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Interactive Comment

# *Interactive comment on* "Study of a prototypical convective boundary layer observed during BLLAST: contributions by large-scale forcings" *by* H. Pietersen et al.

**Anonymous Referee #3**

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Summary:

This paper outlines a numerical study of the convective boundary layer (CBL) and a comparison of its evolution to field measurements from the BLLAST field campaign. The study focuses on a single day with high data availability and "light" winds (as characterized by the authors). The stated goal of the study is to see if the "prototypical" CBL is a useful description and to understand the role of large scale forcing in the CBL evolution. While I think both of these goals are good ones, I am not sure that they have been accomplished to a high enough degree. One of the main problems is that the paper fails to convince me that it has included all of the relevant forcing/BC terms to

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properly compare to the field data (mainly surface heterogeneity and geostrophic forcing). I feel that these two things either need to be included directly in the analysis or strong arguments against their inclusion should be made. In the current form since we are still dealing with an "idealized" flow that doesn't exactly agree with measurements I consider this study to be only slightly less "academic" than the other studies that the have been label as such in the text. In the end, the answers to the two main questions aren't definitive enough to make this more than an incremental contribution. Basically, if I was asked before reading this if I thought the CBL description was useful and that large scale forcing is important I would say yes and yes with some hesitancy on when and by what mechanisms. After reading this paper, I am not sure that my answer has changed much. My recommendation is that while the contribution is incremental it is useful and that if the authors can address my main concerns about forcing and BCs this work can be published. Addressing these concerns may require further analysis including new simulations so I consider this a major revision. Please find below specific comments on the paper (in approximately the order they appear).

Specific Issues:

Abstract, 5: "Combining" really isn't an appropriate word here. The two models are only minimally "combined". When I read the abstract before the paper I got the impression that I would read about a new way to force LES with a mixed layer model (I'm not even sure what that would mean but it made me curious). After reading the paper, it is clear this isn't the case. I think the wording of this sentence was what got me.

Abstract, 20: I would not consider the surface forcing to be "well-characterized". I wouldn't object to using the word "adequately" but well is stretching it considering the gross simplification (and lack of testing of this simplification).

Introduction, 19249, 15: I really don't like the way that this paragraph is formed. It misses key recent studies (e.g. see work by Nadeau et al.) and tries to diminish the observational studies as too focused on specifics. This paragraph needs to be rewritten to better reflect prior work. I think its current form is biased by the authors preconception of the uniqueness of BLLAST. For example, arguably, the study of Acevedo and Fitzjarrald is much more realistic than what is presented in this paper when it comes to evening transition. So that while I agree that they don't address large scale forcing, they still are important contributions to understanding transitions.

Introduction, 19250, 25: I think this needs to be a more complete review of diurnal cycles and forcing. While I agree that simulations over land with subsidence are rare, several papers have used diurnally varying forcing (large scale advection, geostrophic forcing etc) and looked at its influence. A short (and probably not exhaustive list) includes, Basu et al., JAMC 2008 (Wangara); Kumar et al., JAMC, 2010 (GABLS2 case), Edwards et al., 2014 (GABLS3), Rizza et al., Meteorol. Appl., 2013, Holtslag et al., BAMS, 2013.

Section 2.2: I find it very strange that shear is completely ignored in this study. It is well established that shear plays an important role in CBL dynamics (e.g. Conzemius and Fedorovich, JAS 2006) and especially in entrainment. Ignoring this by saying wind speeds are small seems problematic. Some sensitivity needs to be demonstrated with one of the models. I see no justification for this choice and it may lead to improper conclusions on the impact of large scale forcing (for example level of subsidence based on BL height in the iterative determination). The authors have decided to exclude what is (arguably) the most important large-scale forcing! What makes this even stranger to me is that they do include large scale advection. To me this implies (at least) shear at the boundary layer top which should enhance entrainment and at the surface which would enhance mixing. What makes this more puzzling (the lack of inclusion) is that almost all of the deviations with the measurements are explained away by saying "well we didn't include mean wind".

Section 2.3: Along the same lines as the fact that mean shear (at any level) is ignored, I find it puzzling that surface heterogeneity was ignored in this study. From my understanding (and the description in this paper), this site is locally highly heterogeneous (deviations in near surface fluxes of 100%, see figure 4 and 5). It is well established that surface heterogeneity plays an important role in the dynamics of the CBL across a wide range of scales (e.g., Letzel and Raasch, JAS 2003; Suhring et al., BLM, 2014; Suhring and Raasch, BLM 2013; Maronga and Raasch, 2013; to name a few of many from just one group). In addition, much of this work shows an enhanced impact under weak wind conditions with the blending height concept (ie homogenization by turbulence) all but failing (e.g Letzel and Raasch, 2003). One of the impacts is highly localized boundary layer heights (Maronga and Raasch, 2013). Given this large body of research (I've only mentioned a fraction), what is the justification for ignoring heterogeneity? To me, ignoring this without giving an indication of the impact of ignoring it makes the comparisons with the field experiments highly suspect, especially given the "tuning" used to find the large scale forcing. It also brings into question the comparisons with ground data for evening decay where one might expect highly local behavior that may not average linearly as assumed here. What is interesting is that the authors actually acknowledge this and present evidence that heterogeneity has some impact (fig 9) yet they don't followup on it.

Section 3.2, 19257, 1: Again I find the phase "combining these models" to be somewhat of a misrepresentation of what was done. You could say combining the output (or results) or using the two models.

Section 3.3, 19259, 3-5: When I look at figure 3, I see what appears to be directional shear and (at least at 16:44) what I would call moderate and not insignificant winds in the boundary layer. This would be my conclusion from the figure if I don't read the text description. Why should I believe that these are insignificant? At least some sensitivity (or at least scaling or a reference or something) needs to be given as justification.

Section 4.2: In this section (and throughout) the authors should be careful to state that this clearly shows that other processes play a role besides the mean surface forcing, explicitly this is all that is shown. Drawing other conclusions to me is stretching things.



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Section 5, 19263: The statement that “the differences in the entrainment velocities . . . are due to slight different methods used” is a little misleading. It makes it sound like the difference that is being discussed is the estimation used to develop the figure. When it is really the differences between the models that is reflected. The DALES model doesn’t prescribe the entrainment flux as a function of the surface flux while the mixed layer model does. I would say this difference is critical difference and not the different estimation methods. I think it is more appropriate to say that the differences are a result of the explicit representation of entrainment in DALES (I think this is what is meant in the text but I had to read a few times to convince myself of that).

Section 5, 19264, 6-13: A second explanation for the scatter in the specific moisture measurements at 60 m could be changing wind direction (see fig 3) combined with surface heterogeneity (see figs 4 and 5).

Section 5.2.2, 19269: I don’t understand why DALES integrated values across the entire boundary layer are compared to average surface observations? Why not simply compare DALES TKE at the surface with the observations? I know that much of the previous work has used BL integrated TKE but recent experiments have explored the near surface and can be used as a model (e.g., Nadeau et al.). Also please state if the TKE is the total or the resolved TKE (a simple technicality but important enough to be stated).

Section 5.2.2: It should also be noted that the exclusion of mean wind likely plays a role in the too fast decay in the models. Basically the models miss the likely (due to orography) reduction of the mean wind and a shift in the mean wind direction in the evening. I would expect this mean BL wind behavior to impact the tke (especially near the surface) by first acting like a source by producing tke through mean shear production and then dying off (as the winds shift) resulting in a rapid decay. This is what I see in the figures. This is brought up at the end but minimized. To me this is further evidence that really this study is almost as “academic” as past studies.

Section 5.2.2 and Figure 14: Note that all the symbols used in figure 14 don't match the text.

Section 5.2.2, 19270: In previous studies, the TKE decay rate is quantified. Why not do that here so that the differences can be quantified between the different cases. This would also facilitate a wider comparison with other studies (although introduce the problem of decay of the integrated or single level value).

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 19247, 2014.



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