Anonymous Referee #1
Received and published: 29 November 2010

Thank you for your insightful comments on the manuscript, your comments were very helpful in improving the manuscript.

General Comments
The manuscript treats the problem of determination of boundary-layer height from surface sonic measurements. It is interesting and it has potential applications for routine monitoring. However there are some points that should be addressed before publication as specified in my specific comments.

- Detailed responses concerning your comments are described below

Specific comments
In some cases lateral wind is used in other cross-wind. It would be better to use the same name in all the manuscript.

- We have corrected the terminology to be consistent.

In the introduction it is reported "these compounds are in general emitted from the surface, or produced secondarily within the ABL, possessing lifetimes similar to or shorter than the time scales associated with the largest eddies confined by zi.". The formation of secondary pollutants, like for example secondary aerosol, could have time scale of several hours or days (in some cases) so it necessarily a time scale comparable with that of the large eddies.

- In this part of description, we attempted to explain the importance of boundary layer height (z_i) in atmospheric chemistry, mentioning one of a number of examples specifically compounds that are "reactive". In this case "reactive" necessarily means compounds that have chemical reaction time scales that are short or comparable to large eddy turnover times. It is well known that the lifetimes of particulate matter vary widely: from less than a few or several hours (nucleation mode) to several days (accumulation mode). However, we do not think all possible effects of z_i on atmospheric chemistry, and more specifically on particulate matter, should necessarily be included in the Introduction considering the limited space and broadness of its applicability.

In the introduction it is reported "In addition, it is essential to determine the stable nocturnal boundary layer (NBL) height (h), because the impact of dry deposition on chemical species budgets at night is determined by h, and dry deposition can be a major loss mechanism for many reaction products." It is not clear the dependency of dry deposition on h. Actually the deposition velocity in stable conditions is related to friction velocity and to particle diameter (for aerosol).

- The dry deposition rate is determined by both the deposition velocity and boundary layer height: $\partial C/\partial t = v_d/h \cdot C$, where v_d=dry deposition velocity, h=vertical length scale, and C=concentration of the depositing scalar. The distinction is between a *deposition velocity* which is, as you point out, dependent upon surface stress and stability, and the overall mechanism of *dry deposition* which is additionally inversely dependent on the boundary layer depth. Considering the large diurnal variation in boundary layer heights over the continents (less than 100 m to more than 1 km), the overall dry deposition rate is significantly affected by the such variations.

In section 3.1. The surface layer stability was unstable whenever tethersonde measurements were conducted even in the early morning with values (z/L) ranging from 0.03_0.29." The values should be negative. Further, in table 1 zd/L arrives at -0.313.

- We have corrected these sign ambiguities in the revised manuscript.

In section 3.1. "the NBL depths are shown in table III-2 during the period of radiosonde measurements in 2009." It actually appears to be Table 4. It would be useful to have this as Table 2 because it is mentioned just after Table 1.

- There is a typing error in the published manuscript, although it was corrected in the verification processes. We corrected the table number again. However, most descriptions concerning NBL depths are made in Section 5, and thus, we think it is better to locate the Table in Section 5 instead of Section 3.1.

In Section 4. It is reported that Oncley et al. (2004) used the relationship to estimate zi over flat and open snow cover at the South Pole under unstable conditions. However in the introduction this reference was referred for stable environments. There are both stability analyzed in the paper?

- Yes, Oncley et al. (2004) attempted to estimate boundary layer height under both stable and unstable conditions. For unstable conditions, they followed a relationship between integral length and boundary layer depth suggested by Lenschow & Stankov (1986), and for stable conditions they used other simple relationships between *z/h* and spectral and cospectral peak wavelengths based on the stable dataset of Caughey et al., (1979).

In Section 4. “the ratio of integral length scale in our study” it should likely be “length scale”.

- We mean the ratio of integral length scales, Λ_u to Λ_v, as described in the preceding sentence. Because it may cause confusion, we revised the sentence, using just “this ratio”.

Section 5. Measurements of the NBL show limited range (standard deviation 7m corresponding to less than 10% of the NBL height). This is a very limited range of variation so that it is necessary to discuss the uncertainty in the measurements to understand if actually it is possible to see a variation less than 7m in the NBL height. This is important also to understand Figure 11.

- As described in the text (Section 3.1), the NBL height is defined as the top of the stable layer above which dθ/dz is near zero. In most cases, dθ/dz is less than 0.02 $K\cdot m^{-1}$ above the NBL. Considering the distance between each layer (~ 4m for the NBL conditions), the resolution of a temperature sensor (0.01 °C), the detection limit of dθ/dz (~ 0.0025 $K\cdot m^{-1}$) is small enough compared to much steeper temperature gradient near ground as shown in Fig. 2. In addition, we also used humidity (both specific and relative humidity) and wind profiles as well as the profiles of Richardson number to determine the NBL height. Thus we believe the uncertainty in the measurements does not limit the variations of the NBL heights in this study. We have tried to establish a qualitative uncertainty of the NBL measurements in the text to provide a sense of this.

In the concluding remarks. The estimated mean zi in the CBL environments (10:00_16:00 PST) is 780 m (median 810 m) in August and 640 m (median 660 m) in September of 2007. The monthly variations in the fully developed CBL depths most likely resulted from the variations in solar radiation intensities and hence in the surface heat flux. Again it is not clear if this seasonal difference is statistically significant. A discussion o this should be included in the manuscript.

- In order to see if the variation of z_i in September of 2007 is significantly different from the other summer months, we have added the results of a Kolmogorov-Smirnov test in the revised manuscript. This statistical test shows that variations in the mean of z_i during daytime (10:00~16:00) in September are different from those in other months (June, July, and August) at Blodgett Forest ($p << 0.001$). Thus, based on the Kolmogorov-Smirnov test, we conclude that z_i in September **is** statistically lower than other summer months.

In the caption of Table 1 it is used Zi instead of zi as in the text. The same in Table 2.

- We have corrected it.

The fit results in Table 2 should include the uncertainty on the fitted parameters. It is reported the case of all data and the case of 2007 but not the case of 2009 by itself. It appears that data of 2009 could lead to different results. Could the authors comment on this?

- Because we have only 5 data points in 2009 during daytime, we feel it is not appropriate to report stand-alone relationships based on such limited data. Instead, we added these data only to examine if they fall into the range of the relationship obtained in 2007, and in fact, inclusion of both data sets tends to improve the correlation coefficients and alters the fit parameters by <10%. For the same reasons of limited data, we also caution drawing any definitive conclusions regarding the NBL height relationships. Nevertheless, we have added an uncertainty estimate in each of the parameter fits of Table 2 as recommended.

In the caption of Figure 6 it is reported "obtained from Eq. (3) and (4): : :". This should likely be "obtained from Eq. (4) and (5): : :"

- Figure 6(b) and (c) shows the scatter plots between $n_{max,1}$ and $n_{max,2}$ vs. observations. $n_{max,1}$ and $n_{max,2}$ are normalized frequencies at spectral peak obtained from Eqs. (3) and (4). Thus, it is correct.