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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Lifted Temperature Minimum during the atmospheric evening transition" *by* E. Blay-Carreras et al.

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Firstly, we would like to thank the referee for her/his detailed review. The comments and suggestions will surely improve the final version of this article. Below we answer to all the remarks.

Referee 2 comments

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.

In our opinion, the manuscript, besides increasing the knowledge of the physics of the surface layer, it can be also relevant for the agriculture. Lifted minimum temperature can modify the occurrence of frost, which has adverse effect on crops (Lake, 1999). Moreover, it can help to describe the presence of radiation fog, as it is shown in the article, the presence of LTM is related with a variation of the radiation (Mukund et al., 2014). A paragraph has been added in the new version of the manuscript about this subject.

- 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?

Even though the Lifted Temperature Minimum is observed during evening transition on this study, other effects such as radiation, subsidence, or advection have a greater influence on this period (Vilà-Guerau de Arellano, 2007; Angevine, 2008; Pietersen et al., 2014). Therefore, we cannot define the exact consequences of having these LTM along the evening transitions; moreover, it is not the objective of this study.

As shown in the manuscript, the presence of LTM during afternoon transition is not a common phenomenon. In our case it is related to the local conditions created by an early evening calm period. Therefore, as shown in Nadeu et al. (2013), really calm conditions observed during evening transition due to the orography, in our case the

Pyrenees Mountains, produce an early evening calm period, which affects the evolution of the transition of the Boundary Layer and the characteristics of the later Nocturnal Boundary Layer.

- 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?

Regarding the temperature measurements, we used a Campbell Scientific E-TYPE model FW05 with 12.7 micron of diameter. We considered the influence of direct or indirect solar radiation. Moreover, as Campbell (1969) showed, as the size of the thermocouple goes down, the radiative influence is reduced. For a 25 micron sensor a 0.1 degree of error was observed. Our sensor is half that size hence the error of the instrument should be lower than 0.1 degree, which is smaller than the values of the LTM intensity. This point is clarified in the new version of the manuscript.

- 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.

We agree with the referee, the accuracy in measuring height is very important. We measured the height of LTM over surface/ground to obtain a height value not affected by wind or other phenomenon. Moreover, we know that the grass height is around 0.03–0.07 m, and the observed LTM height occurred above 0.1 m from the ground, therefore, LTM is always over the grass height. Some lines have been added in the new version to make this point clear.

- 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 Tbase upwards and have a decreasing temperature rate,

and then you did not have a real LTM. (As Dq=DT+ 0.0098Dz; so Dq can be positive and DT negative, and in that case LTM condition is not fulfilled).

The referee is right. However, in the surface layer height (Dz in the formula above) is very small (from 0.015 to 8 m) so the difference between the potential temperature and the air temperature close to the surface is nearly negligible. Therefore, we cannot observe an increasing potential temperature from Tbase upwards with a decreasing temperature.

Taking into account this fact and considering that we use potential temperature to develop the turbulence analysis, we decided to use potential temperature to be consistent in the whole research.

- 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.

We agree with the referee. Figure 1 below shows the temporal evolution of the air emissivity approximated using the longwave radiation at 0.8 m and the closest air temperature measurements (1 m). We do not observe any change in the evolution of the air emissivity during the occurrence of LTM. We have not included this figure in the new version of the manuscript but some lines have been added to make this point clear.

In relation to other meteorological parameters influencing LTM presence, Raschke (1957) and Oke (1970) show that there is no correlation between LTM intensity and air humidity.

On the other hand, Sign et al. (2013), Mukund et al. (2014) and Sreenivas et al. (2014) showed that the presence of aerosol at low altitudes could influence LTM intensity. However, during BLLAST campaign, the presence of aerosols at lower heights was not monitored.

- 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.

We agree with the referee. We will modify these sentences in the new version of the manuscript.

- 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.

The referee is right regarding this particular paragraph. It is true that in section 3 "Observed LTM characteristics" the results are shown in a descriptive way because in this section we want to characterize the phenomenon. However, the following sections deal with the physical discussion trying to explain the presence of this phenomenon based in the analysis of different variables that influence LTM development.

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??

As described in the article, we conclude that LTM are detected several hours earlier than in previous works as a consequence of very calm conditions observed during evening transition. This calm period is produced due to the presence of the Pyrenees Mountains. Moreover, a change in the radiative conditions was observed during LTM period, which confirms its radiative origin.

2) In the introduction, the last observational reference seems to be Oke (1970). Haven't you found more recent observational works?

In the introduction more recent observational works like Bhat (2006), Mukund et al. (2010) or Mukund et al. (2014) are referred. Moreover, there are other works e.g Narasimha (1994) that even though they are not exclusively focused on observations they also include some measurements to contrast their results.

2) Line 74: change 'He' by 'They'.

We agree with the referee. We will modify it in the new version of the manuscript.

4) Surface and air near the ground emissivity 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.

BLLAST measurements took place over different land uses to study the importance of heterogeneity during afternoon transition. Nevertheless, the surface emissivity was not different at T1 and T2 because both towers were installed over short grass, so the surface characteristics in the both locations were similar.

As shown in Fig. 2 below, air emissivity near the surface is changing during the transition in all the IOP analysed. We approximate the ground emissivity using the longwave radiation at 0.8 m and the air temperature measurements nearly at the surface (0.015 m). However, the results did not show any specific modification during the period of LTM. We have not included Fig.2 in the future version of the manuscript but some sentences have been added to explain this point.

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?

In relation to the referee's question, we have included below new figures showing the temporal evolution of the upwards longwave radiation estimated, by using Eq. (7) using the values of emissivity of Gayevsky (1952) and Arya (2001).

Comparing Fig.3a and Fig.3b we can observe the sensitivity of longwave radiation to the value of emissivity. We observe higher upward longwave radiation values in the first figure when large emissivity is used. However, in both cases we observe a change in the radiative conditions that is not observed in the upward longwave radiation measured at 0.8 m. This is the relevant information that we want to obtain from these figures. Therefore, in our opinion, we can keep the values selected because it does not modify the change in the radiative conditions of the temporal evolution of the longwave radiation. We have maintained Fig.8 in the new version as it was in the previous manuscript but some lines have been added in to make this point clear.

We agree with the referee that there is an error in the text and short and long grass are wrongly cited. We will modify it in the new version of the article.

6) Line 250: LTM intensity calculated after eq. (2) would be negative, but values given in Table 1 are positive.

We agree with the referee. We consider the absolute value in the table but it is not mentioned in the text. We have modified the table in the new version of the article.

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.

In our opinion, the different duration of LTM in T1 and T2, for instance on 24 June, could be caused by small differences in the surface surrounding the towers. T1 was covered by short grass, but the T2 surface had also in some occasions some cut grass over the terrain, which could cause some heterogeneity in the surface thermal properties modifying the LTM duration.

Furthermore, we agree with the referee that there is an error in the duration of the LTM in 24 June in section 3. The sentence will be modified accordingly.

8) With regards to the problems described in lines 288-302 it could be interesting to analyse LTM intensity as Dq/Dz instead only Dq as Dz in T2 is larger than in T1.

We have analysed the LTM phenomenon by using Dθ/Dz to verify its duration. In Fig.4, we can observe an example of Dθ/Dz above (θLTMup $-$ θbase $\rightarrow$ dashed line) and below LTM (θbase $-$ θLTMdown $\rightarrow$ solid line) of T1. We can observe that the LTM exists when the solid line is < 0 and the dashed line is > 0.

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?

We select the wind measurements at 2m because it is the lowest height where we can obtain wind measurement in both towers. In T1, a Campbell Scientific CSAT3 Sonic Anemometer Thermometers was mounted at 2 m. We agree with the referee that there were Kaijos located under 2m but the measurements obtained were not reliable. In T2, separated approximately 2 m, there was also a Campbell Scientific CSAT3 at 1.95 m, recording data at 20 Hz.

10) Line 326: Why the decrease in wind speed is faster on 24 June, 1 and 2 July?

In our opinion, on 24 June, 1 and 2 July a clearer mountain–plain circulation was observed because the horizontal thermal gradient between the plane and the mountains was different for the different days of the campaign. To confirm this hypothesis, the results of a WRF-mesoscale simulation (Skamarock et al., 2008) with 3 km horizontal resolution from 29 June at 00 UTC until 3 July 2011 at 00 UTC were analysed. It can be clearly observed in Fig. 5 that on 30 June 2011 a surface northerly wind is simulated at Lannemezan (43°12'- 0.39°) until a later hour than on 1 and 2 July. This is due to the lower temperature simulated at the Pyrenees mountain range on 30 June. We have not included Fig.5 in the future version of the manuscript but some sentences have been added to explain this point.

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.

Figure 6 below shows the temporal evolution of the Richardson number from 17:30 to 19:00 UTC on all the studied days at T1. We will modify this figure in the new version of the article.

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?

We agree with the referee. In our case study, the sensors used to compute the friction velocity are located at 2 m. Therefore, in the same way that we extrapolate wind speed, we need to analyse friction velocity values at the height of the LTM. We have considered this decrease of height as a reduction of the friction velocity as it happens at wind speed. Therefore, the friction velocity values should be less than 0.1 m/s before 18:30 UCT, during the LTM period. We have added included a similar explanation in the new version of the manuscript.

13) Line 441: change 'moist, air' by 'moist air,'.

We will modify this sentence in the new version of the article.

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?

We agree with the referee that during BLLAST campaign we observed large latent heat flux. However, the fifth term of the conservation of heat equation is latent heat release which includes Lv (latent heat of vaporization of water), E (phase change rate), (density of the air), Cp (specific heat at constant pressure for moist). Analysing a specific IOP, the mean maximum of the latent heat flux in SS1 is approximately 0.15 K m/s during daytime on 1 July. During afternoon transition this value decrease to values close to 0.01 Km/s, which are small value compared to the other parameters of the equation

as shown in Fig.7. Part of this discussion will be included in the new version of the manuscript.

15) Line 499: change 'inceases' by 'increases'.

We will change it in the new version of the article.

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.

We partially agree with the referee. On 30th June the LTM intensity was very low and its duration was short. On 24th June, the intensity was higher even though the duration was similar and on 2nd July the intensity was similar but the duration was higher. Therefore, on 30th June we observe the lower combination of LTM intensity and duration from all the IOP analysed. Part of this discussion will be included in the new version of the manuscript.

17) Line 519: What do you mean with 'moderate ground emissivity'?

We agree with the referee that moderate ground emissivity it is not the appropriate way to define the emissivity of a ground covered by grass with an emissivity of 0.986. Previous studies (Mukund et al. 2014) define the emissivity of bare concrete patch (0.91) as high emissivity. The sentence will be modified by large ground emissivity in the new version of the article.

18) Lines 519-522: Again I think this result does not match with Table 1 information.

We do not agree with the referee. Table 1 clearly shows that LTM profiles were observed during all IOPs except on 27 June 2011. Moreover, in T1 we observe LTM intensity from 0.3 to 0.35 in contrast with T2, which has LTM intensity from 0.5 to 0.7. A similar situation is observed in the LTM duration.

19) Line 603: Change this reference by the actual one published in ACP

The reference has been changed.

References:

Angevine, W. M.: Transitional, entraining, cloudy, and coastal boundary layers, Acta Geophys., 56, 2–20, 2008.

Bhat, G. S.: Near-surface temperature inversion over the Arabian Sea due to natural aerosols, Geophys. Res. Lett., 33, L02802, doi:10.1029/2005GL024157, 2006.

Campbell, G. C., 1969: Measurement of air temperature fluctuations with thermocouples. Atmospheric Sciences Laboratory, White Sands Missile Range, ECOM-5273, 10 pp.

Lake, J. V.: The temperature profile above bare soil on clear nights, Q. J. Roy. Meteor. Soc., 82, 187–197, 1956.

Mukund, V., Ponnulakshmi, V. K., Singh, D. K., Subramanian, G., and Sreenivas, K. R.: Hyper-cooling in the nocturnal boundary layer: the Ramdas paradox, Phys. Scripta, 142, 014041, doi:10.1088/003

Mukund, V., Singh, D. K., Ponnulakshmi, V. K., Subramanian, G., and Sreenivas, K. R.: Field and laboratory experiments on aerosol-induced cooling in the nocturnal boundary layer, Q. J. R. Meteorol. Soc., 140, 151–169, doi:10.1002/qj.2113, 2014.

Nadeau, D. F., Pardyjak, E. R., Higgins, C. W., Huwald, H., and Parlange, M. B.: Flow during the evening transition over steep Alpine slopes, Q. J. Roy. Meteor. Soc., 139, 607–624, 2013.

Narasimha, R.: The dynamics of the Ramdas layer, Curr. Sci. India, 66, 16–23, 1994.

Oke, T. R.: The temperature profile near the ground on calm clear nights, Q. J. Roy. Meteor. Soc., 96, 14–23, 1970.

Pietersen, H., Vilà-Guerau de Arellano, J., Augustin, P., de Coster, O., Durand, P., Gioli, B., Hartogensis, O., Lothon, M., Lohou, F., Pino, D., Ouwersloot, H. G., Reuder, J., and van de Boer, A.: Study of a prototypical convective boundary layer observed during

BLLAST: contributions by large-scale forcing. , Submitted to Atmos. Chem. Phys, 2014.

Raschke, K. (1957), ÂÍ Uber das nÂÍachtliche temperaturminimum ÂÍuber nackten boden in poona. Met. Rundschau, 10, 1–11.

Singh, D. K., Ponnulakshami, V. K., Mukund, V. , Subramanian, G. and Sreenivas, K. R.: Radiation forcing by the atmospheric aerosols in the nocturnal boundary layer, AIP Conference Proceedings, 1531, 596-599, 2013.

Skamarock, W., J. B. Klemp, J. Dudhia, D. O. Gill, D. Barker, M. G. Duda, X.-Y. Huang, 869 and W. Wang (2008), A description of the advanced research WRF version 3„ Tech. Rep. 870 NCAR/TN475+STR, NCAR

Sreenivas K R, Singh, D. K. and Ponnulakshami, V. K. and Subramanian, G.: Importance of Aerosols in Predicting the onset of Radiation Fog, EMS Annual Meeting Abstracts, EMS2014-174, 2014.

Vilà-Guerau de Arellano, J.: Role of nocturnal turbulence and advection in the formation of shallow cumulus over land, Q. J. R. Meteorol. Soc., 133, 1615–1627, 2007.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C11773/2015/acpd-14-C11773-2015-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 27769, 2014.

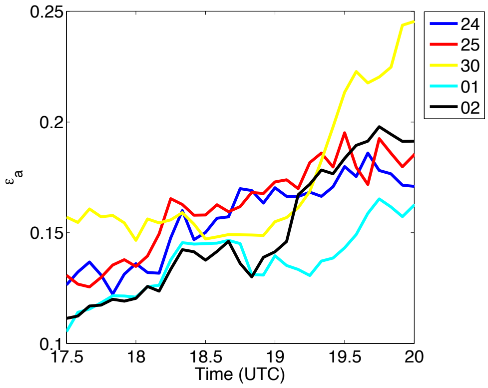

Fig. 1. Temporal evolution of the calculated air emissivity.

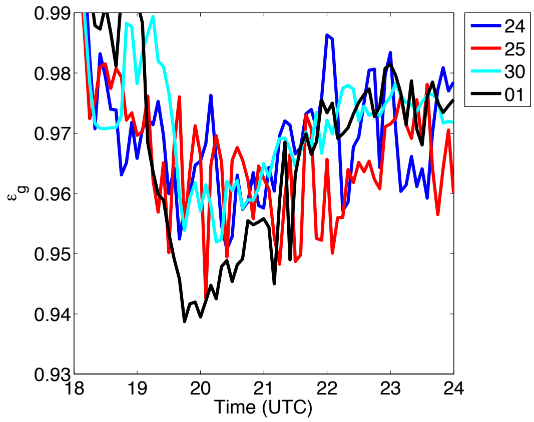

Fig. 2. Temporal evolution of the calculated ground emissivity.

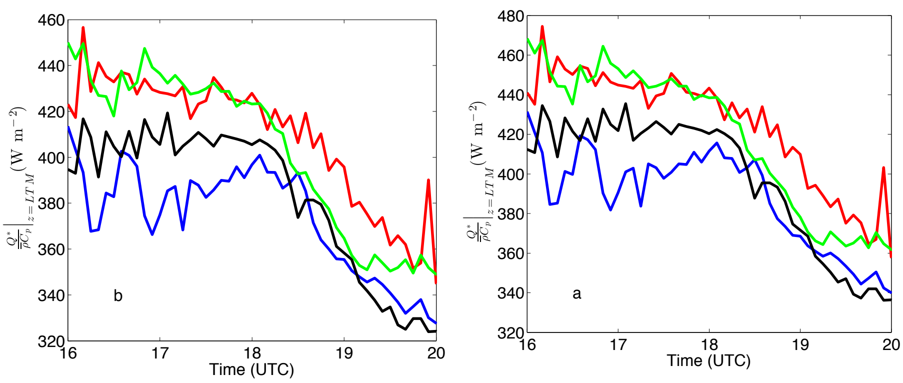

Fig. 3. Temporal evolution of upwards longwave estimated, by using Eq. (7) (a) using the values of emissivity of Gayevsky (1952) and (b) using the values of emissivity of Arya (2001).

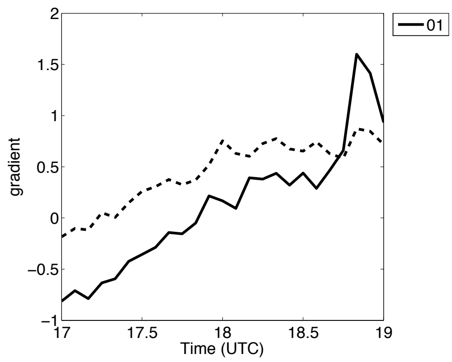

Fig. 4. Temporal evolution of the temperature vertical gradient on 1 July in the section over LTM (dashed line) and under LTM (solid line).

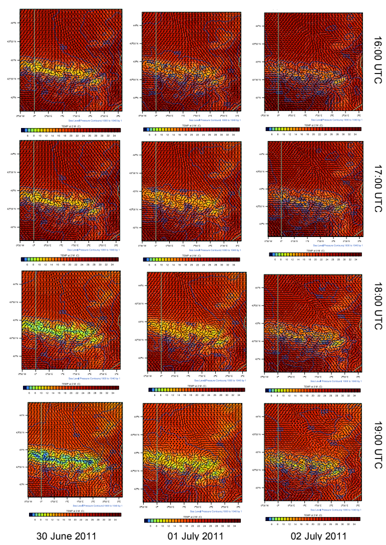

Fig. 5. Temperature at 2 m (contours), sea level pressure (white contours) and wind (arrows) on 30 June and 1 and 2 July at 16:00, 17:00, 18:00 and 19:00 UTC.

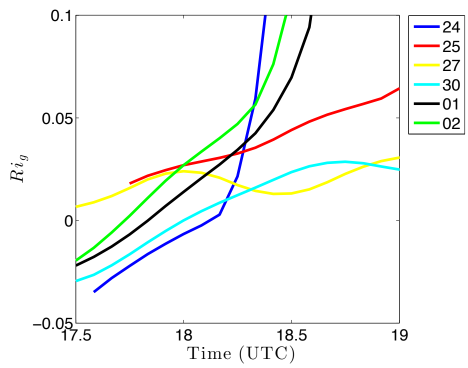

Fig. 6. Temporal evolution of the Richardson number from 17:30 to 19:00 UTC on all the studied days at T1.

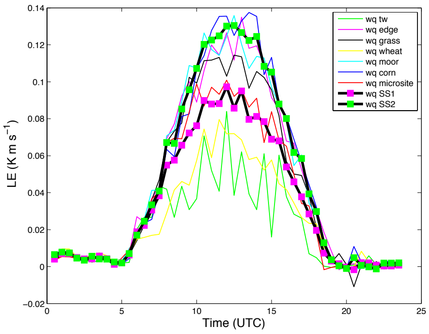

Fig. 7. Temporal evolution of the latent heat flux on 1 July at SS1 and SS1.