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Atmospheric Measurement Techniques Discussions

Interactive comment on "Evaluation of methods for gravity wave extraction from middle atmospheric lidar temperature measurements" *by* B. Ehard et al.

Anonymous Referee #1

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Lidar temperature observations of gravity waves (GW) always have to solve the problem of extracting the GW signal from the background composed of mean temperature structure, planetary and tidal waves. The paper by Ehard et al. compares different methods of background temperature subtraction, either in the temporal or spatial domain. The methods are applied to synthetic data of monochromatic gravity waves propagating through steady-state or time-varying background atmosphere, and to a true time series of temperature observations. All methods have their particular pros and cons. Finally, the authors suggest a Butterworth filter to remove the background temperature data.

The paper by Ehard et al. is well written, concise and clear. Even if the Butterworth filter might not be optimal for all applications, the synthetic data study allows the reader to understand the different methods and to choose the best method for the particular task. An important result of the paper is the dependence of the retrieved GW parameters on the chosen method and therefore the limitations for comparison of studies using different methods. The paper should be published after the following comments have been addressed.

General comment: The authors simulate the effects of different GW extraction methods using a monochromatic wave with, of course, distinct properties and discrete resolution in altitude and time. Altitude range and length of the data are set to typical, but fixed values. Depending on simulation either the vertical wavelength or period of the wave are changed. Unavoidable, part of the results depend on the chosen set of parameters and the limited resolution. Unfortunately, in the description of the results often it remains unclear which effect can be generally expected by the particular method, and which is only valid for this special combination of parameters. I suggest making this more clear for every case study. Please compare specific comments below.

Specific comments:

p. 9054, l. 3: The authors should make clear that the overestimation is a result of the selected wave period and running mean width. As, e.g., visible in Fig. 3, other periods result in underestimation or even stronger overestimation of the "true" perturbation. Obviously, depending on period, different fractions of the wave variance are attributed to the background.

Figure 2 a, b: I assume that the patterns visible in the figures is mainly due to numerical effects and not a general feature of the filter. Please comment.

p. 9054, l. 6: It should be explained whether the 9 km feature is a general result of the polynomial filter method with this particular parameters.

p. 9055, l. 1-3 (Fig. 3b, e, f): I do not understand the reason for the oscillations at very small periods. While the slow decrease of T' (yellow line) at large periods is reasonable and obvious, it is unclear why short period oscillations should hide in a 3 h averaged background. Please explain!

Figure 3 a, b: As in Figure 2, I assume that the fast change in response with altitude is a numerical effect and not a general feature of the filter. Please comment.

Figure 3 c, d: Please comment on the strong change in response below 30 km and above 80 km.

p. 9055, l. 5-7: I think it is not only a potential phase delay being responsible for the over- and underestimation of the wave. Both vertical filtering methods react similarly with periodic changes in response by up to 20% for long-period waves. Potentially this is because spatial and temporal scales of the wave are related via Eq. 6. The authors should explain their filter characteristics for long periods in more detail, especially since they propose their application for interrupted data sets, or generally speaking for data sets where the continuous time series is shorter than the GW period. It should also be noted that the flat response of the Butterworth filter (as well as the polynomial fit) (Fig. 3e, f) is partly a result of the 10 km vertical averages of T'.

p. 9055, l. 22: This overestimation by the nightly mean method is a very interesting result. Do you can explain why the overestimation happens at this specific altitude and whether this result can be generalized to other situations?

p. 9058, l. 10-11: The increase of GWPED with altitude is an important topic. It would be interesting to learn why the running mean method results in a stronger increase of GWPED with altitude, compared to other methods?

p. 9059, l. 25: As stated correctly by the authors, tidal signals are hard to retrieve from lidar observations of limited duration. Seemingly, in this case they discern GW and tides by their vertical wavelengths only. Unfortunately this is partly ambiguous

because similar signatures might occur also from GW of very large vertical wavelength or a superposition of waves at very different scales.

p. 9063, l. 14: I think this statement is too strong, even if the nightly mean method has indeed its limitations. Nevertheless, a correct application of this method takes the different measurement durations into account.

p. 9064, l. 7-8: The results presented here are mainly true for (nighttime) RMR lidar data only – or generally speaking for observations covering a large vertical range but limited time. Resonance lidar soundings are the main tool in the mesopause region, but cover only a limited vertical range even if they are partly not restricted to nighttime. This suggests other methods for GW analysis. I recommend making this clear and not writing about "lidar" in general.

Technical corrections:

p. 9058, l. 18: I would suggest calling this "increase" a "decrease with (increasing) altitude".

p. 9059, l. 14/15: Rauthe et al., (2006) write about resolved wave periods of 1.5-12 h in winter (1.5-3 h in summer), not lengths of observation.

p. 9062, l. 22: "may by" should read "may be"

Figures 3 a and 5 a: Insert "z" behind "Altitude"

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 9045, 2015.