higher altitudes, it may be useful scale amplitudes in

It gave us great pleasure to see that the reviewer appreciates our scaling approach which could also be appropriate in this case, as noted by the reviewer. Nevertheless, we decided to show the reconstructed fluctuation in real physical units (K) to make it easier for experimenters to compare this result with their measurements and to get better filling of the output expected from such analysis.

Fig. 10. P14, L14-15: But didn't you mention earlier that the sensitivity of your analysis to the chosen background is small?

P14, L17: compare comment P6, L19 P14, L21: Wright et al 2017, see major comment 2

Addressing these two points together, we improved the wording to make it clear in the manuscript that:

1) Among different techniques for background removal we give preference to the 2D-FFT method.

2) Our algorithm to detect GWs is so insensitive to the particular background removal scheme (which is opposite to common knowledge and practice) that it is enough to simply extract a mean value (e.g., to decrease computational load/time)

See also reply to the major comment 2.

P15, L12: "additional robust algorithm to pick out wave packets automatically". Isn't this what your algorithm does already as implicated by "our algorithm resolves many more GWs than it can be inferred by manually applied hodograph technique"? Please clarify.

To address this comment and to avoid similar confusions we slightly extended the summary to make it clear that our technique is automatized in spatial domain and not yet in time domain.

Another specific feature of our analysis technique is the extension to the linear wave theory introduced in Sec. 3, the wave packet envelop term $\exp(-(z-z_0)^2/2\sigma^2)$ *that accounts for limited presence of the GW-packet in observations. This, however, only works in spatial domain, i.e. vertically. At the current stage of development our analysis technique is not capable of detecting life-time of gravity waves in observational data set. This capability is currently under development as well as an additional robust algorithm to pick out wave packets in time domain automatically.*

P15, L14: holographs should be hodographs

Corrected.

References

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