

Replies to reviewer 2 comments/suggestions

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

The discussion paper presents a variety of observations of afternoon transitions. The observations are of good quality. Analysis at a tropical site adds something to the literature, which mostly has looked at mid-latitude situations. I think the paper could be a useful addition to the literature, but some aspects of the presentation need to be improved before publication.

Reply: We thank the reviewer for his positive comments. We wish to inform that all suggestions given by the reviewer were considered and incorporated in the revised manuscript.

General comments:

1. The paper introduces a new term, "afternoon-evening transition." Transition terminology is already confusing enough. Please choose a term from the Lothon et al. paper.

Reply: As per reviewers' suggestion, the afternoon evening transitions (AET) is modified as Afternoon Transition (AT) in the revised manuscript.

2. Since this is a tropical site, something should be said about how it is or should be different than a mid-latitude site. For example, does the smaller range of solar zenith angles matter to the range of transition times? What about the more rapid reduction of incoming radiation at lower latitudes?

Reply: Yes...It, in deed, the incoming radiation differs at tropical and mid or high latitude sites, in terms of its magnitude, reduction during the afternoon, etc. All these factors impact the start time of transition, duration of transition, etc. A small note on this issue is included in the revised manuscript.

3. By the time availability of data from all platforms is taken into account, the number of days included in each section of the study is small and different. Please be more clear about this, even to the point of being tedious and repetitive. A related point is that the filtering for clear-sky days must introduce important biases especially in the monsoon seasons. Again, this needs to be made very clear.

Reply: As per reviewers' suggestion, the above information is included in Section 2 for each season, like total number of days in a season for which the instrument is operated and exclusion of data due to rain/dense clouds and quality. Since this information is given for each season, the biases, if any caused by the exclusion of data (say in monsoon season), will be known.

<i>Instrument</i>	<i>Data period</i>	<i>Total days</i>	<i>Instrument not works</i>	<i>Clear days</i>	<i>Quality of data</i>	<i>Data discarded by conditions</i>
<i>15m Tower</i>	<i>2009-2011</i>	<i>1095</i>	<i>311</i>	<i>531</i>	<i>423</i>	<i>108</i>
<i>Sodar</i>	<i>2007-2010</i>	<i>1460</i>	<i>241</i>	<i>673</i>	<i>530</i>	<i>143</i>
<i>Profiler</i>	<i>1999-2000, 2010-2011</i>	<i>1461</i>	<i>429</i>	<i>505</i>	<i>482</i>	<i>23</i>

4. The transition times are apparently chosen subjectively and are necessarily somewhat uncertain. This is not a problem, but should be made completely clear.

Reply: The thresholds used in the present study are not chosen arbitrarily. As per our definition, the start time of evening transition is the time at which atmospheric state variables show large and consistent 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 noticed that the results do not change much even if we vary the threshold by $\pm 20\%$. This new figure (included here for reviewers' reference) and the above information are included in the revised manuscript.

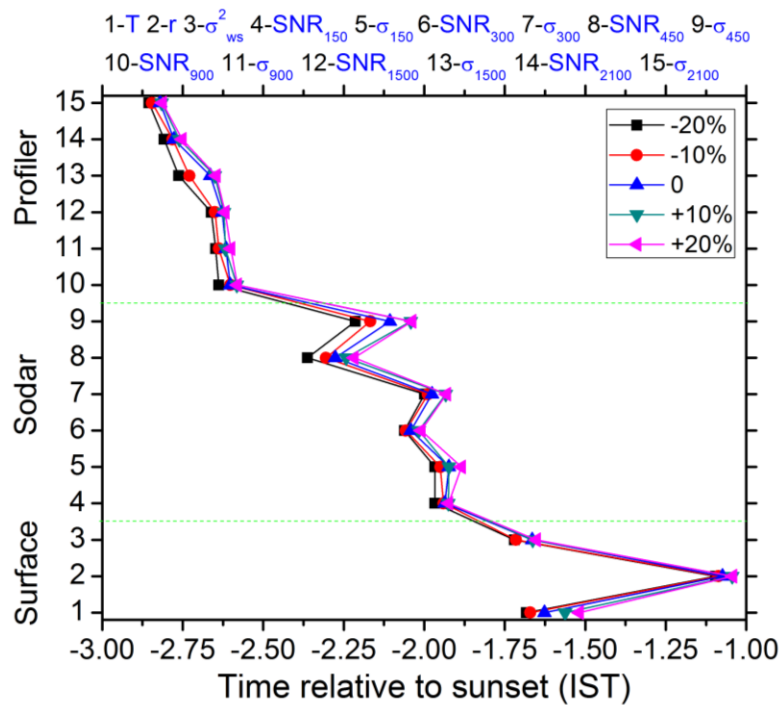


Figure: $Trans_{sunset}$ identified by different instruments employing a variety of atmospheric state variables by varying the thresholds, indicating the sensitivity of $Trans_{sunset}$ on the threshold.

5. The word "collapse" should be removed everywhere it occurs, since the paper shows that it is an inappropriate way to think about the transition.

Reply: Sorry. The word "collapse" is replaced with appropriate word (like 'fall', 'descend', etc.).

6. The entrainment flux analysis starting on p.31498 is interesting but difficult. This has been attempted previously but with no success. The main difficulty in obtaining meaningful measurements of the very small mean vertical velocity. Advection, which is not included in eq.3 but should be, is also usually important and very difficult to estimate. The results presented in figure 6 seem reasonable, but in order to give readers confidence that they are in fact correct, much more information is needed. A detailed uncertainty analysis should be done and error bars put on the fluxes. Some justification for the neglect of advection is also needed. If this harms the

flow of the paper too much, it could be put in an appendix or supplement, but it must be available to interested readers. Finally, figure 6 c and d are confusing because the days are run together as if they were continuous. At least the lines should be broken between the days, but a separate, larger figure might be better.

Reply: We agree with the reviewer that the estimation and analysis of entrainment flux is difficult. Several authors earlier tried to estimate this flux (see Angevine (1999) and references therein). As per reviewers' suggestion, we have now added advection term to equation 3 and the figures are modified with complete entrainment flux (including advection term), error bars, and vertical dashed lines separating different days. In addition, the fluxes are shown in a separate figure as suggested by the reviewer.

7. A related point to the above is that, as shown here, the concept of entrainment ratio has limited applicability and should be used with caution.

Reply: We do agree that the entrainment ratio, which is the ratio of entrainment and sensible heat fluxes, can vary due to the variation of any of those fluxes. The present study also shows the entrainment ratio variations are primarily caused (in our case) by sensible heat flux. Reviewers' suggestion is considered and the text is changed accordingly.

8. In general the figures need to be bigger and more readable. Not all of this is under the direct control of authors, but I urge the authors to work with the journal staff to make readable figures.

Reply: As per the reviewers' suggestion, we increased figures size, to depict the variations clearly. Also, figure 6 (in the old manuscript) is divided into two figures in the revised manuscript for clarity.

Specific comments:

1. Abstract, second paragraph: The wording is unclear. In fact the first evidence of the transition is aloft in the profiler data, followed by the sodar data and then the surface.

Reply: We modified it in the revised manuscript.

2. It seems that the SNR plots are not range-corrected. Is this true, and if so, why not? Plotting range-corrected SNR is clearer and more customary.

Reply: Range corrected SNR plots are given in the revised manuscript.

3. p.31495, top: Is there really not a consistent pattern between the radar and sodar? Elsewhere it is asserted that there is a significant difference in timing.

Reply: What we mean here is that there is no consistent pattern in $Trans_{sunset}$ as measured by SNR and spectral width at different altitudes (as measured by Sodar and Radar). For instance, SNR showing transition earlier than the spectral width or vice versa at a particular altitude. Nevertheless, there is a significant difference exists in the start time of transition as identified by Sodar and Radar. To avoid confusion, we modified the above sentence in the revised manuscript.

4. p.31495 line 3 and p.31496 line 21: Why is it considered easier to use SNR than sigma? It looks to me like sigma is even clearer than SNR.

Reply: In fact, the performance of SNR and sigma in identifying the transition is nearly the same and any one of them can be used for this purpose. The only reason we selected SNR is that it is consistent as evidenced by the narrow distribution of $Trans_{sunset}$. This point is made clear in the revised version of the manuscript.