Atmos. Chem. Phys. Discuss., 14, C3878–C3881, 2014
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Atmospheric Chemistry and Physics Discussions

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14, C3878–C3881, 2014

Interactive Comment

Interactive comment on "Countergradient heat flux observations during the evening transition period" *by* E. Blay-Carreras et al.

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Received and published: 20 June 2014

First of all, we would like to acknowledge the referee by his positive review and comments that largely help to improve the manuscript. Below we answer all his comments.

Referee 1 comments

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

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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.

Specific comments: 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. 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 zi. 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.

Technical corrections: 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.

Fig. 4: I cannot see asterisks but bullets

The asterisks maybe were not clear enough. We have modified the symbols for presenting clearer asterisks.

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

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 7711, 2014.

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