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 2

To test this approach, I suggest the author combine the reconstructed upward and downward perturbations for temperature and wind fields in Figure 10 and 11, and compared with the real perturbations shown in Figure 2,3,4. The total reconstructed perturbations should be quite close to the measured perturbation.

To address this reviewer's comment we made the 2D (time vs altitude) plots of the total reconstructed perturbations. As an example, Fig. 1 demonstrates the reconstruction for meridional wind. Original measurements, reconstructed GWs, and the difference between those are shown in the upper, middle, and lower panel, respectively.

As can be seen from Fig. 1, the reconstructed GW-field (middle panel) resembles the picture formed by the measurements (upper panel), as far as GW-structures are concerned. The lower panel reveals very small-scale noise as it is expected from data treatment and is described in the manuscript. We should admit, however, that critical reader could argue about comparability of and similarity in these figures, as well as about meaningfulness of the noise shown in the lower panel of Fig. 1. To make a justified judgment on such figures one needs to have quite some experience in such (e.g., lidar) data analysis. In other words, we think that non-experienced (in data analysis) reader could rather be confused or missleaded by such figure. Also, including

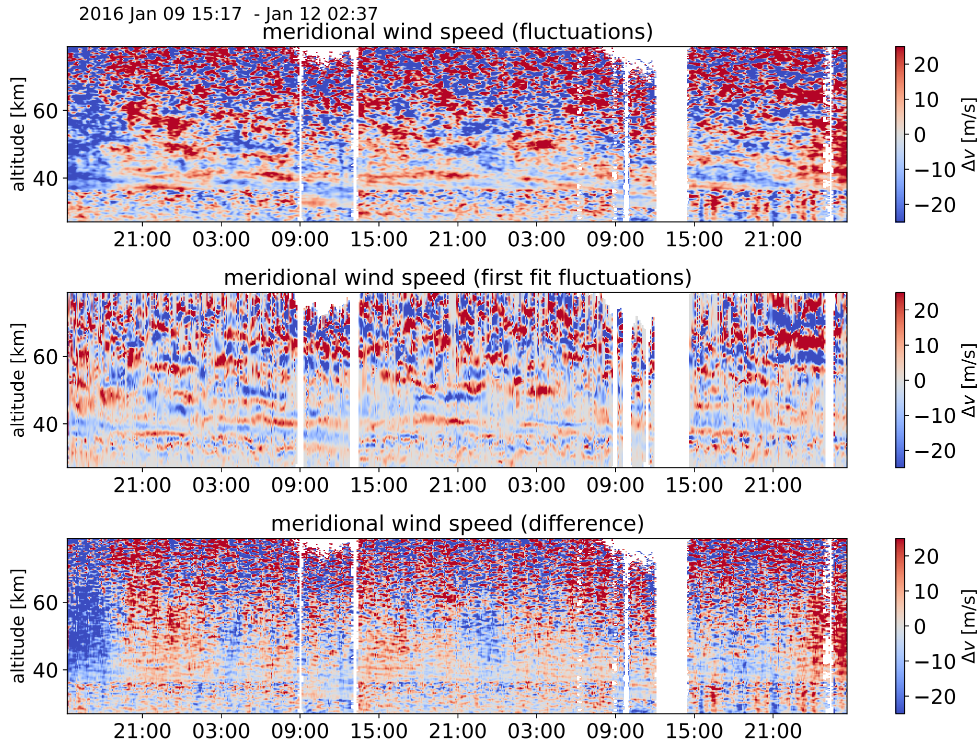

Figure 1. Upper panel: Observed fluctuations of meridional wind. Middle panel: reconstructed fluctuations. Lower panel: Difference.

such plot in the manuscript would need to describe and explain vast of details and thereby, defocus the paper. That is why we decided to not include such comparison in the manuscript.

On the other hand, the next reviewer's suggestion looks brilliant and, in our opinion also serves the same purpose.

It would also be helpful, if the author could test the results in temporal domain by looking at the wavelet (or lomb-scargle) results (in time) of the real perturbation and the total reconstructed perturbation, to see if the algorithm does not lose the temporal variations of these waves.

To address this comment and, as mentioned above we believe it supports also the idea in the previous point, we added Fig. 13 with to spectra to the manuscript, as well as a short discussion.

Another way to check the consistency of our technique is to look at the spectrum of fluctuations before and after analysis. As an example, Fig. 13 shows Fourier spectra of the temperature fluctuations calculated in time domain. The measurements and analysis results are represented by blue and orange lines, respectively. We recall that the analysis is made in spatial domain, that is it only deals with altitude profiles of fluctuations. Close similarity in both spectra which were calculated in time domain, that is across the analyzed profiles, suggests that the reconstructed two dimensional (time vs altitude) GW-field does *not significantly deviate from the observed one. The reconstructed field indeed reflects the main GW-content and, therefore, in this respect it may be qualified as lossless algorithm.*

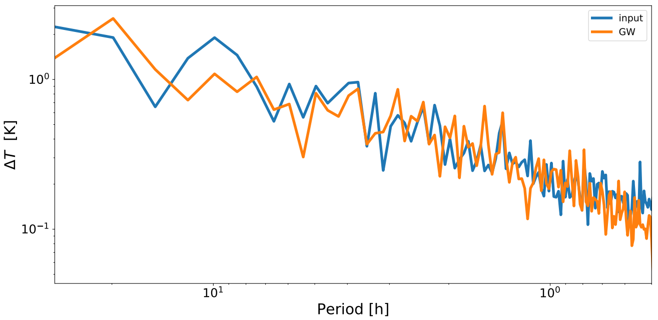

Figure 13. Fourier power spectra of measured temperature fluctuations (blue) and of the reconstructed GWs (orange).

I would also like to know the measurement uncertainties during this lidar campaign, although I am aware that the author treats (weights) every lidar measurement the same (without error?). This is a numerical technique based upon lidar observations, so, I think it is important to know the data quality.

We completely agree with the reviewer that it would be great to derive valid uncertainties when analyzing experimental data. However, the error propagation issue is not fully addressed in our manuscript since algorithm includes many different data analysis techniques among which the error derivation methods are not well established. So, for instance, is the 2D-FFT analysis or the background removal procedure itself. Frankly speaking, we do not have a clear idea how to estimate uncertainties introduced at some steps of our analysis. Thus, for instance, we can quite precisely derive the uncertainty for every single fit of the harmonic waves (step 4.4), but we cannot even approximately estimate the uncertainty regarding how precise (or applicable) is the fitted linear model for any particular observation, or how significant is the part for which the fit did not converge (for whatever reason). In other words, even though we are working on this issue, at the moment it does not look convincing to us to discuss the errors related to the reconstructed GW-field in this manuscript.

We can comment, however, on the question how measurement errors affect this data analysis. Certain steps in our analysis algorithm directly deal with the measured quantities. These are the places where the uncertainty propagation mathematics can be directly applied. Thus, e.g. as mentioned above we can derive fitting error if measurement errors are known. However, we do not see that these errors can enlighten real uncertainties related to entire analysis.

Also, to our knowledge, there is no e.g., rigorous mathematical theory to describe error propagation through hodograph analysis. This, we believe, must account also for errors connected with sampling rate and its relation to the eigenfrequencies of the system (GW-filed) under investigation. The similar problem arises also for spectral analyses.

References