

Interactive comment on “Temporal variability of tidal and gravity waves during a record long 10 day continuous lidar sounding” by Kathrin Baumgarten et al.

Anonymous Referee #2

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This paper presents a study of gravity wave and tidal activities in the stratosphere and lower mesosphere using lidar temperature measurements that span continuously over 14 days. Such continuous measurement is unprecedented and is extremely valuable for the study of wave propagation through this region and interaction of waves at different time scales. This work is fairly thorough, and the key points are well described and supported by analyses from various angles. Because of this, I think this work should be published. I do find some various places where the manuscript can be improved and have given my suggestions below.

Aside from that, it's important to note that tides are global features while GWs are

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highly localized. The decrease of diurnal oscillation seen in this measurement does not necessarily mean a decrease of the global diurnal tide amplitude. Such decrease could be due to interactions among different tidal modes or with planetary waves. Nevertheless, I think the analysis in this work does make a case that it is possible the local decrease of the diurnal oscillation is related to a GW. The authors do need to carefully distinguish between diurnal tides (global) and diurnal oscillation (local) in the text.

page 2, line 25: This is perhaps not a fair comparison. A lidar's capability is not measured by the altitude range it can measure (unless they all measure the same thing). The Rayleigh and metal lidars measure different regions and serve different purposes. If the lidar data used in this study included the mesopause region, then this statement would be appropriate.

page 3, line 26: It is not clear how 'strong solar background' is related to the starting altitude. Does it make it lower or higher? Please explain.

page 3, line 31-32: please specify the cut-off periods/vertical wavelength of these filters.

page 4, line 12: suggest changing to '... data set contains not only ..., but also ...'

page 5, line 1: remove 'using'

page 5, Figure 2: Since the data is averaged for 2 hrs (stated on page 3, line 28), the highest frequency that can be resolved is $1/(2\text{hr})$. Even though the data point is every 15 min, the figure better not extends to higher frequency because there is really no information beyond $1/(2\text{ hr})$. Caption: not sure what the 'first' and 'second' half mean and how they relate to the error bars. Need clarification. It's not clear what data was used to calculate this spectrum. Is it from 50 km temperature only or average over an altitude range around 50 km? How is the PSD 'smoothed'? The

page 5, line 9: it'd be good to specify the order of the Butterworth filter used.

page 5, line 16: 'more perturbed' means larger amplitude?

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page 5, line 18: why this is due to propagating conditions, not the source?

page 7, line 2-5: It's not clear which panels in figures this sentence refers to. The wave activity difference is obvious at 50 km, but not at 60 km. The sentence needs to be more specific for the readers to make the comparison. Also, why using 6-14 hr for the vertically-filtered then using 8-24 hr for the temporally filtered? Use the same time range (e.g. 8-14 hr) for comparison makes more sense.

page 7, line 8: 'increased' to 'largest'

page 7, line 13-page 8 line 1: I think the short-term tidal variability is well recognized in the science community. It is not 'unexpected.' Even using satellite data, researchers have been trying to extract short-term variabilities, such as doi:10.1002/2016JA022528. page 8, line 10: The 'monthly mean' probably means 'average over the 14 days'?

page 8, line 10: 'relevant' to 'noticeable'

page 8, line 11: 'at an altitude of 50 km' to 'from 30 to 50 km'

page 8, line 23: besides -> aside from

page 8: line 26-27: This statement implies that the composite analysis does not include oscillations from GWs Doppler shifted into the tidal periods. I don't see how this can be the case. The wavelet method and the tidal fitting are no different. Neither can separate out the Doppler shifted GWs from tides.

page 8: line 5: 'that for' to 'from'

page 8, line 15-16: Where is the 'strong diurnal component' in Figure 6? I don't see any.

page 8, line 11-20: Similar to the discussion about Figure 4, the text often does not refer to specific panels in the figure, which makes it hard to understand what features the authors are pointing to.

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page 10, line 11-12: Is this also due to sponge layer? If so it's better to state the sponge layer here than later on page 11.

page 12, last paragraph: While I agree with authors that ozone is perhaps not the main cause of the tidal variability during the 14 days, the argument here is not accurate. Because tides are global structure, they are forced and therefore related to the global structure of the ozone layer. Planetary scale perturbations of the ozone could cause tidal variability, but it may not show up as a correlation between local ozone concentration and local tidal amplitudes. Lack of correlation at a single site does not support the argument that they are not related because it could be a weaker ozone at other longitudes that causes the weaker global tides.

Figure 11: The amplitude of temperature perturbation is not a complete representation of GW energy. The potential energy, which is related to N^2 is more appropriate. Because of the quick change in the temperature gradient from the stratosphere to the mesosphere around 50 km, the GW potential energy variation may be quite different from temperature perturbation amplitude.

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