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Atmospheric Measurement Techniques Discussions

Interactive comment on "On the estimation of boundary layer heights: A machine learning approach" *by* Raghavendra Krishnamurthy et al.

Anonymous Referee #2

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Review of the article titled "On the estimation of boundary layer heights: a machine learning approach" by Krishnamurthy and coauthors for publication in the Atmospheric Measurement Technique.

The authors have used a machine learning (ML) approach to improve the retrieval of boundary layer depth from the data collected by the Doppler Lidar. They first develop a ML model to calibrate the DL retrieved PBL depth with that derived from the radiosondes. As the radiosonde measurements are temporally sparse, they use the higher resolution PBL depth retrieved from the Doppler Lidar to understand boundary layer parameters affecting it. In the end they also evaluate two days of output from two different models. The article is overall well-written and is easy to follow. However, the article can be further improved by addressing the following concerns. These can be regarded as minor revisions.

Major Concerns:

It will be good to add some discussion in the last section on the use of machine learning in deducing PBL depth, and understanding its controls. The authors have mentioned and acknowledged several things in the text, i) like the training could have been performed by using a different estimate of PBL depth from the radiosonde, and ii) how the authors are only demonstrating the use of ML for deriving the PBL in the nighttime, but refrain to call it the “true” nighttime Zi (Line 313-315). This is simple the limitation of the use of ML in deriving physical understanding. This should be discussed in the text in detail. If the authors truly believe (#2 above) to be the case, then can you trust the numbers reported in Table 4 and 5? Maybe the Tucker method is correct and just the training needs to be done on a different dataset. This concern does not mean that the article is not valuable, however this needs to be addressed in the text. Thank you.

Figure 10 and associated text: it is a bit confusing as to the whole purpose of this exercise. Just because the variance is being scaled by a higher PBL depth, the profile will look different. So not sure how it speaks to the Random Forest (RF) PBL depth being better than that derived by the Tucker method. Also, the variability of variance is probably huge, so the differences wouldn’t be statistically significant anyways. This needs to be clarified in the text, or else removed from the manuscript. Thanks.

Minor Concerns:

Line 14: Might be better to say four years rather than multi-year. Thanks.

Line 41: MISR is mis-spelled.

Line 42:43: The satellites measure cloud top temperature from which the cloud top heights are calculated. During cloudy conditions, it is assumed that the PBL top corresponds to cloud top heights. This statement states that there has not been any validation of the satellite derived cloud top heights. Please add reference to support

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this, or else remove. Thanks.

Line 58: you mean rely and not relay.

Line 64: better word would be “lowest gate” rather than minimum range.

Table 1: It will be good if you add units to the measurement features. Thanks.

Line 145-146: please revise this sentence. Thanks.

Line 165: The numbers do not add up. Four years of data should equal 1460 days, not sure how you got 1785 days.

Line 285: you mean “hourly” cloud fraction greater than 0.1?

Figure 8: Please describe the vertical bars in the caption.

Figure 11: Looks like the LASSO simulations are able to accurately capture the development of the daytime PBL. I assume that the E3SM values are within the model range resolution as well. So this is very good news for the modelling community and should be highlighted.

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