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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Turbulent structure and scaling of the inertial subrange in a stratocumulus-topped boundary layer observed by a Doppler lidar" *by* J. Tonttila et al.

Anonymous Referee #1

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This paper describes an analysis of surface-based remote sensing observations of a stratus boundary layer over few days. The observations are taken at a coastal site (Mace Head, Ire). The analysis features vertical motions from a Doppler lidar plus some ancillary data from a cloud radar (principally used to define cloud top). Conditions vary diurnally and synoptically over the observation period. The authors concentrate on profiles of properties of the vertical velocity variance sprectrum including the variance, skewness, spectral peak, and rate of dissipation of turbulent kinetic energy (TKE). The time series of these variables are interpreted in terms of a coupled vs de-coupled boundary layer.

In general the paper is reasonably well-written. The background and methodology sections are fairly straightforward and comprehensible. These sections are mercifully brief but provide enough information to make the paper self-contained. The discussion of meteorological conditions is lengthy and is almost a blow-by-blow description of the events of the entire period. To some extent this is needed to set the synoptic context of the changes that are observed. These are confusion factors in attempting to relate turbulence dynamical effects to local BL structure, etc. I confess I found this a bit hard-going, mostly because of the poor quality of Fig. 3-5, which are real eye-strainers. In section 4 the authors strive manfully to relate the estimated turbulent variables to important changes in BL structure and forcing. Decoupling is a key aspect. It is apparent that this is a messy business and just two days of data are not going to bring any clean insights.

In my opinion the paper represents a usable description of an amusing data set. The major weakness is that the authors provide little guidance on how their data are unique and if they have found new insights. Assuming they can do a little more homework on this, I recommend publication with minor revisions. I also have a few editorial comments for the author to consider.

*I suggest the authors make it painfully clear that their turbulence observations are essentially sub-cloud only. See Ghate et al. (JAMC, vol 53, p117-135) for an example of combined lidar and radar turbulence observations.

*The parameter Lambda0 is actually the wavelength associated with the wavenumber peak of k*S(k). I don't know why you would refer to it as a 'cut-off wavelength'.

*Suggest given a value for a (eq. 4). Did you state what value you used for mu?

*As an alternative to eq. 6, you can use the value of kˆ(5/3)*S(k) for wavelengths smaller than Lanbda0 to compute epsilon. It would be interested to see how those values compared.

*One well-used index of decoupling is the difference in the lifting condensation level (LCL) and the observed cloud base height. Is it possible to provide that?

*Speaking for myself, I don't think Fig. 2 adds much to the paper and could be eliminated.

*I found the cloud mask shown in Fig. 6 to be useful, suggest it be added to Figs. 4-5. Also suggest enlarging Fig. 5 to make it easier to see.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 24119, 2014.