Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-537-RC1, 2017

Author's response on "Temporal variability of tidal and gravity waves during a record long 10 day continuous lidar sounding" by Kathrin Baumgarten et al.

Anonymous Referee #1

The paper by Kathrin Baumgarten and coauthors presents a continuous observation of middle atmosphere temperatures of unprecedented length. This is facilitated by newest advances in daylight capability as well as favorable observation conditions. The data are analyzed with a number of different techniques discussing the different effects of vertical and horizontal detrending. This is comprehensive and well presented. However, I do not agree with the interpretation of the findings in terms of a GW-induced change of the tidal amplitudes. This interpretation is based solely on the data without a clear discussion of the mechanisms. I would ask the authors to reconsider this part in the light of the major comments given below.

We thank the reviewer for the valuable and constructive comments. We revised the manuscript with regard to your comments. Detailed answers are given below. The line numbers for the changes refer to the manuscript with marked changes.

Major comments
The central figure in terms of interpretation is Figure 10. The figure presents the temporal variation of the tides, the GWs and the background winds. The tides show a general tendency of decrease over the 10-day period with some variation seen only in the temporally-filtered data. GWs show several peaks with the strongest peak after day 10, after the wind reversal of both zonal and meridional winds. That peak is more outstanding in the temporally filtered data, but amplitudes are much larger in the vertically filtered data.
The only point which supports your hypothesis is that the strongest decrease in tidal amplitude and the strongest peak of GW amplitudes appear both at the same day, i.e. day 11, in the temporally filtered data. However, this is a single event! It may be coincidence, the two variations could be caused by a common reason or it could indeed be causality. In order to argue that it is not a coincidence you would need more data. You claim that you can find a strong correlation: that could be tested by actually calculating correlation coefficients. (Proof me wrong, but I do not expect you to find a correlation significant based on Student's t-distribution.)
In order to argue for causality you would need to describe the mechanism in more detail. Here I see several difficulties.

We agree that this event is only a single event from which we are not able to derive all details of the potential mechanism. However, our measurement shows a unique case for the variability of different waves on time scales of days in the atmosphere. In the revised manuscript, we make clear that our explanation of the observation is a hypothesis. Additionally to this, we mentioned other possibilities now in the text (P17L9).

A) The (part-)global nature of tides. In general tides are understood to be a superposition of Eigenmodes of the atmosphere resonantly excited by solar heating (or chemical heating or latent heat release triggered by the solar cycle). In the superposition of different modes there may be locally varying amplitudes depending on the place of the observation. The relative phases then also would decide on the amplitude at a single observing station. In addition, non migrating tides may be excited by the interaction of tides and planetary waves (cf. e.g. Liebermann et al., JGR-Atmos., 2015). By the way, it is not correct that satellites do not observe short term variability, see this paper and also e.g. Pedatella et al., JGR-space, 2016.

If you want to understand the cause for the variation at Kuehlungsborn you have to first find out which tidal modes are involved and whether these modes change amplitude or rather phase. Then you would need to look for mechanisms on a part-global scale. The column above Kuehlungsborn is definitely insufficient.

Thank you for the comment. It is right tides in the atmosphere have a global nature. To completely understand their variation it would be necessary to use also data sets which cover them globally. But for a comparison to our ground-based measurements we need to measure the same tidal modes. These cannot be clearly identified from ground-based measurements. From our measurement we also calculated the mean phases of the tidal components over the three time intervals shown in Fig. 5 to investigate whether a possible change of tidal modes could have happened. The corresponding phase of the diurnal component shows no phase jump. Therefore, we assume there was no change in the tidal modes during the different time periods. This is not the case for the terdiurnal tidal signature. But the main focus in this paper is the variation of the diurnal tidal signature. Unfortunately, a ground-based measurement is limited in the observation of these tides, as it is, e.g., not sufficient to distinguish between migrating and non-migrating tides. This could give also potentially rise to a variation of the observed tides with the lidar. We mentioned these other possibilities in the manuscript now (P17L10).

The provided short-term variability of tides from satellite measurements by Lieberman et al., 2015 and Pedatella et al., 2016 are based on the calculations from Oberheide et al., 2002. This method has some limitations why we think this it is not suitable to access a day-to-day variability of the diurnal, semidiurnal and terdiurnal tides at the latitudes discussed here in our study.

The difficulties are:

- *applies only to low latitudes and non migrating diurnal tides (Oberheide et al., 2002)*
- *PW activity is not taken into account (but is relevant at latitudes higher than 50°N)*
- *the LST separation between ascending and descending nodes decreases for latitudes above 50°N (Oberheide et al., 2002)*

As a result our lidar observations present a unique case to illustrate the tidal variability on short time scales, which are not accessible by other remote sensing techniques so far. We added some of these information in the introduction (P2L22).

B) You would still have a stronger argument if you could show that the event considered is exceptionally strong. A single event may dominate the momentum flux of a whole latitude circle. You have climatologies from you own observations as well as from satellite data. Put your event into a context! That is a value of its own. In your discussion at the moment you focus on the temporally filtered data, because there you have only on peak while you have three in the vertically filtered data. However, GW momentum flux is proportional to the square of the temperature amplitude, which is a factor 9 (amplitude squared) larger in the vertically filtered data. Also the latter would be more susceptible to wind filtering and dissipation.

We have followed your recommendation and we have tried to put the event into a wider context. The diurnal tidal amplitude is stronger in May 2016 than the mean value of May from former years (P15L21). To compare the lidar measurement with further global tidal fields derived for instance from satellite measurements, we would need the same data retrieval and the same accuracy of data acquisition. But unfortunately, this is hard to achieve either because the altitude range covered is not the same or the temporal resolution is quite different.

C) Also GWs propagate obliquely. And what causes the peaks? The question what the origin of the peaks in GW amplitude might be is probably the most obvious one. Again you try to answer this solely from the column above Kuehlungsborn. The wind reversal points to some cyclone or anticyclone in the vicinity. Is there frontal activity? Please consider also weather maps and search for potential sources. Also high resolution ECMWF data may be helpful (you show quite remarkable agreement) to find potential sources.

It is absolutely right that an explanation solely from the column above Kühlungsborn is not possible. Our conclusion is more like a hypothesis or a speculation. Obviously also a change in the gravity wave source could have happened during the measurement period. As gravity waves can propagate hundred or thousand kilometers from their source region away, it needs further investigations to retrieve their origin using ray-tracing algorithms and additional model data. Referring to next comment of the referee also UWaDi could be helpful here, but UWaDi is only able to provide the absolute wave numbers without the sign and also 3D fields of wind and temperature from a model are needed. In our opinion this is out of the scope of this paper and needs to be done in a future study.

Overall, my recommendation is to strengthen the observational part by putting the observed event into a context: is it an unusually strong event? In addition, dig somewhat deeper into the sources of the GWs with the help of NWP data, potentially considering high resolution ECMWF data. Could UWaDi be helpful? Keep the ozone discussion short, local ozone does not tell you much. Then at the end you may suggest that the GWs could have an influence on the tides but do not build your whole paper around this hypothesis and do not quote this as an "elegant demonstration of the strong impact of gravity waves on the diurnal tide": you are missing the evidence!

We agree with your recommendation. Therefore, we have limited our interpretation and have mentioned also other possibilities. In the end, we cannot provide the evidence or the true explanation for our observation, but our study provides a unique case of the short-term variation of a diurnal oscillation, which could be related to a short periodic wave. The variation of the diurnal wave needs to be taken into account in the future when gravity wave parameters are retrieved from such ground-based measurements because there it is often assumed that the tidal background is constant over time (P17L9 and P18L16).

Specific comments:

P1L2 infected -> affected *Done*

P1L22 Separate your references for GWs and tides. There is also tidal excitation due to latent heat release, i.e. convection. *Done*

P2L1 therefore -> accordingly *Done*

P2L10 ground based = what remains on the ground = radar, lidar, airglow radiosonde, rocket: in situ *Done*

P2L35 data during the 10 days of continuous lidar data in May 2016 and *Done*

P3L6 flash-lamp pumped *Corrected to flashlamp pumped.*

P3L7 better signal-to-noise? In which way?

We are basically using Rayleigh scattering, which cross section is proportional to λ^{-4}. This means the scattering is much more effective for a wavelength of 532 nm compared to the fundamental wavelength of 1064 nm.

P3L27 below the initial retrieval altitude ? *This is right, we clarified the sentence ending.*

P4L9 over the entire period are shown in Figure 1 as well. *Done*

P4L12 only contain tidal waves*;* also gravity waves as *Following the suggestion from referee 2 we have changed the sentence.*

P5L1 calculated using as a <- one or the other *Done*

P5L2 Please include the 50km altitude also in the text *Done*

P5L17 Within this altitude range clear waves What do you mean? The altitude range 40-55km or the ranges below and above? I cannot follow your discussion here: In both cases I see waves, though at different frequencies. Maybe waves in the upper panel weaken at the highest altitude. Is that the effect of the vertical Butterworth filter? It also appears to me that the change between receiving channels also causes a noncontinuity in the waves - though that is hard to say as the line is in.

To clarify this we have added a statement in the manuscript with the altitude information (P5L14). The wave structures from the vertical filtered data are not as regular as from the temporally filtered data. This is not an effect of the vertical filtering. The vertical Butterworth filter could lead indeed to errors, but only for the last altitude bin what can be seen at 65 km. Another point you mentioned is a possible non-continuity in the structures because of a change of the receiving channels. The vertical filtering leads to a smoothing of this discontinuity, therefore it is unlikely that this is the reason for a change in the wave structures.

P6F3 vertically-filtered data
Please show also the low-pass filtered data in Fig.3

We have changed Fig. 3 accordingly. Now the temperature deviations induced by gravity waves and tides are shown. Also some text was added to describe Fig. 3 (P6L3).

P7F4 Wavelet spectra *Done*

P8L1 The unexpected decrease of the diurnal component shown above indicates a strong short-term variability for tidal components What precisely do you want to infer here? That the tides have a boost at 50km which not yet reached 60km? Then: what is the vertical group velocity of tides? Your period is quite long. Shouldn't the high amplitudes you see at 6 May reach a few days later also 60km? Or do you mean simply that there is a strong variation at 50km with a peak at 6 May? Please clarify.

We apologize for the misleading statement, the interesting point here was indeed the strong variation over time at 50 km. We clarified it now (P7L28).

P8L1 referred to be constant (e.g. from the satellite community). a) Also in the satellite community there are quite a number of approaches b) Often some temporal coherency is used to identify a number of different migrating and non-migrating modes. These, evaluated at a single location, may result in apparently strongly varying amplitudes, albeit a constant amplitude was assumed for all these global modes. c) That amplitudes are assumed to be constant is then usually just the lack of data. So far we unfortunately do not possess the perfect observations with simultaneously global coverage and good coverage of local time.
Please omit the half sentence here. You can include some discussion in the introduction or summary section.

We followed your suggestion and omit the half sentence. Additionally, we have included some information in the introduction regarding the satellite measurement (P2L22).

P8L15 That you need a closer look tells you that the differences cannot be huge ... Are you even sure that they are significant? Probably yes, as they seem to be consistent over a wider altitude range. However, that they match at 43km does not seem to be significant to me given the variations in the profile above.

The differences are significant as the first interval show clearly larger amplitudes for the diurnal variation which is indeed consistent over a wider altitude range. We changed the beginning of the sentence as it was misleading before (P9L4).

P8L22 50 % : Sorry, I am bewildered. In Fig. 5 left panel, blue curves you have at 44km a mean amplitude of roughly 1.5K, and a minimum (dashed) of 1K = 30%. Just above at 46km the three curves are inside .2K at 2K mean amplitude, i.e. about 5% variation around the mean. For a global mode with long vertical wavelength such as a tide I would not assume the minor zigzagging to be real but indicative of the precision with which you can determine your amplitudes. On the other hand, the wavelet analysis indeed shows a change of a factor 2 in amplitude, so I am missing that special part of consistency. My interpretation would be that tidal amplitudes are stable, if periods of 7 days and larger are considered but that on shorter scales the local tidal amplitudes vary strongly.

The minimum value of the diurnal tidal amplitude from the second period is generally between 30 and 50 % lower than the maximum value from the first period in the altitude range of 43 – 58 km (except of the values at 46 km and 55 km). We rephrased the sentence to make clear what values are compared with each other (P9L12).

P8L26 which could be Doppler shifted to *intrinsic* periods larger than the Coriolis Period

We have clarified in the manuscript which period we are referring to (P10L1).

P8L27 In the composites you implicitly assume a constant phase of the tide over the analysis interval. Phase variations hence would also be a reason for different results.

You are right. The basic assumption in the composite analysis is a constant tidal phase during the single days used for the composite. Furthermore we assume that the phases of GW are more randomly distributed. Due to the overlaying of the data according to the local solar time, GW are *averaged out and the remaining waves are probably tidal signatures. We added such a statement in the manuscript (P10L2).*

P9L17 we assume that the disappearance -> we want to investigate whether ????

We rephrased the sentence.

P10L2 depends on wind conditions as well as on their interaction. Please be more precise, e.g. the propagation conditions of tides depend on the mean background winds and the propagation of GWs both on the mean wind and the tides.

We changed the sentence to be more precise (P11L2).

P10L6 ECMWF is able to reproduce the meteorological situation above Kuehlungsborn. *Done*

P11L8 the sponge layer and the fact that there are basically no data above the stratopause assimilated.

Aside from the sponge layer, that there is no data assimilation above the stratopause is also a reason why the data reliability becomes worse at larger altitudes. Now, this is also mentioned in the text (P11L13).

P12L5 What do you mean: that the variation, albeit weak, is caused by PW or that the weakness of the perturbation is caused by PW

Sorry for the misunderstanding, we meant the general variation is presumably caused by planetary waves. We clarified this in the text (P13L7).

Fig10: Please assign panel indices (a,b,...). There seems to be a data gap after day 10 in the observations. There are some odd blue lines at the bottom of the plots in the middle row.

We removed the odd blue lines in the plots and added some panel indices for readability. There are also two data gaps in the data (duration ~5 h and ~1 h). But both gaps are small enough to interpolate them easily and therefore they have no effect on the results (P4L18).