Interactive comment on "Lifted Temperature Minimum During the Atmospheric Evening Transition" by Blay-Carreras et al.

The paper deals with observations of *Lifted Temperature Minimum* (LTM) obtained during the BLLAST field campaign, and try to analyse the role of turbulence and radiation on the formation of these LTM. The subject is well introduced; the paper is well written and structured, but however from my point of view it is not enough stressed why it is important this subject and I think that the next general and specific comments should be taken into account before the manuscript could be accepted. My recommendation is 'Major Revisions'.

General key comments:

- A major effort must be done to underline the importance of the presence of these LTM during the evening transition. These temperatures are found very close to the ground (in the first 14cm from the ground), so at first it could be thought that they are not very important for the study of the Atmospheric Boundary Layer.
- What is the importance and what are consequences of having these LTM along the evening transitions? Are they important in the evolution of the transition Boundary Layer itself or in the later Nocturnal Boundary Layer developed?
- The intensity of the LTM is really small (around 0.3 K in T1 site, and 0.5-0.7K in T2 site), so small uncertainties in measuring temperature could produce a distrust of the results. What cautions have been taken into account to have temperature measurements with high accuracy?
- Are the heights measured over surface/ground or over vegetation (grass in this case)? This should be clarified as grass height can change with wind for example, and the LTM height (<14cm) can be is comparable to grass length, so the accuracy in measuring height seems to be very important also in this case.
- I do not understand why potential temperature has been used instead air temperature to define the LTM (Fig. 3 for example). As a matter of fact you can have an increasing potential temperature from T_{base} upwards and have a decreasing temperature rate, and then you did not have a real LTM. (As $\Delta\theta=\Delta T + 0.0098\Delta z$; so $\Delta\theta$ can be positive and ΔT negative, and in that case LTM condition is not fulfilled).
- A key point to form the LTM is the different (larger) air emissivity compared to surface emissivity. However no air emissivity values are given nor discussed, neither what meteorological parameters (specific humidity for example) can be important for the emissivity.
- Why do you use the term LTM profile instead of LTM measurement? You have a LTM in a vertical temperature profile, but you do not have a profile with different LTM values, as it can be thought using LTM profile.
- Sometimes the observations are shown in a very descriptive way (for example lines 278-287). Try to discuss physical reasons for the different results found.

Specific comments:

1) Why the LTM found in the present paper are detected several hours earlier than in previous works? Is it due to different processes, conditions, locations??
2) In the introduction, the last observational reference seems to be Oke (1970). Haven't you found more recent observational works?
3) Line 74: change 'He' by 'They'.
4) Surface and air near the ground emissivities seem to be determinant to produce LTM. Are surface emissivity different at T1 and T2 (BLLAST took place over a quite heterogeneous terrain)? How different? Is air emissivity near the surface changing along the transition? Why? This should be discussed.
5) Lines 211-212. Surface emissivity is 0.986 (long grass), considering the reference of Gayevsky (1952). However Arya (2001) in his Micrometeorology book (page 32) gives a value of 0.9 for long (1m) grass and 0.95 for short (0.02m) grass. How sensitive can be the results to using these different values of emissivity? On the other hand in lines 223-224 you say that grass is short while in lines 210-212 long grass is referred; what is the truth?
6) Line 250: LTM intensity calculated after eq. (2) would be negative, but values given in Table 1 are positive.
7) Line 265: Is there any reason for different duration found in LTM event (24 June) at T1 (20 min.) and T2 (40 min)? Please try to discuss it. By the way, in Table 1 time duration at T1 is 10 min. not 20 min. as it is said in line 266.
8) With regards to the problems described in lines 288-302 it could be interesting to analyse LTM intensity as $\Delta\theta/\Delta z$ instead only $\Delta\theta$, as Δz in T2 is larger than in T1.
9) Section 4.1: It seems that you use wind measurements at z=2m. Following Fig. 1 I do not know exactly at what levels you have wind data. Don't you have wind from Kaijo?
10) Line 326: Why the decrease in wind speed is faster on 24 June, 1 and 2 July?
11) It could be interesting to extend the time for Fig. 5 up to 20 UTC, as in Fig. 4, or at least at 19UTC, as LTM at T2 ends at 18:50.
12) Lines 391-400: It is said that friction velocity is less than 0.1 m/s around 18:30UC at T1. However LTM is formed earlier (so with friction velocity>0.1 m/s). Could you explain this?
13) Line 441: change 'moist, air' by 'moist air,'.
14) Lines 445-450: I am not sure than in BLLAST latent heat release is small in comparison to other terms in eq. (5). Due to the high soil humidity in BLLAST, latent heat is important and often larger that sensible heat. Can you give some values to justify your sentence above?
15) Line 499: change 'inceases' by 'increases'.
16) Line 508-511: I think that what is said here is contradictory with values shown in Table 1. LTM intensity and duration for 30 June are similar to other days.
17) Line 519: What do you mean with 'moderate ground emissivity'?
18) Lines 519-522: Again I think this result does not match with Table 1 information.
19) Line 603: Change this reference by the actual one published in ACP.