Anonymous Referee #2

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We appreciate the referee's valuable and insightful comments on our manuscript. All of the comments and suggestions have helped us improve the manuscript significantly.

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.

- We have attempted to revise the manuscript according to your comments below to the best of our ability.

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.

- Despite not obtaining an extremely large data set for this analysis, the data spans the entire summer season (June to September over two separate years), and successfully captures the general trend of PBL development at Blodgett forest. Much experimental effort regarding atmospheric fluxes and photochemistry have been conducted here, and thus the data is extremely useful to the field of atmospheric chemistry. However, the day to day variability in boundary layer height has not been studied to date, in spite of its importance. Thus, we expect this study to help many other research teams who have, are, or will study biosphere-atmosphere interactions and atmospheric chemistry at Blodgett Forest and other similar sites around the world. We feel this work provides both important insights into observed and estimated boundary layer heights, and its diurnal to seasonal variability, in a very relevant environment.

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?

- We would like to emphasize that our results are based on observations. And further, we do not fully agree with your characterization of the problem: we are not trying to identify the processes that control ABL growth, but rather trying to find fingerprints of its depth (at any given time of its diurnal cycle) on surface measurements. The wavelength (or frequency) at peak spectral power of the u-component of the wind did not change much with time (as shown in Fig. 5), which we believe is due to terrain effects as discussed in the manuscript. Discernible diurnal variations in spectral properties of the u-component of the surface winds were not observed, while significant variations appeared in both spectral peak frequencies of the v-component and observed z_i. Thus we conclude that u is decidedly not a better predictor of z_i than v. We have tried to re-emphasize this in the text, and have expanded the discussion of terrain effects on u to the extent that was possible.

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?

- Based on our observations and other studies conducted over flat terrain (Oncley et al. (2004); Contini et al. (2009); Liu and Ohtaki (1997)), we believe that surface-layer measurements of winds can place useful constraints on z_i. However, the empirical relationships between the spectral parameters and z_i most likely vary substantially across location (perhaps due to terrain effects, and perhaps due to other variables.) Thus, we think further investigation will be required to determine the empirical coefficients that scale z_i to the surface spectral parameters, before achieving any useful generalization. The publication of these spectral parameters, without the concommitant measurement of z_i, may not provide much utility at this stage.

Specific comments:

Section 2:

It should be noted that these observations were taken at ~4 m above a ~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.

- As we mention in Section 2. (Theory and Methods) wind data were sampled for 90 min to conduct the spectral analysis during the daytime and 30 min for NBL conditions. As you mention, the Planar Fit (PF) method most significantly affects flux measurements (vertical turbulent motion) and does not heavily influence the horizontal spectra. We have tested the PF method on our analysis and found that the tilt is negligible (α = -0.10°, β = -0.21°). Thus we have concluded that PF does not significantly affect our results. We have added statements to Section 2 to clarify our exact wind rotation method and our justification for considering the method to be inconsequential to our analysis and conclusions.

Section 3:

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

- As described in the manuscript, by its very nature the NBL depth cannot be unambiguously determined. Furthermore, we purposely avoid the use of z_i in reference to NBL depth because it is not the height of an inversion. In this study, we defined the NBL depth as the top of the stable layer (Stull, 1988), above which $d\theta/dz$ approaches zero. Although, the temperature gradient is steepest below 15 m, significant temperature gradients still exist up to ~ 80m. In addition, considering that the source of water vapor is at or near the surface, the increase in specific humidity with height at 15m is not likely evidence of the NLB top. Rather, we interpret sharp discontinuities near ~10 m to be likely caused by the presence of the forest canopy. Contrary to your statement, *low* Ri_b values are expected in the residual layer due to near neutral stratification there, which is not considered turbulent but retains the appearance of turbulence from active mixing during the previous day.

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 - lambda_max) for all time periods as a function of wind direction and relating this to the terrain or other surface conditions.

- Unfortunately, the wind direction at this site is extremely constant due to the strong thermal ciruculation of the Central Valley and Sierra Nevada Mountain Range. The daytime winds during the summer are therefore mostly southwesterly (220-260°) but gently veer throughout the course of the day which would induce a correlation of wind direction with boundary layer depth. In any event, we do not believe the relatively small variation in wind direction will permit a fruitful investigation of its correlation with Λ_u.

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.

- The wavelength of the maximum in the u-spectra (from Fig. 5) is approximately 100 m, so no it does not bear an obvious resemblance to the estimated scale of surface 'patchiness'. It is not clear exactly how to appropriately quantify this scale from the local tower-based experiment. Nor is it clear how the surface heterogeneity would substantively influence the data collected by the sonde. We do not see how this line of speculation could be investigated easily with our current data set.

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.

- Good point. We have removed the discussion concerning Batchelor’s isotropic relationship.

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

- We included a slab model solely for the purpose of comparing our z_i estimates with some other form of simple estimate that could be developed with surface heat flux measurements. Being a simple model it ignores many of the complexities of the real environment, such as subsidence and advection; however, we feel it is still instructive for comparison purposes.

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

- We do not feel that very small positive heat fluxes observed at night are necessarily indicative of a coordinate system rotation error. First, please note that two of the four intervals with $L<0$ in Table 4 are very near twilight. Second, looking at the data set as a whole indicates that most of the positive heat fluxes observed at night are less than 5 Wm^{-2} in magnitude. This is well within conventional measurement uncertainties under nocturnal conditions. Even so, there are many examples of experimenters finding slightly upward heat fluxes over canopies including Lee et al. (Ag. & Forest Met., 1996) who state that 18% of their nocturnal data exhibited upward sensible heat fluxes over a forest site in Canada attributing them as the results of "wave activity". Other examples have been found above the Amazon (Fitzjarrald & Moore, JGR, 1990), over Duke Forest (Oren et al., Glob. Change Bio., 2006), and during the CASES experiment (Coulter & Doran, Bound. Layer Met., 2002).

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?

- The data in Figure 13 is an average of 6-hour NCEP/NCAR reanalysis 1 data for the duration of the field experiment. What we are attempting to explain is a phenomenon (southerly flow above the boundary layer) which was observed on all of the day and night sonde launches conducted throughout the 2009 experiment. Therefore we assume its forcing is more or less stationary over that time frame. Seasonal pressure patterns are reported and considered to have significance commonly in meteorology. For example, Fig. 13a explains the prevailing southwesterly flow that dominates the mid troposphere above the site. The temperature data, because it is aloft at 850 hPa, should not experience a strong diurnal variation and therefore we feel is appropriately averaged over the six week period of interest.