Journal: Atmospheric Chemistry and Physics
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Title: Observed spatial variability of boundary-layer turbulence over at, heterogeneous terrain
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General comments

This study examines the spatial heterogeneity of vertical velocity variance profiles as observed by multiple Doppler lidars. The lidars are situated at the vertices of a triangle with roughly 3km separation between each vertex, and the study area includes substantial small-scale variability in land use. The authors show that traditional local scaling techniques (i.e. using the convective velocity scale from a collocated surface station) is not effective at collapsing the profiles and that knowledge of the wind direction and the up wind fetch is important. One of the main conclusions is that multiple lidar measurements are needed in order to adequately capture the spatial variability of vertical velocity variance profiles in the convective boundary layer. Considering the widespread use of these local scaling methods, I believe the paper makes an important cautionary point.

The manuscript is well-written, well organized, and the authors are particularly thorough in their error analysis. I believe the paper should be published with a few fairly minor revisions and/or clarifications. My main points are listed below.

Specific comments

Abstract

The author states that "…differences between variances at different sites were about three times higher than between those derived from measurements by different lidars at the same site." This statement doesn't make much sense to me. Given the three measurement sites, how could you possibly have differences between all three sites (1-2, 2-3, 1-3) equal to 3x the difference at one site?

page 18016, lines 13-16

Its not clear from this discussion how the WLS7 was operated. The author mentions the VAD mode and gives the temporal and spatial resolution for that mode, but then only briefly states that the system was operated in a vertical stare mode. Why was the VAD mode mentioned? Are the winds from the VAD mode being used in this study? Please clarify.

Table 1

This table lists the various lidar systems and some specs. It would be useful to also include the pulse repetition frequency, the pulse integration time and the duty cycle for the vertical staring data.

page 18021 lines 6-9

The author states "…the spectra of WLS7 show some artefacts at the highest frequencies... This is presumably the signature of an aliasing effect." The WLS7 spectra as shown in Figure 4 do indeed show some peculiar behavior, and I believe the author should elaborate on the above statement. Why is this occurring? What is the radial velocity sampling period? Is this significantly different than the averaging time? This is why I asked about the duty cycle above.

Pages 18023 and 18024
The author discusses three different methods for estimating boundary layer height, with method 3 being based on the velocity variance profiles from the lidars. The author points out that the sonde and backscatter method are merely proxies for method 3 (and I would tend to agree). But then at the top of page 18024 they essentially dismiss this method and say "methods (1) and (2) showed good agreement." This implies to me that method (3) did not show good agreement. I believe the authors should show the results of method 3 in Figure 5, and provide a more thorough discussion of the differences.

Page 18023 lines 23,24
The author states "…can presumably be attributed to the existence of gravity waves…"
Since the authors offer no firm evidence, it is best not to "presume." It would be better to say something like "may be caused by gravity waves in the capping inversion layer."

Page 18036 line 26
The author states that "Different methods to derive zi agreed well." This conflicts with the statement on page 18024 (see comment above).

Page 18039 line 23
The author should be more explicit about the type error they are referring to. In this case it is the variance of the noise that is being referred to. The author might say "…the noise variance is equal to the difference…"

Page 18040 line 11
The author should be more explicit here and say "…it can be seen that the **systematic** error…"

Page 18040 line 16-17
The author should be more explicit here and say "…the **random** error can be approximated **as**…"