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Atmospheric
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Discussions

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

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1) "Additional experimental details. . ." Paragraph 2 of section 2.1 has been modified to include some of the details required – please note that the Wood et al. 2010 reference contains much more detail about the Tower, quality assurance and site description, and is highlighted at the start of the paragraph.

i) The diameter of the BT Tower has now been added to the text (14.8 m). Recent wind tunnel work to simulate flow at model scales 1:160 and 1:40 around the top of the BT Tower has shown that there is a slight deflection due to the lattice, rather than the Tower itself, and a small influence from the turbulent wake of the lattice at the height of measurement. This work is currently being prepared for a full paper for Journal of Wind Engineering and Industrial Aerodynamics, as a Special Issue from the following conference: Barlow, J.F., Wood, C.R., Harrison, J. and Robins, A.: Urban wind observations made at the BT Tower, London, Proceedings of the 9th UK Conference on Wind Engineering, 20-22nd September 2010, Bristol, UK.

ii) sensors are on the south west corner of the lattice tower, now included in text. We found that there appear to be no biases in the scaled turbulence statistics as a function of wind direction (not shown in paper), which were reported in full in Wood et al. (2010), i.e. any flow distortion errors seem to be within the scatter of the data.

iii) the flux source area was determined using the Schmid (1997) model, as described in Wood et al. (2010), and the range of 1 to 10km corresponds to stable to convective conditions respectively according to the z-d/L value at the BT Tower. To first order, the lidar measures throughout the depth of the boundary layer, so given vertical mixing timescale of z_i/w*, the horizontal advection in that time is Uz_i/w*. Given average daytime mixing height of 500m, an average campaign windspeed of 8 m/s, and average w* of approx. 0.7 m/s, the estimated daytime upstream source area for the lidar is c. 5km. As this is within the same range as for the sonic anemometer, the source areas for the two instruments seem to be similar sizes. The two parks, Regents Park and Hyde Park, represent the major land-use change within the source area, being c. 1 – 2 km2 in size, the rest of the surface being dense urban (see Fig. 1 in Wood et al 2010). The lidar does lie closer to the Regent’s Park, but does not measure below 90m, therefore may not “see” the local impact of the Park. By these estimations, I think it is unlikely that the lidar and BT Tower fluxes see substantially different surfaces affecting the measurements, and the scale of the source areas is comparable, allowing the lidar measurements to be scaled using the Tower fluxes.

A scale has been added to Figure 1.

2) “To what extent…” NB: The sampling rate of the lidar has been more accurately specified in the revised manuscript: it effectively samples every 3.6s, giving a sampling rate of 0.278 Hz. This has been corrected throughout, and Figure 3 has been

regenerated using averages of 72 sonic anemometer samples rather than 80. It is true that degrading the sonic measurements temporally does not take into account spatial averaging due to the lidar gate. A "spatial filter" cannot be applied to a single point measurement (cf. Inagaki and Kanda 2010 averaging several point measurements) to take this into account. But the reduction in variance occurs in the right part of the spectrum – ie high frequencies – and therefore seems the next best thing. Applying no averaging at all, as shown in Fig 3 comparing 20 Hz with 0.25 Hz data, is indeed comparing "apples and pears". Detailed assessment of this problem requires use of e.g. CFD data, which is outwith the scope of this paper. A sentence has been added to section 3.1 to summarise this issue.

3) "Discussion of Fig. 3" The text has been changed to give a more neutral description of the relationship. Given the scatter, we would argue there's not much evidence to support use of an exponential model as opposed to a linear relationship. 2/3 is indeed an arbitrary threshold for the ratio but its use is in comparing the percentage of points lying below that value for 20Hz with 0.25Hz sampled sonic data. The text has been modified to state that it is arbitrary. In terms of stability, Figure 7b shows the range of stability observed in the REPARTEE dataset as determined using the BT Tower fluxes: it can be seen that there are quite a few weakly stable conditions (e.g. $0 < z'/L < 1$) , and a few occasions where it was more strongly stratified and we would expect turbulence to be suppressed (e.g. $z'/L > 1$). Given that Hogan et al. 2009 have already demonstrated that stable conditions can lead to undersampling of variance determined by Doppler lidar, we feel that the discussion as it stands is credible.

The data pairs used in Figure 3 were not identical for the 20Hz data and 0.25 Hz data: this has also been rectified in the revised manuscript. This is why the averaging produced a surprising change in the figure, as pointed out. We thank the reviewer for spotting this!

4) "Discussion of Fig. 4" A sentence has been added to quote the Monin-Obukhov stability parameter values determined for the 00:00 to 07:00 period on 6th November, as

evidence that stable conditions (based on this locally determined parameter) existed. The fact that the lidar observes a turbulent layer is commensurate with the smaller values of z-zd/L (less than 1), so the negative buoyancy is not enough to suppress the turbulence. Later in the night the values are ~1.5, and the lidar turbulent layer has decreased. True, in an urban area the surface sensible heat flux can be positive some time after sunset: but this data is taken several hours after sunset the previous day (c. 16:30), so it is unlikely to be unstable, more likely neutral or stable. Another paper within this special issue specifically discusses a fog event in London during the REPARTEE experiment, which demonstrates that stable layers can form at the ground in this city.

5) “Discussion of fig. 5” Please see answer to question 1 of Referee #1 with respect to the delay in growth of turbulence and aerosol layers in the morning. The observation that the backscatter gradient is above the turbulent layer in the afternoon may be similarly due to an artefact in interpreting backscatter profiles, in that humidity may be causing certain aerosol particles to grow when the temperature is decreasing, thus intensifying the backscatter. Also, if the turbulence decays more quickly than the time it takes to transport the particles back down to the ground, then layers of aerosol may “hang” in the air aloft, subject to slower, diffusive type dispersion. Investigating the relationship between backscatter and aerosol properties is the topic of future work, which has been stimulated by this interesting result.

6) “Discussion of Fig. 6” We agree that assumptions have been used in this scaling work. But we believe that the assumptions are reasonable: in the case of extrapolating fluxes to the ground, Wood et al. (2010) clearly showed that using the local value of the heat flux instead of an estimated surface value produced physically unrealistic profiles of variance, when scaled. Thus it is better (but not ideal) to take this known discrepancy into account. Ideally, it would be better to use actual surface heat flux measured at the ground, but this is well known to be a difficult thing to measure in urban areas given the heterogeneity and wasn’t available in the present study. In terms of the correction of

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the mixing height, it is clear that it artificially constrains the vertical extent of the profile to be similar to the existing expressions. But it can be seen in comparing 6a and 6b that it makes little difference to the scatter of the results, ie the error in the mixing height does not change w* enough to produce a better collapse, so there is another source of uncertainty or process causing the wide spread. This logic is explained in the third paragraph of section 4.3, and the process is presented as an exploration of possible errors in the scaling variables. We feel that the existing text does clearly highlight the assumptions, their shortcomings, and that it is not ideal to have to resort to them. But we have modified the fourth paragraph in the conclusions to bring these caveats out more strongly there.

Minor comments: “Title is too generic” Response: The title has been modified to specify that it is a campaign-based investigation, rather than longer-term data-set.

P19903L24 “physico-chemical process”? Response: the term is an accepted one, e.g. http://www.eionet.europa.eu/gemet/concept?cp=6233

P19904, L17 Response: The Vesala et al 2008 reference has been added

P19908, L20 Response: The sentence has been altered slightly, but at the start it refers to dry bulb temperature measured at the tower and it is referring to near surface dry bulb temperature allowing for adiabatic lapse rate with height.

P19909, L3 The method for determining “clear sky” periods is described in paragraph 2 of section 2.2: it is based on visual inspection of the lidar data, i.e. a cloud free period should have no clouds as observed by lidar, i.e. high backscatter at top of boundary layer. Given the lack of radiation measurements during the campaign, this relatively crude method was used.

P19911, L3 Yes, “overnight” = “night-time”. Both terms are used throughout the paper.

P19912, L15 “Dotted” has been changed to “dashed”. The caption to figure 4 has been modified to include the dashed line to clarify its presence.

P19915, L11 Please see response to 1iii): yes, we believe that the source area of both lidar and sonic anemometer are likely to be similar, therefore the zero-plane displacement height determined from the sonic measurements is appropriate for the lidar measurements.

The Dall'Osto reference has been removed. Figures have been amended to use standard notation. Figure 6 text has been enlarged.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 19901, 2010.