Replies to reviewer 1 comments

At the outset we would like to thank the reviewer for his encouraging comments and useful suggestions.

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.
Reply: As per reviewers' suggestion, we tried our best to minimize the grammatical mistakes in the revised manuscript. Further, we have implemented all the suggestions given by the reviewer in the revised manuscript.

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?
Reply: Yes. It is a typical phenomenon in the Tropics. It is mainly due to the continuous decrease of moisture transport into the mixed layer from the surface. Since most of the moisture sources are at the surface, the continuous waning of moisture transport by turbulence (thermals) increases the moisture at the surface. The vapor condensation does occur sometimes, but not during the evening transition. It generally occurs in the early morning during the winter period.

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?
Reply: Although the maximum height for SODAR is fixed as 1.5 km, useful data comes from a height region of 30 m to ~600/800 m, depending on atmospheric conditions. Since the SODAR uses acoustic pulses, which generally gets attenuated quickly in the atmosphere, the height coverage is limited even in the presence of convection (during the day time).

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.
Reply: We agree that in the case presented here, the SNR enhancement is not significant. But it is clearly seen in many other cases. We, therefore, changed the text in the manuscript accordingly. However, the enhancement is clearly seen in Sodar spectral width and profiler SNR and spectral width images.

Pag. 10, line 14. What do the authors mean with "horizontally stratified" ?
Reply: What we mean by that statement is "The small-scale fluctuations in the SNR and spectral width primarily caused by the daytime turbulence are reduced during the nighttime. Both SNR and spectral width variations with time are small". To avoid confusion, we removed that part of

the sentence and now the sentence read as "Nevertheless, about 2 h before the sunset, both the intensity and vertical extent of thermals start to decrease continuously till the sunset occurs."

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^{o}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^{o}C in 30 min, see pag. 13).
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Pag. 11, lines 12-14. The mixing ratio grows suddenly also at nearly 1510 IST. It is not clear the criterion used by the authors to identify transition. Their choice seems to me somewhat arbitrary and questionable.
Reply: We agree with the reviewer that the temperature decrement or mixing ratio increment starts at 1510 IST. But this decrement in 'T'/increment in 'r' is not consistent and the magnitude of decrement/increment is also small. As per our definition, the start time of evening transition is the time at which atmospheric state variables show large and persistent increase/decrease (i.e., the increase/decrease should be significant and should persist for at least an hour). We examined the 19 cases, for which we have the data from all our instruments, and identified the start time of transition in each parameter manually. We then estimated the gradients in each parameter and finally fixed the thresholds based on these gradients. Later a sensitivity analysis is carried out to know the impact of the chosen thresholds on $Trans_{sunset}$ as obtained by different state variables at different altitudes. We found that the results do not change much even if we vary each threshold by ±20%. This new figure (shown for reviewers' reference) and the above information are included in the revised manuscript.

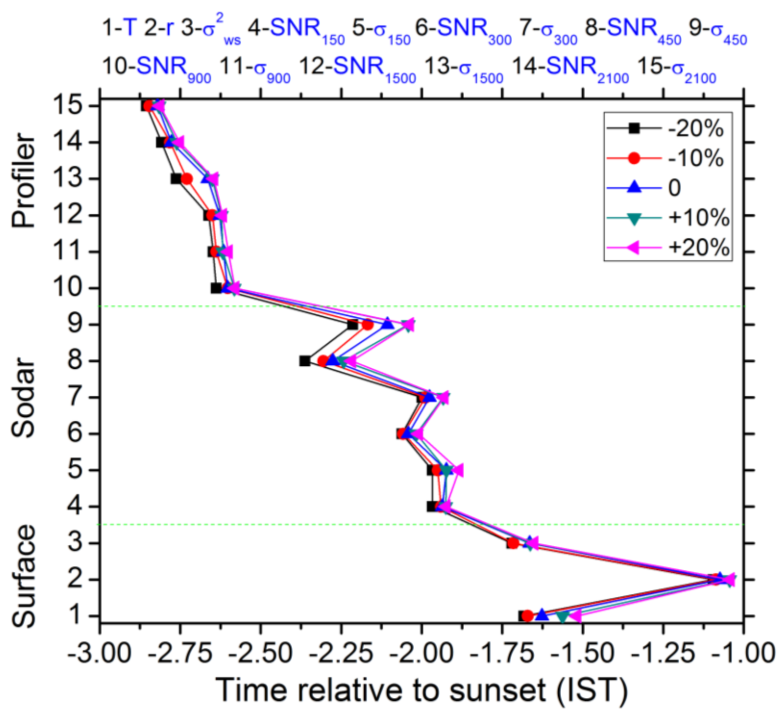

Figure: $Trans_{sunset}$ identified by different instruments employing a variety of atmospheric state variables by varying the thresholds, indicating the sensitivity of $Trans_{sunset}$ on the threshold.

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.

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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.

Reply: Since Gadanki is located in a complex terrain, sudden gusts will increase the wind variance. As correctly pointed out by the reviewer that it is difficult to identify the transitions from such noisy temporal variations. In fact, this is one of the reasons for not recommending variance as a parameter to identify the transition.

Yes. We agree with the reviewer that different air masses at different times or at different altitudes can cause gradients in the atmospheric state parameters or can give a clue to top-to-bottom evolution of transition. As suggested by the reviewer, time-height variation of wind direction is added to Figure 1. It clearly shows that the wind direction remained same (northeasterly) for about 3-4 hours during the transition, i.e., 2 hours before and after the sunset, at all heights below 1.5 km. It clearly indicates that the increase/decrease in atmospheric state variables is due to the evening transition rather than the passage of different air masses. Also, as we do not see any appreciable change in the wind direction at different altitudes before the sunset (it remained same at around 130°), we can rule out the possibility of different air masses causing the top-to-bottom evolution of the evening transition. This information is included in the revised manuscript.

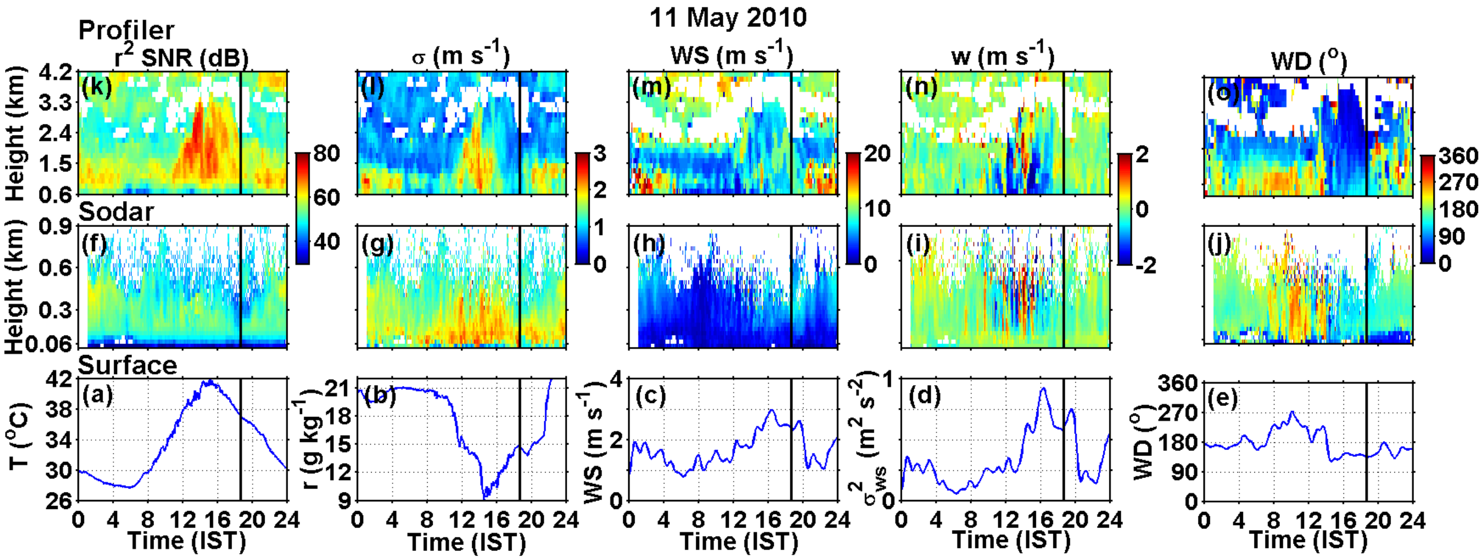

Figure: Time height variation of different atmospheric state variables on 11 May 2010, indicating their diurnal variation.

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.

Reply: The variations in both SNR and spectral width during the daytime are primarily caused by thermals. Though the strength of thermals continuously decrease during the transition, they do cause enhancements in SNR and spectral width (see Figures 1l and 1g). As a result, we see enhancements in SNR and spectral width (albeit small) overlaid on a general decreasing trend during the transition. While choosing the thresholds, these complex variations are considered.

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)?

Reply: As mentioned above, some of the variations in state variables are typical of tropical sites. Though other variables can be used at non-tropical sites, some tuning of the thresholds may be required. This information is given in the revised manuscript.

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

Reply: Sorry for our mistake. It is now corrected in the revised manuscript.

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.

Reply: The top-to-bottom evolution of the AET is very complex. In fact in the revised version, we have added the advection term to the entrainment flux. Still the conclusions do not change much.

The present study clearly demonstrates that the AET follow the top-to-bottom evolution, and gives an explanation for such an evolution. Keeping the complexity in the problem and reviewers' suggestion in mind, we have diluted our conclusions on top-to-bottom evolution in the revised manuscript.