

## ***Interactive comment on “Planetary Boundary Layer variability over New Delhi, India, during EUCAARI project” by Konstantina Nakoudi et al.***

**Anonymous Referee #3**

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### **General comments:**

This paper presents a study on the planetary boundary layer height (PBLH) based on lidar measurements collected in New Delhi, a populous city facing severe air pollution. The detection of PBLH based on lidar data is performed using the Wavelet Covariance Transform (WCT) method, which is developed by Baars et al., 2008. The derived PBLH is then compared with the results obtained from other sources, i.e. radiosonde, CALIPSO and models (ECMWF and WRF). The authors did a lot of work in the data treatment, comparison and statistical study. The content of this paper generally falls into the scope of AMT journal, but major modifications are still needed.

This paper looks more like a report, but not like a scientific article. The authors present mostly what has been processed and some obvious results according to it. It is not

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well structured and the content is not concise. The authors need to re-organize the materials and improve the manuscript to better support the topics they want to present in this paper.

### **Particular comments:**

The application of WCT method is one the main topics of this paper. The first two case studies show satisfactory performance of the WCT method. However, there is not an overall comparison of PBLH between lidar PBLH detection and model estimation, although that exists for radiosonde (Figure 6) and CALIPSO product (Figure 7). The radiosonde measurement is very sparse and with low resolution, and the CALIPSO data are too few. So I consider that comparison of PBLH between lidar detection and model estimation should be of more importance, but it is not shown.

There is a lengthy comparison between lidar PBLH detection and other datasets. Maybe the authors are trying to 'grab' too many materials: radiosonde, CALIPSO product, ECMWF and WRF, as a result the 'story' becomes complicated. As the authors have mentioned in the manuscript, the definition of PBLH in different data sources is different by nature. Apart from the definition of PBLH, the comparison is also limited by other factors such as the temporal, spatial resolution of the data sources, data abundance and the spatial displacement. Comparing these results will inevitably bring some disagreements and also agreements. I think it is not necessary to show all of them, just select one or two datasets to compare and it will simplify the organization of this paper. For example, when comparing the T,  $\theta$  and RH profile, radiosonde and WRF data are used; when comparing the diurnal evolution, lidar and ECMWF data are used, what is the purpose of involving the two models? Too many comparisons are made but some of them lack constructive conclusions.

Moreover, the manuscript needs to be strongly improved. The authors should avoid excessive details in the manuscript. Main messages, not all the details contained in the figures or tables should be addressed in the text. For example, numbers in 'data coverage' section do not carry so important meaning and do not need so many

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words. The figures showing the T,  $\theta$  and RH profiles, contain repeating information with Table 2 and Table 3, maybe only one such figure is enough...Please highlight the important and relevant information. The abstract and conclusion should be re-written after corrections of the content. The conclusion needs to be more conclusive and there is not need to go into details or repeat what has been said.

Additionally, I miss more scientific meaning of this work. The PBLH is certainly important for studying the atmospheric process in the boundary layer and for the air quality model or weather model. Such information will add value to this paper.

#### Other comments:

- P3, L85: Please check the latitude and longitude of the two stations.
- P3, L93-108: This paragraph presents very detailed information about the meteorological conditions of the observation site, but I think the messages that authors have presented are not well selected and structured. Details will bring details that need to be well commented (ex. differences between your measurements and WMO data), making the readers lost. The authors could shorten this paragraph and present the information that serves the topics and results that will be present in the following sections.
- P3-P4: I think the lidar system deserves more 'words', even it has been presented in other papers. Main messages should be addressed to the readers:
  - Elastic and Raman wavelengths
  - Telescope, divergence, overlap. . .
  - Data quality

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- P4, L145: "A certain range of signal is cut to avoid strong gradients in the lower range", so how do you choose the range to be cut? Is it automatic?
- P4, L152: Comment the abbreviation 'L-R'; and the paper "Wandinger and Ansmann, 2002" is not included in the bibliography.
- P5: The Subsection '3.1.3 Data coverage' is very 'dry' and not so interesting. It does provide some information, but it is too detailed with unnecessary technical issues, and some numbers are not well defined so I got confused and sometimes had to redo these simple calculations. I think this paragraph could be shortened.
- P8, L203: And in Figure 3, Please comment the PBLH close to 0 m, why do the models produce such low values? There are two T profiles in the figure at right, is it a mistake? What are the white squares in Figure 3 upper panel?
- P9, L311: I miss a clear and quantitative difference between the PBL and RL. In Figure 3, there is a noticeable decrease of lidar signal at 1000-1200 m, do you consider it as the residual layer? Please also check the color scale of Figure 3. I saw a clear discontinuity at 06:00 UTC, 12:00 UTC and 17:30 UTC, are the lidar signals plotted with the same color scale? Moreover, the width of the black zone near the surface in Figure 3 is also changing, what does it mean? The authors should be more careful in preparing scientific figures.
- P9, L315: PBLH=435 m is not found in Figure 3, check the text, table 2 and Figure 3.
- P9, L333, define PBL cycle, when does this cycle start and end?
- P10, L354: which data did you use to derive this 553m/h? WRF?
- Figure 4, and Figure 5: Again, check the color scale please. The discontinuity is quite obvious.

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- P10, Subsection 4.2.2: The data points are so few, and I do not think it is enough for a statistical study. “Based on the analyzed cases, it was found that the overpass distance (here 20 and 101 km) from the lidar station and time difference between the measurements did not affect the PBL heights. ” This conclusion does not convince me, because the dataset is so small and cannot represent the spatial and temporal variability.
- P11: PBLH diurnal Cycle might be more specific than PBL diurnal Cycle, because the authors investigated only the PBL height, not other parameters in the PBL.
- P12, L426: why is the comparison made only between lidar detection and ECMWF, how about WRF?
- P14, Section 5: not relevant and too short to be a section
- P15 conclusion: this section is long and not conclusive, and it is repeating what have been said previously.

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