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Comment

## ***Interactive comment on “Boundary layer dynamics over London, UK, as observed using Doppler lidar” by J. F. Barlow et al.***

### **Anonymous Referee #2**

Received and published: 22 September 2010

This manuscript presents an interesting data set and analysis. Much of the work is original, timely and of value to the urban climate and air pollution community and there is a need to publish work of this nature. The analysis could be more thorough, however, and the many assumptions made to produce the results weaken the conclusions. The manuscript starts off with a comparison of 2 methods to measure vertical velocity variance (remote vs in-situ) in the urban boundary layer. Results differ but are made to agree better with each other by considering sampling limitations in one of the sensors. A second topic looks at the height of the boundary layer based on remote measurements and discusses and compares commonly used indices with each other. Finally, the vertical profile of scaled vertical velocity variance is analyzed for different atmospheric stabilities. The three topics are joined together in a coherent way, but could equally well be treated separately but with more depth and relying less on assump-

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tions.

Major comments:

(1) Additional experimental details are needed. (i) What is the diameter of the BT tower at the top and how does that measure compare to the 12 m height of the lattice tower, i.e. can tower shadow (flow distortion) effects on the turbulence measurements be excluded? (ii) Majority of wind directions is from the west; can it therefore be assumed that the turbulence wind sensors are also facing in this direction? How about possible sensor/tower shadowing effects for other wind directions? (iii) How was the sensor source area (1-10 km) determined? Are the lidar and tower sites close enough to each other to have similar surface characteristics within the respective source areas? Also, please indicate scale on Fig. 1.

(2) To what extent is it possible to directly compare 20 Hz sonic standard deviations averaged over 30 minutes with 0.25 Hz lidar data representing 4 sec (or 30 m) averages with each other? Further, the derivation of the lidar statistics is known to be sensitivity to averaging parameters such as the size of the range gate. A more detailed discussion is necessary to make a more convincing argument that the observations from the two different sensors should indeed be similar. Reducing the sonic sampling rate to 0.25 Hz will exclude small eddies from the statistics because of the way (Reynolds decomposition) they are calculated, but this does not necessarily make them compatible with 0.25 Hz lidar data?

(3) Discussion of Fig. 3 (p. 19911): “Reasonably good linear relationship” is a little bit of a stretch. The lidar values do not change much for small values up to a sonic value of about 0.25 m/s and an exponential (or polynomial) curve might be a better fit for the entire data range. What is the reason to choose 2/3 of the sonic value as a “critical” threshold (seems arbitrary)? The small values are thought to arise from an under-sampling by the lidar during stable nighttime conditions. It is not clear if stratification was indeed stable or if this is just a hypothesis. Given the availability of a

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sonic anemometer it is possible to measure stability and this data should be presented. Otherwise this conclusion (also further down in the same paragraph) lacks credibility. Spectral analysis would also help to ascertain to what extent high-frequency loss could result in the observed underestimation (assuming that the sonic provides the correct result).

Averaged (to 0.25 Hz) sonic data agree better with the lidar data. However, it is not clear how the suggested adjustment results in the “sonic 0.25 Hz” (i.e. red) data points in this figure. Averaging will reduce the value (i.e. bring it closer to the lidar value), i.e. the original sonic values should be smaller, however, they seem to become larger. The same is true for the lidar values which, however, should remain unchanged?

(4) Discussion of Fig. 4 (p. 19912): I do not understand how a stable boundary layer can form near the surface. Radiative cooling is reduced in the urban case and if anything the trapped heat will be released into the lower atmosphere to support unstable stratification. Roughness produced mechanical mixing would also act to destroy stable temperature gradients. What evidence is there to support the hypothesis of a stable boundary layer forming at the ground (which is usually not observed in cities)? According to the variance data a convective mixing layer exists (presumably within which the aerosols accumulate?).

(5) Discussion of Fig. 5 (p. 19914): What is the physical explanation for the delay observed in the heights of the aerosol BL compared to the mixing height BL? How is it possible to have aerosols mix above the mixing layer height in the late afternoon? Is this effect real or an instrumental artifact?

(6) Discussion of Fig. 6: (p. 19915, 19916): This is important work but a lot of assumptions are used in creating these plots. E.g. in the absence of actual measurements, surface fluxes have been calculated to produce realistic mixing and mixing heights have been re-defined to reduce scatter and make profiles collapse. Any subsequent interpretation and conclusion invariably will be weakened by such assumptions and

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tweaking, preventing sound scientific inquiry. Also, Fig. 3 shows that the lidar underestimates in-situ observations from the EC approach, in particular for small values, probably due to sampling limitations, i.e. the plotted data are probably too small even when normalized. A more thorough discussion of these issues is needed to convince the reader that the presented results have general applicability and are representative of the urban BL structure despite the experimental limitations.

Minor comments:

Title is too generic given the limited data set (2 days), which makes more of a preliminary study than a thorough investigation of the UBL dynamics. P. 19903, l. 24: Is “physico-chemical processes” a commonly used term? P. 19904, l. 17: Vesala et al (2008) is not listed in references. P. 19908, l. 20: Do you mean “surface temperature” (i.e. radiant temperature) or “near surface air temperature”? P. 19909, l. 3: What cloud cut-off was used to discriminate between clear and cloudy conditions? P. 19911, l. 3: “overnight” = nighttime? P. 19912, l. 15: Should be horizontal “dashed” line; it is very difficult to make out unless the figure is magnified considerably on the computer. P. 19915, l. 11: Is the zero-plane displacement length value characteristic of the lidar source area? References: Remove Dall’Osto et al (2010) which is a manuscript still in preparation. Figures: Please use standard notation in figures such as mean wind speed (not “meanspeed”), T (not “temp”), etc. Fig. 6: Labels and text are difficult to read.

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