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Comment

Interactive comment on “Comparison of mixed layer heights from airborne high spectral resolution lidar, ground-based measurements, and the WRF-Chem model during CalNex and CARES” by A. J. Scarino et al.

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Received and published: 12 November 2013

Thank you for the valuable comments and suggestions on our efforts here. We have accommodated most of the suggestions and we think that it has helped us in improving the presentation of our paper. We hope that we have succeeded in answering your concerns.

Response to general comments:

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Reviewer: Abstract lines 4-5. “provided a data set appropriate for studying characteristics of the planetary boundary layer (PBL).” Characteristics of the PBL is too general. Be more precise.

Abstract lines 12-13. “which is a subset within the PBL.” This is understood.

Response: In the revised paper, we are more precise in that description and have removed “which is a subset within the PBL” from the abstract. It now reads “provided a data set appropriate for studying the structure of mixed layer (ML) heights and their spatial and temporal variability.”

Reviewer: The results presented in the abstract are not especially significant, and do not lead to any obvious conclusion. Simple lidar-radiosonde and lidar-lidar comparisons have been done many times. This is not a new finding. Lidar-model comparisons are not as common, but a simple listing of statistics with no clear interpretation of the results is not appropriate for the main findings of a research paper.

Response: A clearer purpose on the motivation for this paper (lidar-model comparisons) has been added in the revision – see attached document “Revised Abstract and Introduction”.

Reviewer: The statistics are meaningless without information describing the characteristics of the observations and models. What is the data resolution in time or space, and what domain do the observations and models encompass?

Response: In the revised paper, we included more specific information about the resolution and domain of the model.

The following has been added on the WRF-Chem specifications: “As described in Fast et al. (2012), the WRF-Chem model (Grell et al., 2005; Fast et al., 2006) was used to provide operational support for the CARES campaign by providing high-resolution forecasts of wind and tracer dispersion resulting from carbon monoxide emitted from urban locations. The domain configuration used in this study is identical to Fast et al.

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(2009), with a horizontal grid spacing of 4 km that encompasses all of California and the surrounding region. The specific parameterizations used in this study are listed in Table 3. As in Fast et al. (2012), the Mellor-Yamada-Janjić (MYJ) scheme (Janjić, 1990; 2002) is used to represent PBL mixing. Several other ML parameterizations that employ either turbulence kinetic energy or non-local closure approaches are available in WRF; however, it is beyond the scope of this paper to compare the performance of multiple ML parameterizations using the lidar measurements. Some of the meteorological parameterizations are also different than those in Fast et al. (2012) because of future plans to study aerosol direct and indirect effects that require coupling aerosols to certain radiation and cloud schemes. A continuous simulation from 1 May to 30 June 2010 was performed that was constrained to the large-scale meteorological analyses from 4 km above sea level to the model top at 12 km. The MOSAIC aerosol model (Zaveri et al., 2008) and SAPRC-99 photochemical mechanism (Carter, 2000) was used to simulate regional-scale evolution of aerosols and trace gases, respectively. Secondary organic aerosol formation was represented by a simplified 2-specie volatility basis set approach as described by Shrivastava et al. (2011). Anthropogenic emissions were obtained from the California Air Resources Board and were developed for the 2008 NASA Arctic Research of the Composition of the Troposphere from Aircraft and Satellites (ARCTAS) mission over California (Jacob et al., 2010). Initial and boundary conditions for the meteorology were obtained from the global analyses of the National Center for Environmental Prediction's North American Mesoscale (NAM) model, while initial and boundary conditions for trace gases and aerosols were obtained from the global MOZART model (Emmons et al., 2010). The performance of the near-surface simulated meteorology was very similar to that described in Fast et al. (2012) and the differences in the meteorological parameterizations did not lead to a substantial changes in the overall model performance (not shown). For example, the model reproduced the observed variability in the synoptic conditions and near-surface diurnal variation in thermally-driven flows associated with complex terrain and land-sea contrasts. Details of the performance of simulated aerosol mass, composition,

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and size distribution compared with the extensive CARES and CalNex field campaign observations will be presented in another study. As described in Fast et al. [2006] and Barnard et al. (2009), simulated aerosol properties and Mie theory are used to compute backscatter and extinction at four wavelengths: 300, 400, 600, and 1000 nm. To compare simulated backscatter and aerosol profiles with the HSRL measurements during CARES and CalNex, software tools from the Aerosol Modeling Testbed (Fast et al. 2011) are used to interpolate the simulated quantities in space and time to match the aircraft flight paths. The Angstrom relationship is then used to interpolate the simulated optical properties to 532 nm for direct comparisons with lidar measurements. The software tools also extract profiles of meteorological quantities such as potential temperature and humidity that are used to compute ML heights along the aircraft flight path. WRF-Chem ML heights are determined by two methods. The first method uses a critical potential temperature gradient to identify the top of the CBL, which is widely used by the atmospheric community. We use a value of 1 K km⁻¹ to define the critical gradient, as in Liu and Liang (2010); however, this method can identify multiple layers for weak vertical potential temperature gradients. Therefore, we correct anomalous values by visual inspection using the simulated vertical profiles of specific humidity. The second method is identical to the algorithm described in Section 3.2, except that simulated profiles of backscatter or extinction are used. For both methods, instantaneous model outputs at hourly intervals are used to determine ML heights.”

Reviewer: The introduction needs to be restructured to focus the paper towards a clear scientific objective. This is lacking at the present time.

Response: The Introduction has been streamlined to include the purpose and focus on published work for ML height comparisons between observed and modeled. Section 2 now includes information on the field campaigns and data sets. Section 3 is the methodology for the observed ML heights and includes the WRF-Chem specifications. Section 4 is now the results specific to the field campaign and Section 5 is the summary.

Reviewer: Page 13724, Line 7. Discrete, not discreet.

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Response: Corrected

Reviewer: Page 13724, Line 9. “mix thermodynamic conditions,” Thermodynamic conditions can’t be mixed. They are a method of describing the atmosphere. Air can be mixed. Air of varying thermodynamic properties can be mixed. A senior author should provide line-by-line editing to help bring the document up to standards appropriate for a professional journal.

Response: This explanation and terminology has been removed from the introduction and the boundary layer terminology has been revised (see response from your comment on the distinction between PBL and ML.) In the revision of the paper, Rich Ferrare and Jerome Fast will provide editing before final submission.

Reviewer: Page 13724, Line 22-23. “Even recently, in a 2009 report from the National Academy of Science (National Research Council, 2009), researchers are still recommending that determining the height of the atmospheric boundary layer is one of the highest priorities.” The writing is not at a standard suitable for publication. This is just an example. This sentence should say, “The NRC (2009) recommended. . .(what? Highest priority in what?)” The introduction has many examples of writing that is passable English, but word choices, styles and technical precision that are not refined sufficiently for a journal article. The manuscript must be reviewed carefully by a senior author or technical editor able to provide line-by-line editing assistance.

Response: Restructured the sentence and included info on why it is a high priority: “The National Research Council (2009) points to inadequacies in current national mesospheric observational capabilities necessary for addressing priorities like forest wildfire smoke dispersion, more extensive air quality forecasting, short-range forecasting of high-impact weather, and support to regional climate modeling. In particular, vertically resolved mesoscale observations are lacking and the report specifically recommends that determining the height of the atmospheric boundary layer should be one of the highest priorities for addressing these inadequacies.”

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Reviewer: Line 8-11, page 13728: First, this is a run-on sentence that needs editing. Second, the distinction between the ML and the PBL does not make sense. These terms are not used to define the method of measuring the height of the boundary layer. Why is the lidar measuring the ML, and the radiosonde the PBL? What is the difference, other than the method of finding the top of the layer? There is not a definable difference between the PBL and the ML in convective conditions.

Response: This sentence will be restructured in the revision. Per the suggestion from Reviewer 1 and a decision among the paper authors, we are going to use the term mixed layer (ML) height for both lidar/ceilometer and radiosonde measurements. In areas where it might be necessary to denote which ML height we refer to, we add “aerosol” when discussing the lidar/ceilometer-derived heights. These terms will also work for WRF-Chem as well when discussing modeled thermodynamic profiles and modeled backscatter.

Reviewer: The overview of methods (beginning of section 2) is extraneous. The introduction is the place for describing past work, not the methods section. And as best I can tell, the paper is not focused on developing methods of deriving PBL depth from a given observation (e.g. radiosonde), but on studying the spatial variability of PBL depth. A review of methods of extracting PBL depth from radiosonde data is extraneous.

Response: This overview of methods sections has been removed and some of the descriptions of past work descriptions have been moved to the Introduction.

Reviewer: Section 2.1 should not present a review of other approaches to this problem. A terse description of the method you are using with literature to justify it is sufficient. Figure 1 is not needed.

Response: In the revised paper, we have reduced the description of the method used on the radiosonde profiles and removed Figure 1.

Reviewer: Page 13730, line 15. If the lidar data are 0.5 second extracts from a 10

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second running average, then the number of independent lidar profiles is much smaller than the number of points in any analyses. If the data are averaged over 10 seconds, why are 0.5 second profiles being used for analyses?

Response: Half-second profiles are not being used in the analysis. The 10 seconds are mentioned because that is the temporal resolution of the backscatter profiles. All analyses that refer to MLH computed from backscatter use a one-minute running average of MLH computed from backscatter profiles.

Reviewer: Page 13730. Line 24-27. Dilation is not a distance. It is a dimension or scale of the wavelet.

Response: Davis et al. gives dilation in units meters and defines dilation as the distance of range gates (ie. 10 range gates or 150 m). Brooks refers to dilation as a spatial extent and I will use the same definition in the revised paper.

Reviewer: Page 13732, line 4. “Although this method provides good estimates of the transition zone limits, it often does not provide accurate estimates of H3.” How is “accurate” defined? Brooks (2003) and Davis et al (2000) focused on developing objective methods. The authors are free, of course, to pick different criteria or methods, but these should be defined. “Accurate” implies comparison with something. And if H1 and H2 are sound, it seems unlikely that H3 is seriously flawed. The text also suggests that at times, results of the algorithm are discarded and the PBL depth is determined “manually.” The text gives qualitative descriptions of when this is needed (clouds and complex aerosol structures), but more specificity would be helpful. At what resolution are the “manual” PBL depths determined? Profile by profile, 10s? Or is a color plot used and a pencil draws a line over the confusing gap?

Response: Brooks and Davis focused on developing objective and automated methods. We also use an objective method that is very similar to Brooks and Davis, specifically using the same definition of ML height as a locally maximum gradient; however, we find that their algorithm does not always do a good job of finding the right local

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maximum in conditions that are dissimilar from the single-layered, relatively thin marine boundary layer that their work was based on. Our modifications, which are described in detail in the following paragraph, are about improving the automation of the algorithm to find the appropriate gradient in a wider variety of circumstances. As for the manual heights, they are considered along with the heights determined from the algorithm only when the manual heights differ at least by $\pm 300\text{m}$ from the algorithm heights. So, yes, results from the algorithm are removed, but only when the $\pm 300\text{m}$ difference is met. The manual heights are drawn in through the algorithm with guidance from a three-wave convolution backscatter plot that shows where the strongest gradient is located.

Reviewer: Section 2.2, like 2.1, should not present a review of the development of these methods. It also shouldn't present the results of this study. It should present a terse, efficient description of the methods used in this study. If, however, this paper is intended to be a study of the method of deriving PBL depth from lidar, then more evaluation of the methods is needed. Figure 2 is not necessary. This was illustrated in Brooks (2003) and subsequent publications.

Response: The section describing the methodology for the HSRL backscatter profiles has been simplified to explain the methods of Brooks and Davis that we are using before describing the modifications we have done. Figure 2 has been removed.

Reviewer: Figures 3 and 4 present lots of areas with no backscatter data (black vertical bars – I presume that represents a lack of data) where the PBL depth has been determined. How was the PBL depth determined in these areas?

Response: When the PBL depth was plotted, the dots were connected which is why it looks like there is a PBL depth in those areas. In any updated figures of this type, the PBL depth will be indicated by just dots without the interconnecting lines.

Reviewer: Figures 3 and 4 note that they are 1 minute averages. Why? Aren't the PBL depths being determined at 10s time resolution? If they are averages, are they block

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averages or running averages?

Response: Page 13732, line 14, indicated that the PBL heights plotted are “one-minute running means.” I realize that the caption for Figures 3 and 4 only say “one-minute average.” In the final revision I will use the figure that I have included and the caption will be: HSRL backscatter curtain on 18 June 2010. Deep purple lines show one-minute running average bottom (H1) and top (H2) transition zone heights, and the white line shows the one-minute running average height of the maximum in the wavelet transform of the Haar function. The magenta line shows the “best estimate” ML height. The ground is shaded in brown. The black area between the ground the backscatter data is where the backscatter profiles are limited to 90 m above the ground to avoid contamination from the effects of surface returns.

Reviewer: What is the altitude used in Figures 3 and 4? Altitude above sea level? Where is the ground?

Response: The altitude is above sea level. A white line indicates the ground in these plots. While it is visible on the digital copy of the paper, it barely shows up when printed. In the revised version, the ground line has been made thicker and the ground has been shaded.

Reviewer: Both Figures 3 and 4 are not needed.

Response: Figure 3 has been omitted.

Reviewer: Page 13733, lines 19-21. “For some applications, the full depth of the aerosol layer may be more relevant than the ML height. For this reason, the altitude of the maximum aerosol gradient has also been computed for all HSRL flights.” Figure 5 is interesting, and the correspondence between the residual layer and current mixed layer in both lidar and radiosonde data is a nice illustration. The logic for this additional boundary determination, and the methodology for its derivation, however, are both lacking. “for some applications” is not sufficient. For what applications in this

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paper is it needed? Or is this really a lidar methods paper? And how is “the maximum aerosol gradient” defined and derived from the lidar data? After 3 pages of discussion of wavelet methods, this is not sufficient detail. Reduce the pages of discussion of wavelet methods and focus on the methods you are using for this study.

Response: Any mention of the maximum aerosol gradient has been removed from the paper.

Reviewer: Page 13733, section 2.3. Ceilometers are backscatter lidars. Why are these data not processed in the same way that the HSRL data are processed?

Response: The ceilometer PBL heights that were used were calculated from the backscatter using the proprietary Vaisala ML height algorithm were the only form of Vaisala data and/or results that were provided to us. Consequently, we could not apply the same algorithm that was used to process the HSRL data.

Reviewer: Why is Figure 6 included? If this paper is focused on testing methods of PBL depth determination using lidar, then a common method must be used. If this paper is simply using the ceilometer output and algorithm, then this figure isn't needed. In either case, results belong in the results section, not the methods section. The purpose of the figure isn't clear. A simple demonstration of the data does not warrant another figure. The methods have already been published, as evidenced by the citations.

Response: The figure has been removed.

Reviewer: The ceilometer PBL depth determination appears to fail seriously at 2100 UTC. Line 21 of page 13734 says only that “it is possible” that local minimum 2 could be the PBL depth. It very clearly does appear to be the PBL depth in the area around 2100 UTC (but not at 1800 or 0000 UTC). It appears that the ceilometer algorithm has significant failures, but no means of dealing with these failures is presented. What is done with these data? The methods should explain what methods will be used, not what possible interpretations could be drawn from the observations.

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Response The ceilometer PBL heights on this particular day are indicated by local min 1, except for the area around 21 UTC where local min 2 heights are used. Haman et al., discusses the algorithm in more detail and the methodology used. This reference is included in the text.

Reviewer: The color background for Figures 7 and 8 is never described.

Response: The background on these figures refers to surface elevation in order to show the terrain found in the different regions of California. This is indicated this in the caption.

Reviewer: The two figures could be combined into a single, 2-panel figure. The captions and information displayed are nearly identical in nature.

Response: Thanks for the suggestion. We merged the two figures into the 2-panel figure and adjusted the caption to include the clarifications on what is in the figure.

Reviewer: Page 13734, lines 17-24. The contents of the CALNEX science plan do not need to be rewritten here. The citation is relevant. This material also belongs in the introduction, not the methods.

Response: The discussion of the CalNex and CARES campaigns has been moved between the Introduction and Methods to provide more continuity.

Reviewer: Page 13735, line 15. This paragraph is introductory material and does not belong in the methods. This is not a description of the field campaign or data set, it is motivation for the study.

Response: In the revised paper, the description of the field campaigns is now in Section 2, located between the Introductory and Methods section.

Reviewer: Page 13736, line 27. As above, this paragraph does not belong in the methods section.

Response: See previous response.

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Reviewer: A description of WRF-Chem should be presented in the methods section.

Response: A description of the WRF-Chem model has been included in the methods section.

Reviewer: MYJ is only the PBL parameterization. Several other parameterization choices are required for a brief description of the implementation of the model.

Response: Yes, the MYJ is one of several PBL parameterizations in WRF. The purpose of this study is not to evaluate all of the PBL parameterizations in WRF, but to look at the performance of the model for one particular application. This is described in the updated methods section.

Reviewer: Page 13737, line 17. Why isn't the same method used to derive PBL depth as is used with the lidar data? What is the threshold used here to find PBL depth. "A threshold" is not sufficient information.

Response: The ML depth from a meteorological model is normally computed using the potential temperature gradient. In this case, the model uses a threshold value of 1 K km⁻¹. The specific value, as well as a reference has been added in a revised manuscript. On the other hand, this lidar does not measure temperature; so two different methodologies must necessarily be used for computing mixed layer depth. We therefore perform an additional more direct comparison using the ML heights derived using the WRF-Chem simulated backscatter, processed through the same algorithm as the lidar backscatter. This investigation determined that the difference between the methodologies was not significant. These results were shown in Figure 20 in the initial submission and discussed in section 4.2.2.

Reviewer: Figure 9 is probably the first figure that should remain in the paper, save for the map of the flight patterns and perhaps one figure summarizing the methods of deriving PBL depth from lidar and model. The point of Figure 9, however, is not clear. What was tested with this comparison? What is the point of this comparison? Why

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is the LA Basin considered separately from the Bay area / central valley? What is the reader supposed to learn from this figure?

Response: The purpose of this figure is to see how the ML heights compared to evaluate how the model ML heights performed. We kept the LA Basin separate from the Bay area/Central Valley since they were part of two different field campaigns. The flights during CalNex were only in the LA Basin, whereas the flights in CARES were in the Bay area/Central Valley.

Reviewer: Figure 9 does not describe the times of day of the observation used in the figure, and the location of the data isn't as clear in the caption as it should be. A table of flight times and locations would strengthen the paper. Figures 7 and 8 have no temporal information. The times should be LST, not UTC. PBL development is not a function of UTC – LST is the relevant time.

Response: All references of UTC in the paper will be changed over to LST, as well as indicating what the UTC offset is into the figure captions. We will also be including a table of flight times to the final revision of the paper.

Reviewer: Provide an interpretation of the statistical values from Figure 9. What is the significance of these values?

Response: Figure 9 shows that while there is some agreement in this comparison, there is a large scatter/noise, which results in the large RMS difference.

Reviewer: Figure 10 is not from “the entire campaign” but only from the LA Basin, if I understand the text. Please correct the caption.

Response: We added “CalNex” before campaign.

Reviewer: Why is the comparison in Figure 10 offset by about half an hour for every point? This seems like an unnecessary complication. It might make the figure easier to see, but it adds complication to the comparison by including a time offset. Please clarify the time boundaries of the data being evaluated.

Response: As indicated in the figure caption, it isn't offset by half an hour at every point. There are thicker vertical lines to indicate a block of time, i.e., the HSRL and WRF-Chem points that fall between 11 and 12 LST labels are from the ML heights during 11 LST.

Reviewer: Is the WRF-Chem output an hourly snapshot, or an hourly average?

Response: Model predictions are instantaneous quantities, so a snapshot. When the PBL depths are computed, the potential temperature is interpolated in time (between hourly values) and in space along the aircraft flight track.

Reviewer: Page 13738, line 11. “. . .at AGL” is not proper English.

Response: Corrected.

Reviewer: Page 13738, lines 11-14. The logic for the comparison should be presented in the introduction and methods sections, not with the presentation of results.

Response: These sentences were moved to the methods section as suggested.

Reviewer: The separation between the HSRL data and the ceilometer site needs to be explained more clearly. “and within various linear separation ranges” is not sufficient to understand the results.

Response: Language has been added to try and clarify the separation. Please see the response to the Figure 11 comment below.

Reviewer: Figure 11. “The horizontal lines indicate the spread of all HSRL-derived ML heights encountered as the B-200 flew within ± 7.5 min of each ceilometer measurement, though not limited by separation distance” This does not give enough information about the horizontal lines (the spread = Maximum and minimum? Standard deviation?)

Response: We agree that this figure may be challenging to interpret and easier to read if it were displayed without the horizontal lines. By “Eliminate the time separation” does

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the reviewer suggest that the horizontal lines be removed? We believe that we did exactly what the reviewer suggests in their statement “Certainly collecting measurement from a similar time interval is logical, but then express the comparison as a function of separation distance”. Because both spatial and temporal separation between the aircraft and the ceilometer simultaneously affect the comparisons, eliminating the time separation and grouping all aircraft data as a function of only distance from the ceilometer would not eliminate the differences in ML height measurements due to separation of measurements in time. For example, if the measurements were taken within 30 km of each other, but separated by several hours, we would not expect the measurements to be correlated (see Figure 12). The ceilometer only recorded data every 15 minutes, so we first limited the aircraft data to only those measurements taken within +/- 7.5 minutes of a ceilometer measurement and then looked for correlations at various lateral distances between the aircraft and the ceilometer (i.e., the aircraft within 5 km of the ceilometer, within 10 km of the ceilometer, etc.). The “closest approach” points, represented by the dots in Figure 11, are important because those are the points where we would expect the correlations between the aircraft and ceilometer measurements to be highest. The aircraft flight plans were not designed to take the aircraft directly over the ceilometer on each flight, so we had to find the point of closest lateral approach between the aircraft and the ceilometer for our comparisons. The horizontal bars then indicate the total amount of difference in ML heights measured by the airborne instrument to give the reader a feel for the large amount of variability within a complex area such as the LA Basin, even when the measurements between the aircraft and ceilometer are taken very close in time to one another. Yes, the horizontal lines can be outside the 0-30 km and 30-50 km bounds and indicate the minimum through maximum values in the measurements. Language has been added to the text and Figure 11 caption to try and help clarify the figure.

Reviewer: Page 13738, lines 22-27. These are interesting results. A study of the decorrelation length of PBL depth as a function of the surface conditions would be a challenging but interesting contribution to the research literature. I recommend that the

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authors keep this interesting line of inquiry, but clarify as suggested above by focusing on separation distance, and perhaps by adding more information about the land surface and how the correlations change with changing land surface. This might require spatial maps of PBL depth from the HSRL.

Response: Thank you for your suggestion. This work is underway for a future publication.

Reviewer: Page 13739, lines 1-4. I am puzzled by this text. If the results being presented are all altitudes AGL, then no adjustment of the PBL depth is required. Or by “ground altitude” do the authors mean the elevation of the earth’s surface at the locations of study? If the latter, this is interesting, and worth developing further. Understanding the mechanisms governing spatial variability in PBL depth, and the degree to which this can be captured by simulations is a worthwhile topic for publication.

Response: We mean the latter, the elevation of the earth’s surface at the locations of study. We have added the term “MSL” to sentence 2 to help clarify. This work will be developed further for a future publication.

Reviewer: Page 13739 lines 7-14. This is repetition of previous discussion. Delete.

Response: These lines help explain the horizontal bars in Figure 11. Please see our response to the comments on Figure 11 above.

Reviewer: Figure 12 is not needed.

Response: We feel that Figure 12 is necessary to display to the reader that the differences in ML heights measured by the HSRL and the ceilometer are not primarily or even significantly due to natural evolution of the ML height in time, which would be the main explanation for differences between measurements that are not exactly correlated in time. Figure 12 shows that the ML height evolution is slow across the time scales of our comparisons, and therefore cannot be the source of the differences.

Reviewer: Line 19-26, page 13739: Figure 13 is interesting, but what’s the point? How

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does this differ from the compilation of differences in PBL depths shown in Figure 11? If the idea is to study, in addition to spatial decorrelation lengths, the dependence of spatial differences in PBL depth on day / weather conditions, make this part of the study, describe the methods in the methods section of the paper, and show us the results of the study. As it stands this is an interesting figure but with little to no interpretation. What should the reader learn from this figure? Why are 19 and 20 May different? Why are only 19 and 20 May included? (Side point –the colors are difficult to interpret, particularly the white areas on the 20 May plot.)

Response: Figure 13 is intended to show visually that a point measurement of PBL height (i.e. from a ceilometer) may not be indicative of PBL behavior even in areas very close to the ceilometer. This figure is a more visual extension of the information being presented in Figure 11. 19 and 20 May were chosen specifically because they illustrate this point strikingly: within just 1 day, meteorological conditions changed such that the ceilometer PBL height went from a good analog for a large area of the LA Basin (19 May) to not even being within 1000 meters of the PBL heights being measured just tens of kilometers away (20 May). A full interpretation of the differences seen between these two days and between other days during this measurement campaign is beyond the scope of this current study, but language has been added to the manuscript in this section to try to interpret these results more clearly for the reader.

Reviewer: Page 13740 lines 7-14. Do not use the text to repeat statistics presented in the figures.

Response: The stats presented in the text from lines 8-11 are for the individual T0 and T1 sites, so they will stay in the text. We changed the last part to indicate the stats for the two sites combined are displayed in the figure.

Reviewer: Figure 14 and associated discussion: What is the point? What do you learn from these numbers and this figure? Why aren't these results combined with the similar analyses from the LA Basin? I really don't understand the significance of this

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comparison. If there is no interpretation associated with this figure, then it should be deleted from the paper.

Response: Interpretation of the statistics will be included in the revisions. These results aren't combined with those from the LA Basin (CalNex campaign) because the radiosondes were from the CARES campaign.

Reviewer: Line 20, page 13740: what was adjusted in the model? What is the point of a comparison if there is not a control for the numerical modeling? Whatever it was that needed to be adjusted for the model to perform well is likely the most important learning that can come from such a comparison.

Response: For the "adjustments", it means that a few different parameterizations were used than in a prior process of the model. They were minor and the mention of "adjustments" has been removed in the revised paper.

Reviewer: What is the point of figures 15 and 16? What is the reader supposed to learn from these figures and statistics? What is being tested in this paper?

Response: Figure 15 is showing the comparison of the lidar and model boundary layer heights and validating how the model is performing. The large bias is telling us that the model tends to over predict the ML heights. Figure 16 is showing the lidar and model BL output to demonstrate how the BL height behaves diurnally.

Reviewer: Page 13741, lines 17-19. Please delete all repetition of statistics presented in the figures from the text, unless they are being used to draw some conclusion or make some point.

Response: Repetition of statistics has been removed from the text.

Reviewer: What is the purpose of Figures 17-19?

If the statistics of the comparisons are essential, the results from Figure 17-19 would be presented more clearly and efficiently in one table.

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Response: The purpose of these figures was to show the comparison between the observed and modeled ML heights for the 3 regions in the CARES campaigns. These figures will be removed and the statistics will be presented in one table.

Reviewer: Page 13742, line 1. This paragraph does not appear necessary, nor does Figure 20 appear necessary. If there is essentially no difference between the simulated aerosol and simulated potential temperature results, and the point of the paper is a model-data comparison and not a model methods effort, then this was an interesting test that leads to no change in the results. This could be discussed in a sentence or two. A page of text and a new figure are not needed.

Response: Thanks for the suggestion. In the revision we reduced this to just a sentence and removed the figure and statistics from this comparison will be included in a table.

Reviewer: Page 13742. Figure 21 and 22 present a line of investigation that is entirely new. It is not mentioned in the introduction or methods. The WRF-Chem aerosol simulation is not documented at all. The comparison is interesting, but if this is the point of the paper, then this needs to be the focus of the introduction and methods. If this is going to be the topic of another paper, then it needs to be deleted from this paper. Evaluation of the simulation of aerosol backscatter is an interesting topic that would be new and interesting research. What is presented in this paper is, in this draft, entirely out of place.

Response: Our intentions were not to introduce an in-depth analysis of the WRF-Chem aerosol simulation, but rather to illustrate the reasons for differences in mixed layer height in cases where there is some discrepancy between measurement and model. This figure illustrates that there can be differences between the two methodologies (thermodynamic vs. aerosol) and that this difference indicates difficulties in accurately simulating aerosol. We added more motivations and explanation to this. We feel that Figure 21 does add value to the paper, but agree to remove Figure 22.

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Reviewer: Page 13743, lines 9-10, “One of the reasons for the differences is associated with the technique used in finding the ML and PBL height.” I disagree. There is no evidence presented in the results that backs up this assertion.

The remainder of the summary of diurnal variation of LA Basin PBL depths is not written clearly, and appears to be speculation that is not backed up by the results presented.

Response: On three particular flights during the CalNex campaign, there were differences in ML height between HSRL and WRF-Chem. In the late afternoon there are regions where the HSRL ML height is located at the aerosol gradient; the WRF-Chem ML height location is lower and not near an aerosol gradient. This shows why it is relevant to examine both the measured and simulated aerosol backscatter. The summary written on the diurnal variation for CalNex and CARES ML heights was revised and also included this explanation when this figure is first introduced.

Reviewer: Page 13743, line 19. This paragraph simply restates the statistics from the results. There is no synthesis. The variation in model-data comparison over contrasting terrain is an interesting topic. If the authors wish to focus on this topic, they need to develop this topic throughout the document.

Response: The summary and conclusions area has been restructured in the revision to include more synthesis. We have explored this topic and have decided that it is beyond the scope of this paper and may be explored in a future paper.

Reviewer: Line 5-8, page 13745: If the purpose of your paper is to establish the ability of the HSRL to detect PBL depth, then it can be a very short note. In truth, this is not really a publishable result, but more appropriate for an internal technical note. Airborne lidar have been used to detect PBL depth for decades.

Response: The purpose of the paper is to describe the methodology used to derive ML height from airborne HSRL measurements of aerosol backscatter and to use these heights to evaluate modeled ML heights in the CalNex and CARES study region. The

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introduction will be updated to include this purpose.

Reviewer: Line 8-10, page 13745: “The HSRL ML heights also provide additional information to modelers that are either updating or developing the parameterization schemes used in simulations of where the PBL is located.” Be more specific. What is the additional information that is helpful for developing parameterizations? Simply stating that “data are helpful,” is not a conclusion suitable for publication.

Response: We can be more specific. Since the initial paper submission, modelers have used the HSRL ML heights in publications. See Baker, K.R., Misenis, C., Obland, M.D., Ferrare, R.A., Scarino, A.J., and Kelly, J.T., Evaluation of surface and upper air fine scale WRF meteorological modeling of the May and June 2010 CalNex period in California, Atmospheric Environment (2013), <http://dx.doi.org/10.1016/j.atmosenv.2013.08.006> <http://www.sciencedirect.com/science/article/pii/S1352231013006158> The Baker et al. paper will be referenced in the revised paper. There are also others that have used our ML heights in the past few years and have been presented at meetings and could be included in future publications. We will also include a discussion on what was learned from these comparisons to further evaluate the WRF-Chem model.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/13/C9002/2013/acpd-13-C9002-2013-supplement.pdf>

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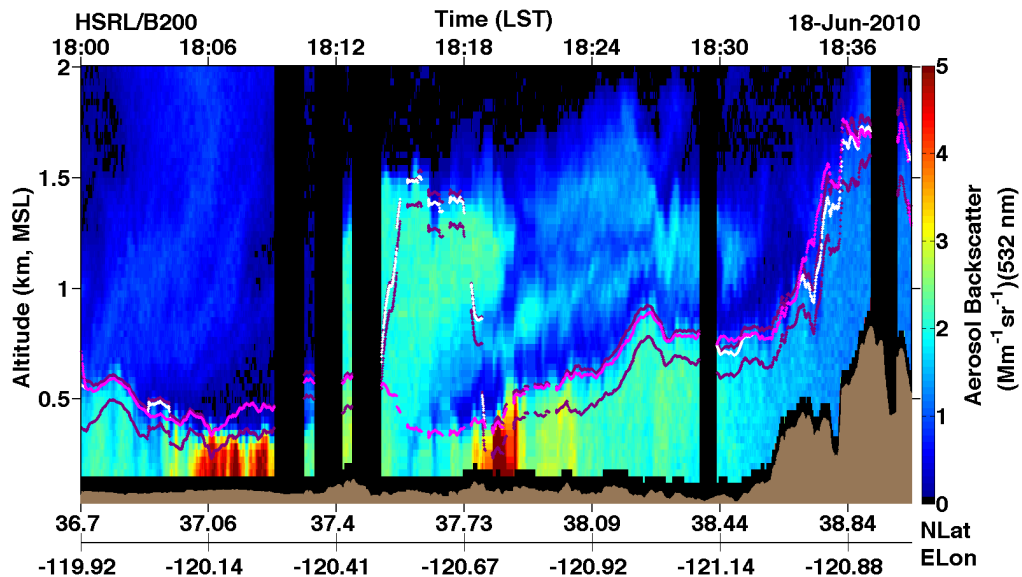
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Fig. 1.

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