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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A comprehensive investigation on afternoon–evening transition of the atmospheric boundary layer over a tropical rural site" *by* A. Sandeep et al.

Anonymous Referee #1

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General Comments.

The paper is based on boundary-layer measurements over a tropical rural site. The aim is to investigate afternoon-evening transition (AET). The authors present a detailed analysis of the phenomena on the basis of in-situ and remote sensing devices. The paper has certainly scientific interest to justify publication of these results. However, I have some concerns about the present version of the paper, and I believe that a careful revision of the paper is needed. I explain these issues in more detail below. I would encourage the authors to improve the English of the manuscript and re-submit.

Specific comments.

Pag. 10, lines 1-2. To my knowledge, the water vapour mixing ratio (r) and the specific humidity generally decrease at night due to vapour condensation. Is the strong increase of r measured at the surface around 14 IST typical of tropical sites?

Figure 1e-h. The height coverage of the Sodar used by the authors is 0.03-1.5 km, but the maximum height where SNR is depicted is below 600-700 m also during the daytime, when strong convection is present up to 3 km (Figure 1i-l). Why?

Pag. 10, line 8. Why does not the Sodar SNR signal show any increase at midday? The authors say that Figure 1e depicts an increase of SNR, but I do not see that increase.

Pag. 10, line 14. What do the authors mean with "horizontally stratified" ?

Pag. 11, lines 11-12. The authors say that "Though the temperature decrement starts little early, but is not consistent and also weak in magnitude". In my opinion, Figure 2a shows a clear decrease of temperature starting from 1510 IST, with a rate of nearly 1 °C per hour, which is of the same order of the rate used by the authors as one of the criteria to identify transition (0.5 °C in 30 min, see pag. 13).

Pag. 11, lines 12-14. The mixing ratio grows suddenly also at nearly 1510 IST. It is not clear the criterium used by the authors to identify transition. Their choice seems to me somewhat arbitrary and questionable.

Pag. 11, lines 16-17. The time history of the wind variance is highly variable and shows several isolated peaks during the afternoon. Can the authors explain the reason why those peaks occur? I suggest to add the time history of the wind direction, it could help in identifying the passage of different air masses both near the surface and at elevated levels. Maybe, changes in r could be due to the different nature of such air masses. Furthermore, the vertical profile of wind direction might explain why the AET follows a top-to-bottom evolution.

Pag 11, line 22. The SNR at z=450 m decreases well before 16 IST. Furthermore,

at z=300 m SNR is highly variable. I think that is not possible to give any definite conclusion based on the sodar SNR.

Pag. 13, lines 1-7. Are the criteria listed by the authors appropriate only for tropical sites? In other words, do they believe those criteria can be used in other contexts (non-tropical sites)?

Pag 14, lines 18-20. Again, I think the authors must add in their analysis the time history of the wind direction taken at several altitudes in order to check the possible presence of different air masses along the vertical, in particular during the AET.

Pag. 19, line 11. The sensible heat flux is in the range 0.15-0.25 Kmsˆ-1, and not 1.5-2.5.

End of pag. 19 and beginning of pag. 20. The authors try to explain the top-to-bottom nature of the AET on the basis of the values assumed by the entrainment ratio and the entrainment flux, but, in my opinion, their explanation is not very convincing and, at the same time, is very questionable. I would suggest to the authors to weaken their conclusions.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 31483, 2014.