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Atmospheric Chemistry and Physics
Discussions

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

Anonymous Referee #2

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This paper describes comparisons of four high-resolution turbulence measurements from LITOS-equipped balloons with WRF simulations, with the goal of better understanding the sources of turbulence observed in the LITOS ascents. The LITOS-derived energy dissipation rates appear to be carefully done, however the WRF simulations, while suggestive, need more attention. I would therefore recommend publication in ACP subject to major revisions that include more thorough analyses of the WRF output.

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

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.

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.

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.

Minor comments

1. p. 2 line 27. Do you mean a precision of 1 cm s-1?

2. p. 3 line 6. Do you mean "sensors" instead of "sectors"?

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?

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 by positive?

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

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.

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

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