Referee 1 comments

1. The transition periods of the atmospheric boundary layer (ABL) are poorly understood. Most of our current analytical models and understanding rely on linear behavior and quite simple assumptions. That these do not reflect reality can be easily seen regarding the correlation of the vertical temperature profile and the buoyancy flux in time, which is often observed to be shifted in phase. In the morning this phenomenon can be described by the Rayleigh-Bernard (R.-B.) hypothesis. The manuscript describes experimental data from the BLLAST campaign, which is focussed on the afternoon and evening transition. Thus a reversed R.-B. hypothesis is tested.
The manuscript is easy to read and understand and well structured. Language could be improved, but this is not necessary in my opinion.
My only criticism is that the data base is rather thin and that the results discussed on page 7724 are based on only a few data points gathered on a handful of days (only two really convective days) at a certain location. However, the (thus statistically spoken not very significant) results are a motivation to study the ABL transitions in more detail and check the presented hypothesis with more data also from other experiments.

We partially agree with the referee. The BLLAST campaign only consisted in three and a half weeks and there were only 11 convective days (IOPs). Moreover, we mainly base the analysis in the observations made at a 10-m instrumented mast, which was not completely mounted during the first part of the field campaign. Therefore, we only have measurements during 6 IOPs. We selected this tower because it was equipped with a large number of closely spaced sensors (16 instruments) and was placed over relatively simple and homogeneous terrain. Therefore, we consider the best option to develop the analysis proposed in this article.

In spite of this fact, to our opinion the results presented can encourage to develop future field campaign focusing on the transitional periods to analyze the defined delay over different terrains, on other seasons and also during longer periods.

We have emphasized these aspects in the new version of the manuscript.

2. Specific comments:
2.1. Did you check whether the fine-wire thermocouple and / or their cold junctions including the connected electronics were influenced by direct or indirect solar radiation?
I saw similar experiments in the past where insolation disturbed thermocouple measurements significantly.

In relation to the referee's question, we have to explain that the thermocouples were very small, approximately, 12.7 micrometers in diameter. Therefore, the radiation influence should be very small. The manufacturer data sheet specifies that the small diameter of FW05 virtually eliminate solar loading.

Moreover, Campbell (1969) measured air temperature fluctuations with thermocouples. He shows that as the size of the thermocouple goes down, the radiative influence is reduced. Specifically, he observed less than 0.1 degree of error in a 25 micron in diameter thermocouple. Therefore, as our thermocouples are half that size, the radiative heating should be within the stated error of the instrument.

2.2. Please explain: how was the height z_i of the ABL detected and quantified using a ceilometer? Note, the cloud base is not a measure for z_i (page 7722, lines 11ff), cumulus clouds may form at any height within the ABL. The convective time scale strongly relies on a correct measurement of z_i and thus the following interpretation. Was z_i correctly determined on weakly convective days? Could this be the reason why the presented hypothesis agreed best on convective days (24 and 30 June)? This can be a minor issue if it turns out that it is just based on a misunderstanding. But if not it may have impact on the data interpretation. This is the only reason why I recommend a major revision of the manuscript.

The referee is totally right regarding how the height of the ABL was defined with a ceilometer. We made a mistake when explaining the instrumentation used to detect and quantify the depth of the ABL. We used a UHF profiler to define z_i. Specifically, we estimated the height of the ABL from the local maxima of the refractive index structure coefficient. We have modified the sentences related to this subject accordingly.

3. Technical corrections:

3.1. eq 1 and in text: dimensionless number Ra not italic!

The referee is right. We have modified it in the new version of the manuscript.

3.2. Fig. 4: I cannot see asterisks but bullets

The asterisks maybe were not clear enough. We have modified the definition in the new version of the manuscript.

Referee 2 comments

1. Due to the lack of response from several nominated second reviewers, I have had to intervene in the interest of time. I have read through the script and agree with reviewer 1 that it is well written and structured. I would ask you to heed each of reviewer 1's comments on the observational side. In addition, from the modeling side I would consider:

Relating your delayed transition idea to the 'demixing' idea of Nieuwstadt and Brost 1986. Are the two related?

To our opinion, despite demixing and the countergradient effect presented at the manuscript coexist during the afternoon transition, both processes are not necessarily related.

The presented investigation is compatible with the existence of a neutral layer, above the stable surface layer, where, due to entrainment, turbulence may still exists (demixing process) (Nieuwstadt and Brost 1986, Grimsdell et al. 2002, Pino et al. 2006a). The last upward movement can be accelerated in the upper part of the boundary layer due to the presence of the demixing process making possible a stronger descend of warm air introduced from the free atmosphere. The idea of having vertical movement in the upper part of the BL when the lower part become stable does not disagree the idea of the last eddy movement and it has been presented also by Sorbjan1997, although he did not relate the movements in the entrainment zone with the ground.

In spite the idea of demixing has been widely accepted, recently Darbieu et al. (2014) have shown, by using LES and aircraft observations that during the afternoon *transition turbulence (TKE and variances) decreases earlier at the upper levels of the boundary layer. If this result is also true during the evening transition, it seems that demixing, if it exists, cannot be also attributed to entrainment.*

We have extended the previous paragraph dealing with this subject in the introduction in the new version of the manuscript.

2. There are several existing parametrizations in weather and climate models that use counter-gradient mixing (e.g. Lock et al 2000, Holtslag and Boville 1993). Can you relate your observational results to these parametrizations?

The papers mentioned by the editor introduce a countergradient term to deal with turbulence (positive heat flux) in stable boundary layers (when positive gradient of potential temperature). Obviously this turbulence has to be produced by nonlocal transport. This occurs at the upper part of the boundary layer, just below the entrainment zone during the all evolution of the convective boundary layer.

On the contrary, the countergradient dynamics analyzed in the manuscript is produced at the evening transition near the ground. In this case, during several tenths of minutes coexist negative heat fluxes with a negative gradient of the potential temperature. Moreover, the temporal duration is really short.

We include a comment in the manuscript about the similarities and discrepancies between the countergradient concept used in weather and climate models and the one presented in our research.

<u>List of all relevant changes made in the manuscript</u>

- *We have included a description of the countergradient term proposed and Boville (1993) pointing out the main differences with our study.*
- *We have included a paragraph discussing the idea of demixing. We have summarized previous research studies about this subject.*
- *We have introduced a short explanation to clarify why the solar radiation doesn't influence FW measurements.*
- *We define that we could not expand the analysis during a longer period because we do not have measurements of both towers for more IOPs.*
- *We have corrected the explanation about the instrument that measures the boundary layer depth.*
- *We have modified the conclusions accordingly to the introduced changes.*

References:

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

Darbieu, C.; F. Lohou, M. Lothon, F. Couvreux, D. Pino, P. Durand, E. Blay, and J. Vilà: Evolution of the turbulence during the afternoon transition of the convective boundary layer: a spectral analysis. 21st Symposium on Boundary Layer and Turbulence, 8-13 June 2014. Leeds (UK).