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***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.**

Anonymous Referee #2

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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.

This paper uses data from a High spectral Resolution Lidar (HSRL) deployed at California for two field campaigns: Nexus Air Quality and Climate Change (CalNex) and Carbonaceous Aerosol and Radiative Effects Study (CARES). They used the HSRL data to derive PBL heights over regions close to Los Angeles and Sacramento, CA. These

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measurements were compared to PBL heights derived from other available measurements (i.e. radiosonde and ceilometers) and simulated by the Weather Research and Forecast model coupled with chemistry (WRF-Chem). They want to explore temporal and spatial variability of the PBL and evaluate the performance of the model at the two field campaign regions.

Overall discussion: The authors are to be congratulated for bringing together an interesting collection of raw materials, and making preliminary investigation into a number of interesting topics. The paper, however, wanders from topic to topic without an overall guiding logic. The focus of the paper is not clear, many of the results presented are not significant or new findings and can be deleted, and the topics that are new and interesting are not yet developed enough to warrant publication. This is a promising beginning, but it is just a beginning of an effort towards a publishable work. The author list includes a number of senior authors who need to help focus the paper on a topic worthy of publication, and help to develop the paper further in that direction. At present the document reads like an internal progress report, not a well-structured investigation that should be published in the peer-reviewed literature.

In particular, the paper addresses a number of topics that I will review.

First, the methods are almost entirely focused on approaches for determining PBL depth from lidar. This discussion is not new, and the paper does not add new progress in that area. Simply establishing that HSRL can be used to detect PBL depth is not sufficient for publication. If the focus of the paper is the methodology for deriving PBL depth from lidar, then I do not see the innovation in this paper. If this is not the focus of the paper (which I believe to be the case), then this discussion can be cut back considerably.

The paper devotes several figures to model-data PBL depth comparisons. This topic is potentially interesting and innovative, in particular given the complex terrain of California. Model-data PBL depth comparisons are not new, but spatially extensive compar-

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isons are less common, and studies over complex terrain are not common. However, the current document simply presents some statistics of comparison, and presents no interpretation of or context for those comparisons. There is the beginning of an effort to contrast model performance as a function of surface characteristics (coast, valley, mountain), but no objectives are presented and no conclusions are reached.

The paper devotes a few figures to the comparison of stationary PBL depth measurements to the spatially varying measurements from the airborne HSRL. This, in my opinion, is the most interesting and best developed topic in the paper at present but again, there is no objective presented for this topic of study and the conclusions reached are weak and diffuse. The study of the decorrelation length of the PBL depth as a function of underlying terrain is interesting and could serve as an interesting focus for the paper, but the results presented are not yet developed sufficiently to warrant publication.

Finally the paper presents a small study of model predictions of aerosol backscatter. This is a potentially interesting topic, but almost no background is given, and the point of the figures appears to be only to introduce a future publication. This paper should not be used as the introduction for another paper. But this topic could lead to interesting, publishable results. Again, the authors need to focus on a scientific objective, and pursue it, rather than present a string of loosely related, very marginally interpreted results that lead to no clear scientific advances or conclusions.

I apologize for the critical remarks, and I again applaud the group for working towards some interesting findings. The current document, however, is premature. I hope that the authors will consider a serious revision with substantial contributions from the senior authors who should be providing this sort of overarching guidance to the paper.

1) Does the paper address relevant scientific questions within the scope of ACP? Yes, the paper addresses relevant scientific questions within the scope of ACP.

2) Does the paper present novel concepts, ideas, tools, or data? There are hints of new ideas and results (e.g. variation of PBL depth over complex terrain, and ability to

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simulate the PBL over complex terrain), but these results are not developed enough to lead to any substantive new results.

3) Are substantial conclusions reached? No major conclusions were reached by this paper. The authors need to choose a clear scientific focus, and develop the paper further in that area. There are some promising elements in this draft, but it is not publishable at this time.

4) Are the scientific methods and assumptions valid and clearly outlined? No. The methods focus almost entirely on methods of deriving PBL depth from lidar data. The results do not deal with this topic. Since there is no clear scientific question being addressed, it is difficult to say that the methods and assumptions are valid. They are not clearly outlined.

5) Are the results sufficient to support the interpretations and conclusions? Clear and substantive results are not presented.

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? The descriptions of the calculations of the PBL height are well explained. The WRF-Chem simulations are not described well.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes, the authors gave proper credit to related work. However, the new/original contributions should be developed further. If this is done, new elements of literature review will need to be added to the manuscript.

8) Does the title clearly reflect the content of the paper? Yes, the title of the paper clearly reflects the content of the paper.

9) Does the abstract provide a concise and complete summary? Given the state of the manuscript, this question is premature.

10) Is the overall presentation well structured and clear? No, the overall presentation

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is not structured well. Methodology is scattered throughout the document, and some is never presented. The scientific focus of the paper is never clarified, and the paper wanders from topic to topic without a logical path.

11) Is the language fluid and precise? There are points where the language needs to be edited to conform with typical norms of published work.

12) Are the mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes, the paper needs major restructuring, and many of the results presented are not necessary. Detailed recommendations follow.

14) Are the number and quality of references appropriate? The quality is usually passable, though the captions are sometimes lacking in detail. There are many figures that are not needed.

Additional Comments:

Abstract

i. 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.

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

iii. 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.

iv. 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

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domain do the observations and models encompass?

1) Introduction

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

ii. Page 13724, Line 7. Discrete, not discreet.

iii. 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.

iv. 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.

2) Overview of methods used to compute the mixed layer height

i. 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.

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ii. 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.

iii. 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.

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

v. Page 13730. Line 24-27. Dilation is not a distance. Is it the dimension or scale of the wavelet.

vi. 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?

vii. 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,

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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.

viii. 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?

ix. 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 averages or running averages?

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

xi. Both Figures 3 and 4 are not needed.

xii. 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 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.

xiii. 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?

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xiv. 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.

xv. 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.

3) Field campaigns and data sets

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

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

iii. 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.

iv. 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.

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

4) Comparison of mixed layer heights 4.1) LA Basin during CalNex

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- i. A description of WRF-Chem should be presented in the methods section.
- ii. MYJ is only the PBL parameterization. Several other parameterization choices are required for a brief description of the implementation of the model.
- iii. 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.
- iv. 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 is the LA Basin considered separately from the Bay area / central valley? What is the reader supposed to learn from this figure?
- v. 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.
- vi. Provide an interpretation of the statistical values from Figure 9. What is the significance of these values?
- vii. Figure 10 is not from "the entire campaign" but only from the LA Basin, if I understand the text. Please correct the caption.
- viii. 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 add complication to the comparison by including a time offset. Please clarify the time boundaries of the data being evaluated.
- ix. Is the WRF-Chem output an hourly snapshot, or an hourly average?

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

xi. 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.

xii. 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.

xiii. 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? 5% and 95% percentile?) or the distance between ceilometer and aircraft. Does this mean that the horizontal lines can be outside of the 0-30km and 30-50km bounds of the figures? If so, why? Why not group all aircraft data as a function of distance from the ceilometer? Why is the “closest approach” important? The figure would be more interpretable if all data were shown as a function of separation distance between the two instruments. Eliminate the time separation. The figures and analyses are about separation distance. Keeping separation in (flight?) time mixed in with distances is not helpful. In fact, it is not even clear what that separation time means. Is that flight time from the ceilometer location, or separation in the timing of the measurement regardless of location? Certainly collecting measurements from a similar time interval is logical, but then express the comparison as a function of separation distance.

xiv. 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 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

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PBL depth from the HSRL.

xv. 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.

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

xvii. Figure 12 is not needed.

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

4.2) Sacramento region during CARES

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

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

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iii. 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.

iv. 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?

v. 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.

vi. What is the purpose of Figures 17-19?

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

viii. 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.

ix. 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.

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5) Summary

i. 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.

ii. 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.

iii. 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.

iv. 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.

v. 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 13721, 2013.

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