

General response

The authors thank the reviewer for his/her helping to improve this manuscript and the English language. We greatly appreciate your detail comments and creative suggestions. We revised the paper according to your suggestions. Our replies to the comments are given below.

Anonymous Referee #1

Received and published: 15 December 2015

Review of “Marine boundary layer structure as observed by space-based lidar” by Luo et al. for publication in Atmospheric Chemistry and Physics.

The authors present an investigation of the marine boundary layer in the eastern Pacific, primarily during the MAGIC field campaign, but also extending to a 4 year (2006- 2010) climatology using the lidar on the CALIPSO satellite. They examine processes related to decoupling within the boundary layer resulting in a well-mixed layer within the deeper boundary layer structure. In addition to CALIPSO, they examine soundings from the field campaign, AIRS retrieved soundings, and estimates from ECMWF among others. Several issues with the analysis and description need to be addressed before publication in ACP.

Major comments

1) You specify that you do not use any cloudy CALIOP profiles in your estimation of BLH structure and this needs further clarification. Did you remove every profile affected by clouds or only those meeting some threshold? Did this include thin cirrus? How did you calculate MLH under these conditions?

Reply: In this paper, we used CALIOP profiles with no cloud below 8km, including cases with clouds above 8km. The MLH is calculated based on aerosol backscattering coefficient, which is not affected by the high clouds. Details were added into manuscript and relative statements in section 2 and section 3.2 were changed.

2) In your comparisons between MAGIC soundings and CALIOP, in order to be consistent, you should only examine clear-sky MAGIC soundings. Otherwise you are comparing different conditions. Also, you specify that the collocations are within

2.5 degrees, but you don't specify the time range. What temporal restrictions did you use for your collocations.

Reply: The evaluation of lidar methodology with soundings were performed with 2-year (2007-2008) clear-sky Atmospheric Radiation Measurement Program (ARM) Climate Research Facility (ACRF) radiosonde and micro pulse lidar (MPL) observations (Xie et al., 2010, Mather and Voyles, 2013) at Nauru (marine site). Compared to radiosonde-derived BLH, the bias and root mean square error (RMSE) of MPL derived BLH is -0.12 ± 0.24 km and correlation coefficient between each other is 0.75. Compared to radiosonde-derived MLH, the bias and RMSE of MPL derived MLH is -0.06 ± 0.16 km and correlation coefficient between each other is 0.66. Compared to radiosonde-derived MLH/BLH, the bias and RMSE of MPL derived MLH/BLH is -0.02 ± 0.1 and correlation coefficient between each other is 0.61. All the correlation coefficients are reported at confidence level of 0.01. These evaluations indicate the good accuracies of our lidar based BLH and MLH determinations for clear-sky MBL structure study. Relative statements were added into this section.

The MAGIC soundings are mostly under cloudy conditions and are poorly collocated with CALIPSO overpass. The temporal restriction for the collocation is 1 day. The purpose of the evaluations of CALIOP with MAGIC soundings was to show that the CALIOP-observed clear-sky MBL structure could be similar to the structure of the nearby cloudy-sky MBL in some extent. This is the basic assumption for discussion in section 4.2. Relative statements were added into this section and section 4.2.

3) In general, parts of the analysis stating good correlation would greatly benefit from the authors providing correlation coefficients and a measure of the statistical significance.

In multiple sections, the authors state that two things are well correlated, but show no quantitative evidence, other than an examination of the plots which can be subjective, especially since some of the plots are small and hard to read.

Reply: The correlation coefficients and a measure of the statistical significance were provided in the analysis now when discussing their agreements. The plots were improved according to referees' comments.

4) In the conclusions, you state that MBL decoupling is mainly controlled by EIS, but I don't think you've shown this. If that were the case, I'd expect to see a stronger relationship between the two. In a global analysis (i.e. not just over eastern Pacific), I would like to see the correlation coefficients and their significance and an explanation of the physical mechanisms and why other variables (e.g. SST) are not responsible before making this claim. Correlation does not equal cause and effect.

Reply: Now, the analysis was performed over the global ocean, and the correlation and statistical significance were provided to show the relationship between EIS and MBL decoupling.

The entrainment of the dry warm air above the inversion can be the main factor controlling the MBL decoupling. The EIS has a significant control of the MBL top entrainment. Because the SST, wind shear and surface heat flux also affect the entrainment process (Venzenten et al., 1999), it is reasonable to expect that these parameters also impact MBL decoupling. However, we did not find significant relationship between MBL decoupling and U_{10m} or SST. Instead, we found that the MBL coupling structure is controlled by both LTS and EIS when $EIS < \sim 3$ K, that is, better mixed MBL with increasing of EIS and decreasing of LTS. One reason for a weak relationship with U_{10m} or SST could be the uncertainties in satellite retrievals of these parameters. Another reason could be that the role of other variables was partially included by the EIS and LTS. Statements were changed to reflect the above discussion.

5) Please proofread the English grammar more carefully.

Reply: We put efforts on the language and the paper was proofread by the OSA Language-Editing Service. The manuscript is improved now.

Specific comments:

1) The title implies that this analysis relies primarily upon space-based lidar when much of the analysis relies on data from other instruments. In section 3.2, it is stated that only BLH estimated from clear-sky CALIOP profiles are used. Considering

that much of the analysis concerns the presence of stratocumulus and cumulus clouds, conditions in which CALIOP is not used, the title is misleading and should be changed to reflect the broader array of instrumentation used.

Reply: The title was changed to 'Marine Boundary Layer Structure as Observed by A-Train Satellites'.

2) Page 34066, line 19: “However, over oceans, the BLH is associated with the aerosol layer top (clear sky) or stratiform cloud top (cloudy sky), : : :” How does this differ from BLH and MLH over land?

Reply: As shown in Luo et al. (2014a), over land, the gradient or variance methods could identify the BLH, while aerosol layer top is usually higher than the BLH. The statement was added.

3) Page 34066, line 20: the word below is misspelled

Reply: Corrected.

4) Page 34066, line 21: You state that there is a strong aerosol gradient at the top of the MLH. Is this not present also at the BLH top? If not, how does your threshold method detect the BLH?

Reply: The lidar methodology were developed and evaluated with the 2-year (2007-2008) clear-sky Atmospheric Radiation Measurement Program (ARM) Climate Research Facility (ACRF) radiosonde and micro pulse lidar (MPL) observations (Xie et al., 2010, Mather and Voyles, 2013) at Nauru (marine site). As detailed in Luo et al. (2014a), over ocean, the MBL top (BLH) is usually at the aerosol layer top, and the MLH is usually close to the height with a strong aerosol gradient below BLH. For decoupled MBL, the aerosol gradient at the BLH top is weaker than that at the MLH top, as a case shown in Figure 4 in Luo et al. (2014a). For well-mixed MBL, the BLH and MLH are same.

5) Page 34066, line 25: “Difficulties in identifying stratiform clouds: : :” Do you mean difficulties in differentiating between cumulus and stratiform clouds?

Reply: Changed to 'differentiating between cumulus and stratiform clouds'.

6) Page 34067, lines 17-19: The phrasing here is awkward. Please reword. Please also specify that the 532nm polarized backscatter is both perpendicular and parallel polarizations.

Reply: Rephrased to ‘CALIOP is a dual-wavelength (532 and 1064 nm) backscatter lidar, which is carried on the Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO) (Winker et al., 2007; Winker et al., 2009). At 532 nm, the CALIOP provides both the parallel and perpendicular polarization components of attenuated backscatter.’

7) Page 34067, line 23: You need to introduce CloudSat (and later AMSR-E) and provide references.

Reply: Information of CloudSat and AMSR-E was added as suggested.

8)Page 34068, lines 9-11: The explanation of EIS could be clearer. Perhaps including the equation would be helpful. Also, what level moist adiabatic lapse rate did you use?

Reply: Equation was added. The moist adiabatic lapse rate is at 850hpa.

9) Page 34068, line 8: The text says you use the surface potential temperature, but the equation here implies 1000 hPa potential temperature. Which did you use?

Reply: The equation was changed to match the text.

10) Page 34068, lines 25-26: How do you differentiate between the AMSR-E passes in your analysis; which did you use? What is the timescale of these maps? Did you use daily data, monthly, etc.?

Reply: We used both ascending and descending passes for daytime and nighttime orbits. The AMSR-E passes were differentiated based on the geo-locations and observation times provided in this dataset. AMSR-E Level 3 daily Ocean Products version-7 was used. Information was added.

11) Page 34068: Specify that AMSR-E is onboard Aqua and therefore is collocated with AIRS and AMSU and in the A-train with CALIPSO and thus the instruments are sampling similar conditions and time of day.

Reply: Statements were added as suggested.

12) Page 34068, lines 26-27: You specify the RMS differences, but don't say relative to what. This information is necessary for understanding the associated uncertainty.

Reply: Statements were changed to 'Error in the data was estimated using the root mean square (RMS) difference between AMSR-E U_{10m} and U_{10m} coming from four other satellite microwave radiometers (three SSM/Is and TRMM TMI) and with U_{10m} from the satellite microwave scatterometer QuikScat (0.92 m/s with a bias of 0.57 m/s) (Wentz et al., 2003). This calculation gave an RMS difference between AMSR-E SST retrievals and the Reynolds SST as 0.76 K (Wentz et al., 2003). Validation using data from a buoy (National Data Buoy Center, NDBC) U_{10m} (mean value of 6.61 m/s) gave an RMS difference with AMSR-E U_{10m} (mean value of 6.46 m/s) is 1.63 m/s with a bias of -0.15 m/s (Luo et al., 2015). Validation with NDBC buoy SST (mean value of 299.49 K) in this study showed that the RMS difference in AMSR-E SST (mean value of 299.26 K) is 0.99 K with a bias of -0.23 K. '

13) Page 34069, line 1: Earlier, you state that AMSR-E data are on a 0.25 degree map, but here you say 25km. Which resolution is used?

Reply: Changed from 25km to 0.25 degree.

14) Page 34069, line 7: Spell out GPCI

Reply: Added.

15) Page 34070, lines 6-10: If the HSRL signal is negatively impacted by cloud cover and much of the transect included clouds, what uncertainty is present in your aerosol distribution estimate?

Reply: The HSRL signal was only used to show the aerosol and cloud vertical distribution along the transect. The HSRL can detect the aerosol signal below clouds because it is looking upward from ship. As can be seen in Figure 1 when western than longitude of $\sim 140^\circ$, there is a layer below clouds (if exist) with more aerosol loading and with the layer top close to the MLH identified from radiosonde. Due to the high cloud occurrence along MAGIC transect, we did not apply the lidar methodology to HSRL clear-sky profiles.

16) Page 34070, line 12: Why is sonde in all capital letters and why is radiosonde or rawinsonde not spelled out?

Reply: Changed 'SONDE' to 'radiosonde' throughout the manuscript.

17) Page 34070, line 22: Did you determine the method based on the structure? Please clarify or reword. Was the MLH detected by radiosonde or the lidar? If it was the radiosonde, how was this determined?

Reply: Statement was removed.

The MLH was detected with both radiosonde and the lidar.

For radiosonde, MLH height was identified as the base of the first inversion layer with inversion strength larger than 0.05K/100m in radiosonde potential temperature profiles.

Statements were changed to make it clear.

18) Page 34070, line 23: "Figure 1 presents the one transect: : :" was there only one transect?

Reply: There are total 40 transects in MAGIC field campaign. Figure 1 presents one transect.

19) Page 34070, line 24: Temperature is misspelled. Please use spell check.

Reply: Corrected.

20) Page 34070, lines 24-25: What is the correlation between the BLH and aerosol

layer/Sc top?

Reply: The correlation coefficient between the radiosonde BLH and aerosol layer top is 0.75 for clear-sky observations (at the ARM Nauru cite), and is 0.56 for loosely collocated clear-sky CALIOP and all-sky MAGIC observations. The correlation coefficient between the CALIOP-derived BLH and Sc top is 0.66. All correlation coefficients are at confident level of 0.01. Statements were added into this section.

21) Page 34070, lines 22-26: How close is the spatiotemporal location of the soundings with HSRL?

Reply: The soundings and HSRL were all deployed on the commercial cargo container ship Horizon Spirit in MAGIC field campaign.

22) Page 34071, line 2 and Fig. 1: It is difficult to see the inversion associated with the MLH described in the text in this figure. It would be helpful to see one profile of theta with the associated TAB from HSRL.

Reply: The figure can be found in Figure 4 of Luo et al. (2014a) with ground-based lidar and radiosonde observations. Statement was added.

23) Page 34071, line 8: How was the 0.05K/100m threshold identified?

Reply: The threshold was chosen based on visual check through all MAGIC transects. Statement was added to clarify it.

24) Fig 1: What does it mean when the MLH in Fig. 1 is at the surface. To my eye, it looks like MLH=BLH east of longitude 137 degrees W.

Reply: The lower limit of the y-axis is 0.3km. The radiosonde-derived MLHs are ~0.3km due to weak inversion above this height when east of longitude 137 degrees.

25) Page 34071, line 23: Since the sonde data are not the truth, do not say that the CALIOP derived BLH are biased lower. Just say that they are lower. And this is not true over the eastern domain.

Reply: 'biased' was removed.

26) Page 34071, lines 25-26: How large is the spatial mismatch? What is the temporal mismatch? How many observations go into both estimates? A better comparison would limit the radiosonde data to clear-sky only so they are more comparable to the CALIPSO estimates.

Reply: We used quite loose constrains (1-day and 2.5 degree) to find collocated MAGIC and CALIOP measurements. However, the statistical results showed that the CALIOP-observed clear-sky MBL structure could capture a similar structure of the nearby cloudy-sky MBL.

The clear-sky only evaluation of the lidar methodology was added by using the 2-year (2007-2008) collocated clear-sky Atmospheric Radiation Measurement Program (ARM) Climate Research Facility (ACRF) radiosonde and micro pulse lidar (MPL) observations (Xie et al., 2010, Mather and Voyles, 2013) at Nauru (marine site).

27) Page 34072, lines 1-4: Why would you expect a cloud-free regime and a cloudy regime to have similar BLH? And if the spatiotemporal mismatch is very large, such that one is not representative of the other, why is there good agreement?

Reply: Even though there are differences between a cloud-free regime and a cloudy region, but during MAGIC, we expect the both regimes show a consistent picture of MBL transition from the coastal to the far ocean. That is why they agree statistically but with small mean differences.

28) Page 34071, line 6: indicates is misspelled. Please use spell check.

Reply: Corrected.

29) Page 34071, line 6: LCL is shown in Fig. 2c, not 2b.

Reply: Corrected.

30) Page 34071, lines 10-12: What are the correlation coefficients for these comparisons?

Are they statistically significant? For instance, the BLH seems to be an average of 1-1.5 km so an error of 25-40% seems large. What is your justification for this being a

good fit?

Reply: Those were compared between CALIOP cloud-free cases with MAGIC cloudy cases based on loosely collocated data, which results in large random errors in the comparison. As mentioned above, data from the both regimes show a consistent trend. As given in the reply to your major comment 2, the comparison based on collocated cloud-free cases at an ARM tropical site was given to more reliably to show the performance of the algorithm.

31) Page 34072, line 16: How is the MBLC dataset built?

Reply: The MBLC dataset includes cloud type and stratiform-cloud top based on 2B-CLDCLDCLASS-LIDAR, and drizzle information based on 1B-CPR dataset as described in the previous section. The statement was changed to ‘In this section, we will investigate the MBL structure over the eastern Pacific Ocean with the 4-year new MBL and marine boundary layer cloud (MBLC, including cloud type and stratiform-cloud top based on 2B-CLDCLDCLASS-LIDAR, and drizzle information based on 1B-CPR) dataset as described in the previous section.’

32) Page 34072, line 24: Cold SST could also be partly responsible. If it was due solely to subsidence, the BLH would be low over the deserts.

Reply: Changed to ‘due to the strong subsidence and cold SST’.

33) Page 34072, line 26: How do you determine the clear-sky BLH when convection is occurring? Usually convection is associated with clouds.

Reply: The SST over the ITCZ region is high, which can result in stronger buoyancy-driven turbulence and thus stronger vertical mixing. Statement was changed to ‘The BLH is high over the Intertropical Convergence Zone (ITCZ) due to large-scale convergence and the high SST, which results in strong buoyancy-driven vertical turbulence mixing’ to make it clear.

34) Page 34073, lines 7-11: Please rephrase. First you say there are magnitude

differences and then you provide an example where you say that McGrath-Spangler and Denning give a similar pattern and value. These two statements are inconsistent.

Reply: The BLH reported in McGrath-Spangler and Denning (2013) show a quite similar pattern and value to the MLH in our results, which means their BLH is much lower than the BLH in our results. Statements were changed to clear this point.

35) Page 34073, line 17: There is a minimum in MLH/BLH over the equator, implying weaker mixing from 160W to 100W

Reply: Statement was added.

36) Page 34073, lines 25-26: This is repetitive

Reply: Deleted.

37) Page 34073, lines 19-29: If clear-sky is included, how do you define a CTH in the case without clouds?

Reply: Within the collocated 0.25° grid-box, there are many cases with the part of grid-box covered by stratiform clouds and the rest being clear sky. In these cases, both BLH and CTH are available and good correlation cloud be found between the BLH and stratiform CTH as shown section 3.2. Statement was added to clarify it.

38) Page 34073, line 27: What cloud types are away from the coast with tops < 2.5km?

Please state to be consistent with your statement about stratiform clouds in the previous line.

Reply: ‘the drizzled stratiform cloud tops’ was added here.

39) Page 34074, line 1: Specify what you mean by “approaching the central Tropical Pacific”

Reply: Changed to the tropic Pacific near longitude of $\sim 180^\circ$ W.

40) Page 34074, lines 6-7: Are these stratiform clouds included in Fig. 3a?

Reply: The BLH and MLH were identified from the clear-sky CALIOP profiles in the collocated 0.25° grid-box. Therefore, Fig. 3a includes all cases with cloud fraction <1 .

41) Page 34074, lines 8-12: Reword to make clear that heterogeneous conditions are less likely to precipitate than more homogeneously cloudy ones.

Reply: Reworded as suggested.

42) Page 34074, line29: When you state something has a good positive correlation, please include the correlation coefficient and the significance.

Reply: The optical depth in the lower well-mixed layer has the correlation coefficient of 0.64 at confident level of 0.01 with the U_{10m} in NPO, but the correlation coefficient is -0.08 at confident level of 0.39 in the SPO. The statement was added.

43) Page 34075: There appears to be a seasonal cycle in the near coastal aerosol concentrations in the SPO transect. Can you comment on this?

Reply: The seasonal cycle in the near coastal aerosols is mainly driven by seasonal sea salt source (link with wind) and MLH and BLH variations. However, the near coastal aerosol can still be influenced by the continental aerosol sources.

44) Page 34075, line 12: “Sc occurrence and EIS correlate well: : :” this is not true near the coast. What other processes could be involved?

Reply: Other processes such as sea-land breeze and cold current near the coasts of Northern and Southern America resulting cold SST could be involved. Statements were added.

45) Page 34075, line 13: If drizzle occurrence isn’t discussed other than to say it isn’t correlated with EIS, why show it in this plot?

Reply: Removed from the plot.

46) Page 34075, line 25: This doesn’t seem to be the case for NPO during DJF. Do

you have any explanation for the seasonal dependence?

Reply: We further investigated the possible controlling factors to the $CTH_{no\ drizzle}/CTH_{drizzle}$ beyond EIS over global oceans. The SST was found to be important. Under cold SST ($SST < \sim 20^{\circ}C$), mean $CTH_{no\ drizzle}/CTH_{drizzle}$ does not vary with EIS. Under warm SST ($SST > \sim 20^{\circ}C$) where the Sc-to-Cu transition happens, the mean $CTH_{no\ drizzle}/CTH_{drizzle}$ shows good correlation with EIS, with the correlation coefficient > 0.89 at confident level of 0.01. The near-coast (eastern than ~ 130) SST for NPO during DJF is close to or colder than $20^{\circ}C$. Therefore, it seems no good relationship between EIS and $CTH_{no\ drizzle}/CTH_{drizzle}$ for this season.

Figure 6(c) and relative statements were changed.

47) Page 34076, lines 17-18: Please rephrase for clarity.

Reply: Rephrased as ‘The $CTH_{no\ drizzle}$ is close to the BLH but is lower than the $CTH_{drizzle}$.’

48) Page 34077, lines 19-21: If Sc represent more than 1/2 of the grid box and CALIOP has a small footprint, why would clear-sky MBL be represented in the average?

Reply: According to the evaluations of CALIOP-derived clear-sky MBL structure with MAGIC cloudy-sky MBL structure and the evaluation of CALIOP-derived clear-sky BLH with the nearby stratiform cloud top from 2B-CLDCLASS-LIDAR in section 3.2, it is reasonable to assume that the cloud-topped MBL can have the similar structure to the nearby clear-sky MBL within the 0.25° footprint for the Sc and Cu MBL cases. Statements were added.

49) Fig. 7: In Section 3.2, you say you cannot use CALIOP to estimate BL structure in cloudy conditions so what is your justification for using it to show TAB under stratiform and Cu conditions?

Reply: Figure 7 is the mean BL structure in term of TAB from the cases with both clear-sky and stratiform/Cu cloud in the same 0.25° grid box. The definitions of the stratiform and Cu cloud conditions were in this section and in the caption of the figure. Relative

statements were revised to make it clear.

50) Fig. 1 caption: The diamonds and circles are black, not magenta.

Reply: Corrected.

51) Fig. 2: Instead of showing the 1:1 line, it would be helpful to show a best fit line or lines.

Reply: Due to uneven data samples the best-fit line may be misleading. Thus, we only provide related statistics in the text. The 1:1 line offers a simple way to assess differences between different measurements.

52) Fig. 3: I don't believe Fig. 3h (u10m) is discussed. If not, please remove.

Reply: Removed as suggested.

53) Fig 4: For consistency with Fig. 5 and to make it easier to determine proximity to the coast, change the x-axis to show longitude.

Reply: Changed as suggested.

54) Fig. 6: (1) Are these scatter plots? If so, please remove the connecting lines. (2) What instrument is used to calculate the MLH/BLH here (6b)? (3) Please label the y-axis in Fig. 6c.

Reply: (1) The lines were kept to make the trends in plots more clear.

(2) It is CALIOP-derived MLH/BLH in (6b). A statement was added to clarify it.

(3) Added.

55) Fig. 7: (1) Are these associated with a particular season or the 4 year climatology? (2) How many profiles are averaged in each? Are any cases better or worse sampled?

(3) If you're including BLH up to 1.6 km in Fig. 7c, extend the y-axis to include these values.

(4) Please include symbols to indicate the estimated BLH and MLH so your point is

clear about decoupling between the two with varying EIS and BLH.

Reply: (1) These are the 4-year climatology. A statement was added to clarify it.

(2) The total case numbers in each plot are (a1) 7019; (a2) 903; (a3) 3050; (b1) 9911; (b2) 1595; (b3) 3900; (c1) 11166; (c2) 1714; (c3) 4249. The number of stratiform cloud case is smaller than other two cases, but is still large enough to support the statistical analysis (70-400 cases were averaged for each stratiform cloud case profile).

(3) The y-axis is extended to 2km.

(4) We tried to add symbols but the plots become too complicated. Also, these are mean profiles from individual profiles with different MLH. The BLH for (a1-a3) is ~ 0.7km, for (b1-b3) is ~1.1km, and for (c1-c3) is ~1.5km. The MLH varies profile by profile from ~0.5 to ~0.9km.

56) Your acknowledgements need to include the instrument teams and data providers.

For instance, if you acquired the CALIPSO data from the ASDC, they provide a sample acknowledgement (available from <https://eosweb.larc.nasa.gov/citing-asdc-data>): Citing ASDC Data Acknowledgments The data obtained from the Atmospheric Science Data Center (ASDC) are free of charge for use in research, publications, and commercial applications. When data from the ASDC are used in a publication, we request this acknowledgment be included: "These data were obtained from the NASA Langley Research Center Atmospheric Science Data Center."

A lot of work goes into collecting and archiving the data that you used and this effort needs to be acknowledged. Additional acknowledgements are required for each of the datasets you used. Most data providers also provide a sample acknowledgement.

Reply: Acknowledgements were revised as suggested.