

Interactive comment on “Case study of wave breaking with high-resolution turbulence measurements with LITOS and WRF simulations” by Andreas Schneider et al.

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Author response to Anonymous Referee #2

Major comments

1. *Regarding the WRF simulations we are not given any evidence the simulations are correctly modeling the atmospheric environment. At a minimum, comparison of wind and temperature profiles at the location of the balloon ascents to the LITOS profiles should be done. And there should be plenty of surface data to compare to as well. Also, what about comparisons to satellite imagery: is there*

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any evidence of waves in the images? If so what are the wavelengths and do they agree with the WRF predicted wavelengths?

We agree that a validation of our WRF simulations was missing in the manuscript. We have plotted WRF data for winds and temperatures along the LITOS ascents, which shows that WRF captures the atmospheric structures well. We have also cited the paper Ehard et al. 2016, which shows a combination of WRF simulations with lidar and radiosonde data over northern Scandinavia with nearly the same model set-up as in our paper and demonstrates the ability of WRF to properly simulate GW events. Our interpretation of the WRF data is not based on specific wave parameters. For instance, we do not extract wavelengths from WRF and the exact wavelengths are not important for our reasoning. Thus, a detailed comparison of gravity wave parameters in WRF with observations is not necessary and outside the scope of this paper.

2. *In a similar vein, while I agree that 2 km resolution is probably sufficient to resolve most gravity waves that may be generated either topographically or from other sources, it is not sufficient to model “wave breaking”. This would require much higher resolutions. See e.g., Kim et al. MWR 2014 and Trier and Sharman, MWR 2016 for examples of the effects of model grid spacing on gravity wave resolutions.*

We agree that our WRF simulations cannot simulate GW breaking of small-scale GWs with horizontal wavelengths smaller than about 10 km. Wave breaking can, however, also occur for larger-scale GWs, which are explicitly resolved by the model. Ehard et al. 2016 show regions of wave breaking at altitudes between 25 km to 30 km by means of convective overturning and reduced Richardson numbers, which was simulated by WRF with grid distances of 2 km. In our paper we use the TKE output from the model, which is provided by the boundary layer scheme and shows regions of intensified turbulent mixing in the atmosphere. For some flights (e.g., BEXUS 12, Fig. 2) these regions agree well with regions of

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increased dissipation rates from LITOS. Apart from that, we do not discuss turbulence that is not resolved in WRF, but observed by LITOS, since of course WRF cannot resolve all details that LITOS can measure. We added some sentences and citations about this issue in Section 2.2. Moreover, we have added a sentence at the end of Section 3.1 stating that we intentionally do not examine turbulence unresolved in WRF.

3. *Another approach might be to attempt to diagnose regions of gravity wave breaking from the LITOS or model derived soundings using standard gravity wave drag parameterizations, described e.g., in Nappo's 2002 book, and used in Kim and Chun JAMC 2011. Also looking for the presence of gravity wave critical levels in the WRF output may be useful in diagnosing regions of likely wave breaking.*

Kim and Chun 2011 diagnosed turbulence sources for a large dataset by looking at lightning data for convective generation, reanalysis data for shear-induced turbulence from jet streams, and a digital elevation model for mountain waves. While this probably works for statistical statements as done by Kim and Chun 2011, we think it will not work for individual cases.

4. *Looking at the LITOS figures I really don't see a good correlation between epsilon and low values of Ri. This is not unexpected (e.g., Galperin et al. ASL 2007), and implies it is difficult if not impossible to assign a threshold Ri for turbulence. The authors discuss this in Section 2.1, but it should be also emphasized in the conclusions section.*

We have added a respective paragraph in the conclusions:

"Turbulence has been observed for Richardson numbers below as well as above the critical number of 1/4, partly even for values larger than 100. Such a violation of the classical theory by Miles (1961) and Howard (1961) has already been described by several researchers, e.g. Achatz (2005); Galperin (2007); Haack (2014). Hines (1988) recognised the limitation of considering only vertical insta-

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bility (as done when using the Richardson number) and proposed a concept of slantwise instabilities as created by gravity waves. He showed that turbulence is more likely to develop via slanted instability. Thus turbulence for $Ri > 1/4$ is comprehensible."

Minor comments

1. *p. 2 line 27. Do you mean a precision of 1 cm s⁻¹?*
We mean a precision of a few cm s⁻¹. The sentence was changed accordingly.
2. *p. 3 line 6. Do you mean "sensors" instead of "sectors"?*
Yes, changed.
3. *p. 3 lines 10-13. While I understand the attempt to use the Heisenberg spectrum to fit the high frequency end of the measurements, wouldn't it be simpler and less error prone to simply fit the portion of the spectrum in the inertial range to determine epsilon?*
Deriving ε from fitting the inertial range of the spectrum is not possible for our measurement. For this method, the dissipation rate crucially depends on the absolute value of the periodogram, which is unknown due to missing calibration. A calibration to infer wind velocities from the anemometer voltage would be difficult because it has to be performed in a laboratory for known velocities under the same ambient conditions for pressure and temperature as the measurement. Conditions of a balloon flight, where pressure varies within several orders of magnitude and temperature changes by ~ 80 K, are very difficult to simulate in a wind tunnel. We do not know a facility where such a calibration would be possible.
4. *p. 3 line 20. How can epsilon computed from eqn (10) ever be negative when the individual terms are raised to the 4th power and are therefore even, and nu should always be positive?*

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We thank the reviewer for pointing at this issue. The condition stems from an earlier version of our retrieval a few years back when ε was fitted instead of l_0 , which numerically allowed negative values of ε to be returned by the fitting procedure (equation (1) had been incorporated in the fit function). With the changed fit parameter this is no more possible. Thus the condition can now safely be removed. In the manuscript, this item has been deleted.

5. *p. 11 line 22. Could you elaborate on what is meant by “continuous fractional wave breaking”?*

We have changed the term to “wave saturation”.

6. *In the LITOS figures (1,3,5,7), what is heating rate on the left panel? It would be interesting to plot shear and stability as well, and this may help in assessing the character of the turbulence.*

On the right panel, the top axis gives the heating rate due to turbulent dissipation, $dT/dt = \varepsilon/c_p$. A sentence was added in the figure caption to explain that:

“The top axis gives the heating rate due to turbulent dissipation, $dT/dt = \varepsilon/c_p$.”

Since the Richardson number does not correspond well to turbulence (cf. major comment 4), splitting Ri in wind shear and buoyancy frequency seems not to provide useful information.

7. *Appendix. The gamma function in the eqns is not defined.*

A phrase defining it was added:

“where [...] $\Gamma(z) := \int_0^\infty t^{z-1} e^{-t} dt$ is the Gamma function, ...”

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