Atmos. Chem. Phys. Discuss., 10, C13620–C13623, 2011
www.atmos-chem-phys-discuss.net/10/C13620/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimating the atmospheric boundary layer height over sloped, forested terrain from surface spectral analysis during BEARPEX " *by* W. Choi et al.

Anonymous Referee #2

Received and published: 14 February 2011

Summary: This manuscript could be made publishable with minor revisions, but there is room for a stronger paper with major revisions to investigate deeper the physics of the BL.

General comments:

This interesting paper ties together a number of concepts in pursuit of a long-standing problem: whether z_i can be obtained purely from near-surface observations – in this case from a rather complex site. The authors argue that they can (during the day), but don't have a lot of data and often resort to discarding outlying values. Also, some of the data analysis can be improved and certain results (such as Table 4's L values) indicate that this improvement is needed.

Overall, the problem is to identify the processes that control BL development and determine if any can be characterized by surface-based observations. The discussion of the influence of terrain/surface on the u-component is intriguing and should be expanded. One wonders if the terrain influences both u and z_i, so that u would be a better predictor of z_i than v?

It is nice to see, from this paper and others, that there is some hope of obtaining at least constraints on z_i for models. Perhaps another conclusion of this paper should be the suggestion that all surface-layer data sets publish lambda_u and lambda_v estimates to allow future uses to compute z_i?

Specific comments:

Section 2:

It should be noted that these observations were taken at $\sim$4 m above a $\sim$8 m high canopy, which would be expected to be within the roughness sublayer. In this sublayer, MOST semi-empirical relations often do not agree with those relations found in surface-layer flows. However, the authors have been careful not to rely on MOST and only use L to classify the stability, so their results should be generally valid.

The authors don't say at what time scale the rotation to v=0,w=0 was performed. Common practice is to use the planar fit method (Wilczak et al.) over relatively planar sites, which may be the situation here. Rotating each run, as presumably was done, has the potential to overcorrect winds in the case that persistent mesoscale structures occur. Overcorrection would have the biggest effect on the flux measurement, rather than the integral length scale estimate and thus may not have a large impact on the primary results.

Section 3:

From Fig. 2, I would have selected z_i $\sim$ 15 m, where there is a discontinuity in humidity

ACPD

10, C13620–C13623, 2011

and a clear break in Ri_b. I would attribute high Ri_b values aloft to be pockets of residual turbulence that are no longer in continuous contact with the surface.

3.3: The key result that v is the best variable to use since w depends primarily on surface-layer scaling and u depends on terrain features. Since this latter statement is rather new, it would be nice to give more evidence for it. For example, plotting n_{max} (or even better – λ_{max}) for all time periods as a function of wind direction and relating this to the terrain or other surface conditions.

I note that the GoogleEarth image of this area shows patchy surface cover and the Blodgett WWW page describes it as "90 compartments, which have an average size of 13 hectares". The square root of 13 hectares would be about 360m – is this the observed peak wavelength in u? Of course, the boundary layer itself would be expected to respond to this surface forcing, so z_i might be related to this same 360m. Furthermore, radiosondes traveling along a slant-path might also be affected by this patchiness.

Section 4:

Batchelor's isotropic relation isn't relevant to this discussion since an integral scale implies a peak in the spectrum and thus non-isotropic conditions.

The slab model also would be expected to fail due to horizontal advection (of heat) related to horizontal divergence mentioned in Section 3.3.

Table 4:

The presence of so many L values that are negative is strange. This suggests a problem in the calculation of the heat flux. This could be due either to the coordinate rotation issue or due to moisture contamination of the acoustic virtual temperature measurement of the sonic anemometer that (presumably) was used to compute the heat flux. Since the difference between acoustic virtual temperature and real virtual temperature is rather small (especially so for the relatively dry conditions expected for the summer-

ACPD

10, C13620–C13623, 2011

Interactive Comment

time Sierra), the problem likely is with the coordinate system.

Figure 13:

What is meant by "mean heights/isotherms" over a 6-week period? Are these really the contours of the fields averaged over 6 weeks (including day- and night-time data)? Does such a quantity have any significance?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 25759, 2010.

ACPD

10, C13620–C13623, 2011

Interactive Comment