

**Reply to acp-2017-167-RC1-supplement,
a review of the manuscript ACP-2017-167
“Winds and temperatures of the Arctic middle atmosphere
during January measured by Doppler lidar”**

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The authors present middle atmospheric wind and temperature observations of a lidar system in northern Norway during three Januaries. These observations are compared to the ECMWF and the HWM07 model. Besides the thermal and dynamical mean state, the authors also examine the variability caused by gravity waves and large-scale waves in the observations and the model data.

In a previous review I wrote to the authors “While the collocated middle atmospheric wind and temperature measurements of the Alomar RMR lidar are unique and unprecedented in their temporal and vertical resolution, I find it hard to learn something new from the paper. As it stands right now, the paper is mainly a comparison of different profiles, but no substantial conclusions are drawn from this.” This is still the case. Thus, I can only recommend publication of the article after substantial revisions.

Please find my detailed comments below.

Major comments

1. As said before, the paper currently lacks scientific significance. This becomes especially clear when reading the introduction: 50 % of the introduction are a mere review of different techniques to observe wind speeds in the middle atmosphere. The only hint for the importance of wind observations is given in the beginning when the authors state that “together with temperature observations, they [wind observations] also offer more sophisticated studies of gravity waves”. Why is this not done in this paper? Showing different profiles of potential and kinetic energy densities does not qualify the paper as a “sophisticated study”. To put it short: the paper lacks a scientific question which is investigated and answered in the end. Without a clear scientific question the paper remains unacceptable. A mere publication of the wind and temperature observations is unjustified in my eyes, despite the fact that it is the currently most extensive data set.

Following the suggestions of the short comment SC1 by *Dörnbrack (2017)* we included a quantification of the variability of winds and temperatures measured in the Arctic middle atmosphere; observations that have never be done before.

As mentioned earlier (e.g., *Meriwether and Gerrard, 2004; Drob et al., 2008; Dörnbrack et al., 2017*), wind observations in the middle atmosphere are of interest to infer direction and speed of gravity waves, to provide more input data and tests for empirical models like HWM07.

We highlighted this importance in the introduction.

2. Most of the very few conclusions drawn by the authors remain rather simple statements which purely describe the observations but the effects which lead to the observations remain in the dark. A few examples:

P. 4, ll. 26–29: the conclusion that the northern hemispheric polar middle atmosphere is highly variable can certainly be considered as textbook knowledge and is therefore redundant.

By quantifying the variability, as suggested by *Dörnbrack (2017)*, we now added additional value to the observations and the comparison to model data.

P. 5, ll. 21–29: the minor SSW and the following elevated stratopause event in 2012 have been well documented by previous studies. Also, as stated correctly by the authors, the mechanism for the formation of an elevated stratopause is known. Hence, I do not see the additional insights which are gained in this study from the combination of wind and temperature observations.

We are sorry that the reviewer did not see the new insight, so we tried to clarify this in the manuscript. In summary, we clarify that these are the first direct observations of winds and temperatures during an elevated stratopause event in conjunction with the reformation of the polar vortex. As stated in the manuscript, this situation is not well represented in ECMWF data, highlighting the need for observations.

We now highlighted in the manuscript why we think the data of this event is worth to be published: To quantify that a state-of-the-art weather model is still having some weaknesses in the middle atmosphere and even more observational data that are not assimilated in the model are needed to provide comparisons for model data.

P. 8, l. 33–p. 9, l. 2: The authors merely speculate on the effects which could cause the different gravity wave propagation conditions. Here, a thorough analysis is needed which investigates the propagation conditions in great detail.

We believe that a detailed investigation of propagation conditions will distract from the main messages and is beyond the scope of this paper. We mention two possible explanations for the observed effect of varying gravity wave propagation: 1. multiple origins of gravity waves; 2. changing background conditions. While the second option is clearly visible in Fig. 5 (large temperature gradient and strong wind shear), the first option can not be excluded.

We now mention in the manuscript that a clear distinction is not possible.

P. 10, l. 9: Why is the E_{kin}/E_{pot} ratio larger for the ECMWF data compared to the lidar data? What does this imply?

In general, a larger $E_{\text{kin}}/E_{\text{pot}}$ ratio indicates a larger ratio of wind fluctuations to temperature fluctuations. Inferring from the left panels of Fig. 8, the kinetic energy densities derived from lidar data and ECMWF data are of the same order, while potential energy densities are smaller in ECMWF data compared to lidar data. Hence, the day-to-day variability of temperatures is weaker in ECMWF than in the observations. This is obvious from the nightly mean profiles of January 2012 shown in Fig. 2.

We now mention this conclusion and the reference to Fig. 2 in the manuscript.

3. P. 8, ll. 25–26: the “approach using energy ratios has the advantage that an (energy weighted) intrinsic period for the ensemble of waves is calculated”. This statement is wrong! *Geller and Gong* (2010) derive their formula from the polarization relations which are fulfilled only for one set of wave parameters ($k, l, m, \hat{\omega}$). If a superposition of waves is to be examined you have to take the sum over the squared wave perturbations in their equations 7) and 8). If you do so and insert the summed polarization relations, you will not end up with a formula, which you can solve for the average frequency. In fact *Geller and Gong* (2010) note in their appendix A1, that their approach always results in larger values of $\hat{\omega}$ than the mean value derived by the hodograph analysis.

We have now revised this paragraph, clearly mentioning the assumptions made.

N.B., *Geller and Gong* (2010) found smaller values of $\hat{\omega}$ with the energy ratio method than with the hodograph method, not larger.

Furthermore, it should be noted that according to *Lane et al.* (2003) one can only see long-period inertial gravity waves in the horizontal wind speed fluctuations. Short period gravity waves exhibit more pronounced vertical wind perturbations. Thus the here applied methodology is already biased towards the large period gravity waves.

This limitation of the method is now mentioned in the manuscript.

If the authors want to infer gravity wave periods from their observations they have to use the hodograph approach instead of the energy approach. The energy approach can certainly be taken in the case of a quasi-monochromatic gravity wave field as shown by *Baumgarten et al.* (2015) but for an ensemble of waves it is not applicable.

The hodograph method is only applicable to the case of one single gravity wave, not an ensemble of gravity waves (e.g., *Sato*, 1994). In the case of an ensemble of gravity waves it is hard or even impossible to identify the superposition of ellipses in the zonal and meridional wind fluctuations. Therefore the hodograph method cannot be applied to observations not showing a quasi-monochromatic gravity wave field. On the other hand, the energy ratio approach yields results when applied to observations showing a superposition of gravity waves. In this case it has to be noted, that the so derived $2\pi\hat{\omega}^{-1}$ is not the intrinsic period of a certain wave.

We clearly address this issue in the manuscript now.

4. I still think that the comparison of the lidar measurements to the HWM07 model is not appropriate. HWM07 is a climatology and thus one cannot derive a meaningful mean profile from three years of observations in a highly variable surrounding (northern hemispheric polar middle atmosphere) which can be compared to this climatology. As a result the authors cannot differ whether the HWM07 takes too little observations into account (cf. p. 6, ll. 12–13) or whether their observations are simply too few for the comparison. Thus, I recommend removing the paragraph on the HWM07 comparison (p. 6, ll. 6–13) and instead focus the paper more on other aspects.

We are aware of the limitations that the reviewer list and they have been clearly stated in the manuscript. However, we think that the comparison to HWM07 is valuable for the scientific community as highlighted by the references given in the manuscript.

5. It seems to me that the ECMWF model does not contain any gravity waves above 40–50 km altitude. Here a detailed investigation of the reasons for this behavior is needed. At the moment I do not see any physical reason why the gravity waves should not propagate to higher altitudes than 40–50 km.

As mentioned by *Dörnbrack (2017)* “the numerical damping applied in the IFS” leads to an underestimation of the variability of winds and temperatures in the ECMWF data. We now mention in the manuscript that damping mechanisms in the ECMWF are the reason for the underestimation of variability, including a reference to *Jablonowski and Williamson (2011)*.

However, a “detailed investigation” of the behaviour of ECMWF regarding the damping of gravity waves is beyond the scope of this study and might be done by experts of the ECMWF model. This manuscripts provides strong hints that gravity waves are not well represented in the ECMWF model at altitudes above 40–50 km, including quantifications of this underestimation.

6. Regarding the methodology of extracting gravity waves from their observations: The authors state that they do not see any significant differences between their methodology and the Butterworth filter suggested by *Ehard et al. (2015)*. If this is not the case, I wonder why the authors do not adopt the Butterworth filter? One of the reasons for using the Butterworth filter is that it ensures a comparability of different studies since the same part of the gravity wave spectrum is extracted from the observations. In fact, *Baumgarten et al. (2017)* recently showed that by applying different methods of gravity wave extraction, a different seasonal cycle of gravity wave activity can be derived.

Numerous approaches to extract fluctuations caused by gravity waves have been applied to lidar data: filters in altitude (e.g., *Ehard et al., 2015*), filters in time (e.g., *Rauthe et al., 2008*), filters in both dimensions (e.g., *Baumgarten et al., 2015; Zhao et al., 2017*), or the variance method used by *Mzé et al. (2014)*. Probably all of these methods have their advantages and drawbacks, and it is simply not possible

to take all of them into account in every study about gravity waves. We mentioned the limitations of the approach we used in this study.

Concerning the comparability of different studies, the gravity wave spectrum taken into account depends not only on the applied vertical filtering technique but also on the temporal sampling of the data.

In a response to my previous review, the authors state that a further reason for not adopting the Butterworth filter is that “When applied to ECMWF data, the Butterworth and the spline method yielded physically dubious results (see Fig. 2): E.g., altitude profiles of GWED derived with the Butterworth method always showed similar oscillating behaviour above ≈ 65 km altitude; the ratio $E_{kin}=E_{pot}$ showed values < 1 for the spline and the Butterworth method, which can’t be true for gravity waves.” This argument can be dismissed in line of my major comment 5), since if there are no gravity waves in the ECMWF model above 40–50 km altitude, the results obtained by all methods are unphysical.

Given that it cannot be ruled out that ECMWF data might contain some gravity waves above 40–50 km altitude, the approach applied in this study was the only one of the three approaches tested that allowed to quantify the underestimation of GWED in ECMWF data.

Furthermore, the 10 h averaging applied by the authors has a significant disadvantage when it comes to analyzing the ECMWF data. I guess (see minor comments) that the authors use data from a different ECMWF run after 00 UTC. The corresponding switch from one ECMWF run to another is very likely to introduce a sudden jump of the temperature profile, which will be detected by the authors method, but not by a vertical Butterworth filter. For example the larger E_{kin}/E_{pot} ratios by the ECMWF compared to the lidar observations (p. 10, l. 9) could easily be an effect of the different ECMWF runs and analysis used here. In fact I think what you see in the large scale wave energy density is mostly affected by the data assimilation of the ECMWF and not the model dynamics. This has to be investigated with great care!

As the large-scale energy density relies on nightly mean profiles, we do not think that by using data of two different ECMWF runs per night the results might be corrupted.

Minor comments

1. In line with major comment 6): I do not know at which times the authors use analysis data and at which times they use forecast data. For example, ECMWF analysis data is available at 00, 06, 12 and 18 UTC, but one can also retrieve forecast data for these times. Also the authors do not state from which runs the data are taken (i.e. runs initialized at 00 or 12 UTC, or a combination of both). This has to be clarified.

As already stated in the manuscript, we use forecast data with 1 h time resolution.

We have clarified in the manuscript that we use both runs: the 00 UTC run for data between midnight and noon and the 12 UTC run for data between noon and midnight.

Furthermore, I was wondering, whether you extract the lidar data really at the named position, or whether you interpolate it horizontally to your lidar position?

We extracted the ECMWF data with horizontal resolution of 0.25° and interpolated these data on pressure levels horizontally to the location of ALOMAR.

This is now clarified in the manuscript.

2. Regarding the measurement uncertainties: At which altitudes do the maximum uncertainties usually appear? How do you treat measurement profiles for which the uncertainties appear at lower altitudes, e.g. 60 km? Do you have further constraints to insure the quality of your observations?

The measurement uncertainties increase with altitude, as the amount of received backscattered laser photons decrease with altitude. Hence, highest uncertainties appear generally at the highest altitudes. Profiles reach only as high as the measurement uncertainty is below the thresholds mentioned in Sect. 3. Raw signal profiles (5 min integration) which are obviously disturbed by poor signal quality (e.g., due to clouds) are discarded prior to the 1 h integration and subsequent temperature and wind retrieval. As only very few profiles were affected, we did not add this technical aspect in the revised manuscript.

We expanded the respective paragraph in the manuscript.

3. P. 5, ll. 12.–13: You state the “also” (why also? what else varies?) small vertical variability of the wind profiles and in the next sentence you state “very pronounced gravity wave structures”. Aren’t both statements contradictory?

We agree that the phrasing was misleading and clarified it.

4. P. 5, l. 35: “comparison of lidar data with ECMWF (...) for the whole data set”: since you compare two different ECMWF cycles to your observations it is misleading to average both cycles like done in Fig. 4d). In fact it seems to me that by averaging both cycles the deviations between the ECMWF and the observations decrease.

Since there is no Fig. 4(d) we assume the reviewer is referring to Fig. 3(d). We like to point out that Figs 3(a)–(c) and Figs 4(a) and (b) clearly show the results separated for the different model cycles. Since this might have gone undetected we have now added the information about the model cycles in the respective figures captions.

Also on p. 6, l. 19, I am not astonished that the comparison is nonuniform throughout the years, since you compare different cycles to your observations. This has to be evaluated in more detail and with more care!

We have carefully separated the data set according to different model cycles and now highlighted this information in the captions of Figs 3 and 4.

It is beyond the scope of this manuscript to investigate differences between ECMWF cycles and why ECMWF data might match differently to certain atmospheric conditions.

Also later in ll. 23–26, you should state the cycles used by the other studies.

Le Pichon et al. (2015) use ECMWF IFS cycles 38r1 and 38r2; see their Sect. 2.3 for details. *Rüfenacht et al.* (2014) use “ECMWF operational analysis data” of various cycles (*Rüfenacht et al.*, 2016): “36r2 (September to November 2010), 36r4 (November 2010 to May 2011), 37r2 (May to November 2011), 37r3 (November 2011 to June 2012), 38r1 (June 2012 to June 2013), 38r2 (June to November 2013) and 40r1 (November 2013 to February 2015)”.

We now note in the manuscript that other studies use different IFS cycles.

5. P. 7, l. 4: what is the RMS, I guess the authors mean “root mean square” but of what? Please clarify and also explain the abbreviation. Maybe also give a short explanation as to why an increase of the RMS is “expected for the effect of gravity waves”.

We now included in the manuscript the abbreviation (root mean square) and clarified that we mean the root mean square of the fluctuations as an indicator of gravity wave activity. We also added the explanation of the expected behaviour.

6. Figure 4b) is unnecessary and should be removed. The information on the deviation of the different profiles from one another is already contained in the profiles and the according standard deviations (shaded area) in Figure 4a).

We have considered removing this panel, but since the shape of the distribution cannot be inferred from Fig. 4(a) we decided to keep this panel.

7. In my eyes also Figure 5 is unnecessary, since the information on gravity wave activity is already contained in Figure 6 and the paragraph (p. 6, l. 30–p. 7, ll. 2) does not give substantial new information. Furthermore, the conclusions drawn in this paragraph again remain pure speculation.

This figure is the only example showing the actual 1 h profiles of lidar and ECMWF data. Furthermore, the discussions of Fig. 4(c) and Fig. 6 build on this figure.

8. A general comment regarding the Figures: most axis are rather small and difficult to read. E.g. values of the RMS profiles in Figure 5 cannot be inferred. Furthermore, all plots showing E_{pot} and E_{kin} on a log axis would definitely benefit from a larger aspect ratio so that concrete values can be inferred by the readers more easily. Furthermore, it should be avoided that plotted values are smaller than the axis values (1st panel, Fig. 3c; 3rd panel, Fig. 8a).

We increased the font size of the tick labels and axis labels. As the RMS profiles in Fig. 5 are intended to have quality character only, to qualitatively compare

fluctuations and measurement uncertainties, we see no need to enlarge this figure. Concerning clipped profiles in Fig. 3(c) and Fig. 8(a), we used the same axis scaling for the sake of comparison of various figures.

Technical corrections

1. P. 1, l. 4 and throughout the text: “month-mean” should read “monthly mean”, the same for “night-mean”.

done

2. P. 2, l. 8: “then” should read “than”

done

3. P. 2, l. 9: give the names for the models (ECMWF, HWM07) at the first appearance of the abbreviations in the text

done

4. P. 3, ll. 17–19: it might be of help for the reader to slightly change the order of the sentences: “To retrieve winds (...) The temperature retrieval relies (...) The two individually derived temperature profiles (...)” Also cite *Hauchecorne and Chanin* (1980) for the retrieval of your temperature profile.

done

5. P. 4, l. 11: the vertical resolution of the two ECMWF model cycles should be stated.

The altitude profiles of the ECMWF data already contained small ticks to mark the respective model levels; indicating that the vertical resolution decreases with altitude.

We now included in the manuscript that cycle Cy37r3 has 91 model levels and Cy40r1 has 137 model levels.

6. P. 4, l. 12: what is the vertical resolution of the lidar data? On p. 3, l. 27 you state that the lidar data is smoothed with a “window size of 3 km” is this the vertical resolution of the lidar data? Your profiles look way smoother than just one point every 3 km.

The internal range resolution of the lidar instrument is 50 m; the data were gridded to a raster of 150 m vertical resolution. These data were then smoothed with a running box filter with window size of 3 km.

We clarified this in Sect. 3.

7. P. 4, l. 32: “or even split, *and* warmer air”

done by using a semicolon instead of a comma

8. P. 5, l. 9: “Only *a* few days later”
[done](#)
9. P. 5, ll. 10 & 11: “some 20 K colder/warmer” – colloquial, state precise values
[done](#)
10. P. 5, ll. 11 & 12: “weak east/west/southward” should read “weakly east/west/southward”
[done](#)
11. P. 6, l. 16: “way too low” – colloquial, state precise values
[done](#)
12. P. 6, l. 20: “it is good below 60 km altitude”, please quantify. “Good” can mean anything.
[done](#)
13. P. 6, l. 26: “some deviations in the mesosphere”, please quantify.
[done](#)

References

- Baumgarten, G., J. Fiedler, J. Hildebrand, and F.-J. Lübken, Inertia gravity wave in the stratosphere and mesosphere observed by doppler wind and temperature lidar, *Geophys. Res. Lett.*, 42(24), 10,929–10,936, doi:10.1002/2015GL066991, 2015GL066991, 2015.
- Baumgarten, K., M. Gerding, and F.-J. Lübken, Seasonal variation of gravity wave parameters using different filter methods with daylight lidar measurements at mid-latitudes, *J. Geophys. Res.*, accepted, 2017.
- Dörnbrack, A., S. Gisinger, and B. Kaifler, On the interpretation of gravity wave measurements by ground-based lidars, *Atmosphere*, 8(3), doi:10.3390/atmos8030049, 2017.
- Dörnbrack, A., Interactive comment on “Winds and temperatures of the Arctic middle atmosphere during January measured by Doppler lidar” by Jens Hildebrand et al., doi:10.5194/acp-2017-167-SC1, 2017.
- Drob, D. P., J. T. Emmert, G. Crowley, J. M. Picone, G. G. Shepherd, W. Skinner, P. Hays, R. J. Niecejewski, M. Larsen, C.-Y. She, J. W. Meriwether, G. Hernandez, M. J. Jarvis, D. P. Sipler, C. A. Tepley, M. S. O’Brien, J. R. Bowman, Q. Wu, Y. Murayama, S. Kawamura, I. M. Reid, and R. A. Vincent, An empirical model of the Earth’s horizontal wind fields: HWM07, *J. Geophys. Res.*, 2008.

- Ehard, B., B. Kaifler, N. Kaifler, and M. Rapp, Evaluation of methods for gravity wave extraction from middle-atmospheric lidar temperature measurements, *Atmos. Meas. Tech.*, 8(11), 4645–4655, doi:10.5194/amt-8-4645-2015, 2015.
- Geller, M. A., and J. Gong, Gravity wave kinetic, potential, and vertical fluctuation energies as indicators of different frequency gravity waves, *J. Geophys. Res.*, 115(D11), n/a–n/a, doi:10.1029/2009JD012266, 2010.
- Hauchecorne, A., and M.-L. Chanin, Density and temperature profiles obtained by lidar between 35 and 70 km, *Geophys. Res. Lett.*, 7, 565–568, doi:10.1029/GL007i008p00565, 1980.
- Jablonowski, C., and D. L. Williamson, *The Pros and Cons of Diffusion, Filters and Fixers in Atmospheric General Circulation Models*, pp. 381–493, Springer Berlin Heidelberg, Berlin, Heidelberg, doi:10.1007/978-3-642-11640-7_13, 2011.
- Lane, T. P., M. J. Reeder, and F. M. Guest, Convectively generated gravity waves observed from radiosonde data taken during mctex, *QJRM*, 129(590), 1731–1740, doi:10.1256/qj.02.196, 2003.
- Le Pichon, A., J. D. Assink, P. Heinrich, E. Blanc, A. Charlton-Perez, C. F. Lee, P. Keckhut, A. Hauchecorne, R. Rüfenacht, N. Kämpfer, D. P. Drob, P. S. M. Smets, L. G. Evers, L. Ceranna, C. Pilger, O. Ross, and C. Claud, Comparison of co-located independent ground-based middle atmospheric wind and temperature measurements with numerical weather prediction models, *J. Geophys. Res.*, 120(16), 8318–8331, doi:10.1002/2015JD023273, 2015JD023273, 2015.
- Meriwether, J. W., and A. J. Gerrard, Mesosphere inversion layers and stratosphere temperature enhancements, *Rev. Geophys.*, 42(3), RG3003, doi:10.1029/2003RG000133, 2004.
- Mzé, N., A. Hauchecorne, P. Keckhut, and M. Thétis, Vertical distribution of gravity wave potential energy from long-term rayleigh lidar data at a northern middle-latitude site, *J. Geophys. Res.*, 119(21), 12,069–12,083, doi:10.1002/2014JD022035, 2014JD022035, 2014.
- Rauthe, M., M. Gerding, and F.-J. Lübken, Seasonal changes in gravity wave activity measured by Lidars at mid-latitudes, *Atmos. Chem. Phys.*, 8, 6775–6787, 2008.
- Rüfenacht, R., A. Murk, N. Kämpfer, P. Eriksson, and S. A. Buehler, Middle-atmospheric zonal and meridional wind profiles from polar, tropical and midlatitudes with the ground-based microwave doppler wind radiometer wira, *Atmos. Meas. Tech.*, 7(12), 4491–4505, doi:10.5194/amt-7-4491-2014, 2014.
- Rüfenacht, R., K. Hocke, and N. Kämpfer, First continuous ground-based observations of long period oscillations in the vertically resolved wind field of the stratosphere and mesosphere, *Atmos. Chem. Phys.*, 16(8), 4915–4925, doi:10.5194/acp-16-4915-2016, 2016.

Sato, K., A statistical study of the structure, saturation and sources of inertio-gravity waves in the lower stratosphere observed with the mu radar, *J. Atmos. Terr. Phys.*, 1994.

Zhao, J., X. Chu, C. Chen, X. Lu, W. Fong, Z. Yu, R. M. Jones, B. R. Roberts, and A. Dörnbrack, Lidar observations of stratospheric gravity waves from 2011 to 2015 at McMurdo (77.84° S, 166.69° E), Antarctica: Part I. Vertical wavelengths, periods, and frequency and vertical wavenumber spectra, *J. Geophys. Res.*, pp. n/a–n/a, doi: 10.1002/2016JD026368, 2016JD026368, 2017.