AUTHORS RESPONSE TO: *Interactive comment on "Interactions among drainage flows, gravity waves and turbulence: a BLLAST case study" by C. Román-Cascón et al.*

Anonymous Referee #1

* Answers are in blue and reviewer comments are in black. Please, note that figures in this document are indicated with * symbol, while figures of the manuscript are linked without this symbol.

This paper describes a case study from the BLLAST field campaign which focussed on formation of stable boundary layers around the evening transition in complex terrain. The case study is an interesting one where after a brief period of calm, a shallow drainage flow forms downslope, eventually overwhelmed by a larger scale nocturnal katabatic flow from the mountains to the south (which is investigated with help of a numerical model). Propagating gravity waves are detected in pressure signals at the surface during both the main phases of the flow, and in turn characterised, and impacts on surface turbulence and fluxes due to these waves and the drainage flow are examined. Multi-Resolution Flux Decomposition (MRFD) is used to elucidate effects across scales, and comparisons made for a range of heights spanning the depth of the shallow drainage flow, and for three different sites. MRFD illustrates nicely the separation of scales and, for instance, direct (wave-induced convergence/divergence) and indirect (modulation of winds and hence turbulence and fluxes as a result of convergence/divergence) influences of gravity waves. These aptly demonstrate local variability, and at times how difficult it is to explain conclusively, and the difficulty in defining averaging intervals for turbulent flux calculations. While I felt explanations in places could be a little better thought through and lucid, and perhaps more attempt made to at least tentatively explain rather than simply describe, I'm happy to recommend publication subject to carrying out minor revisions in response to the list below, expanding their analysis if the answer to a given point exposes any oversight by the authors or potential benefit of deeper examination.

The authors would like to thank Reviewer #1 for his/her helpful comments and suggestions. We are sure that they are going to improve the quality of the manuscript.

As a result of the queries from the three reviewers, the authors include a deeper explanation of some of the processes commented through the paper in the new version of the manuscript.

12824. 22. "allusive" should be "elusive".

It has been changed in the text.

12826. 6. "on the study" should be "out the study".

It has been changed in the text.

12830. 25. What instrument is shown in Figure 3?

This is also a query of Reviewer #2. These profiles were plotted from sonic measurements obtained at the divergence site tower (below 8 m) and from the 60 m tower. This information has been included in the caption.

12830. 27. It looks like there is a shallow (1m) drainage current occurring at the wheat site.

Yes, it is, but after discussion among co-authors and people on charge of the instrumentation of this tower, we concluded that the lower measurements of the edge site towers have been subject to SE flow blocking/distortion by the mast and other equipment mounted on the mast, especially during the dominant low-wind conditions. Therefore, we propose to eliminate 0.5 and 1 m levels from the figure of the grass-site tower (Figure 4a) and 1 and 1.5 m levels from the figure of the wheat-site tower (Figure 4b).

Figures 2 and (especially) 4 - it would be better if a given colour corresponded to approximately the same height in each figure panel.

We do agree. We have changed the colours in the new version of the manuscript, according to the suggestion.

Figure 8(d) - how was this BV frequency calculated? It looks rather noisy. Were adjacent pairs of heights used? If so did the authors try any methods which take into account a deeper range of heights at each level (which would, looking at Figure 8(c), presumably lead to a smoother profile of BVF)?

Yes, we calculated N_{BV} using adjacent temperature measurements. Reviewer #2 asked the same.

N_{BV} has been calculated from temperature measurements (potential temperature) at different heights. In fact, the temperature profile showed in Figure 8c is not as smooth as it seems, since it includes narrow unstable layers (and therefore some narrow layers have $N^2_{BV} < 0$ s^{-2}). Temperature above 60 m is obtained from measurements of tethered balloon descent, which was averaged **every 5 data** in the first version of the manuscript. This is the reason of the noisy behaviour of N_{BV} profile.

To solve this, a new figure has been prepared (Figure 1*, new Figure 8d), where measurements from tethered balloon are averaged **over 20 m layers** instead of over 5 data points. In this case, N_{BV} profile is smoothed, although it still has a clear layer where N^2_{BV} becomes negative, located around 200 m agl. We have changed the main text and we state that it is not so easy to determine exactly the layer where GWs are propagating, since it depends on GWs features and wind and temperature profiles. However, we also say that the propagation around the layer at 200 m agl is not going to be favoured, since the thermal profile is not stable in a shallow layer at that height.

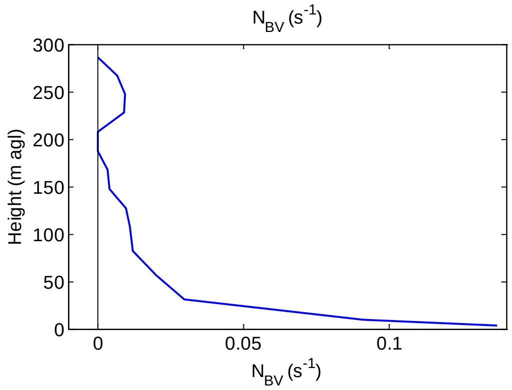

Figure 1*. Brunt-Väisälä frequency (N_{BV}) (s^{-1}) calculated every 20 meters after averaging tethered balloon data.

12833. 8. "as" should be "such as".

It has been changed in the text.

12834. 11-18. It feels as though the authors should at least make some effort to back up their assertion by helping the reader draw a visual correlation between the variables in Figures 2 and 4 and the pressure oscillations, perhaps by drawing dotted lines on the figures to indicate particular features.

The whole text has been completely revised and now we try to write more details observed in figures. It will be included with the next submission of the manuscript.

12836. 8. word missing here?

Yes. It has been changed by "...a continuous signal in the MRFD"

12389. 8. Do the authors have any explanation for the difference in wind between the grass and wheat sites during this period? Can the authors comment on the effect of the field boundary close and to the south (i.e. upwind) of the grass site? Could this play any part in the low winds experienced at the lowest detector levels during the SDF period at this site? Alternatively does the downwind wheat (and associated "flow collision") have any impact.

We do agree. The maize field located to the south (upwind) of the grass site (see Figure 2c* and 2d*) could also be influencing the low wind measured at the grass site. When the flow passes through the grass and arrives to the boundary site, turbulence is increased by collision of the flow with the boundary and then the flow is again different at the wheat, influenced by the canopy of this vegetation. This discussion is included in the new version of the manuscript.

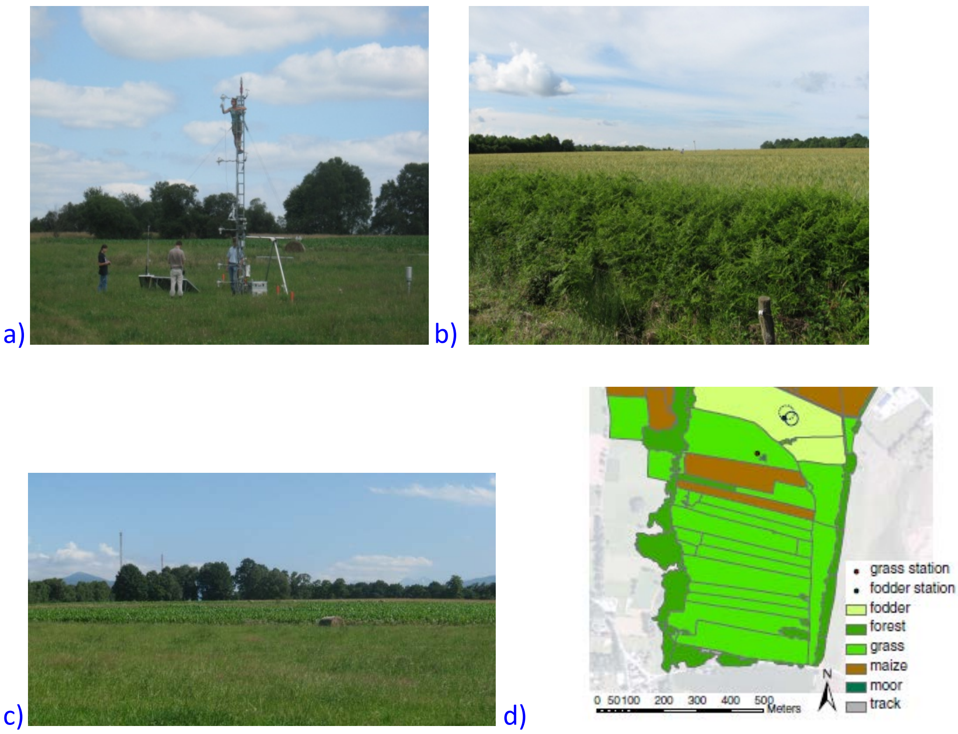

Figure 2*. a) Grass tower with line of three trees in the background (to the SE). b) Vegetation composing the boundary site (note that this kind of vegetation is harder than wheat, located in the background). c) Maize field located to the south of the grass site and line of trees at the background (SE). d) Land-use map from van de Boer et al. (2014).

REF -- Van de Boer, A., Moene, A. F., Graf, A., Schüttemeyer, D., & Simmer, C. (2014). Detection of Entrainment Influences on Surface-Layer Measurements and Extension of Monin–Obukhov Similarity Theory. Boundary-layer meteorology, 152(1), 19-44.

12839. 11. Figure number incorrect.

Yes, it was incorrect. Thank you. It has been changed to "Figure 11c".

12839. 9-20. It seems that a lot of this can be explained simply by the fact that the wind changes barely at the wheat site, but radically at the grass at the onset of the mountain-plain wind...

We do agree. This information has been added in the text.

12840. 21-24. I didn't understand this sentence, could the authors clarify?

This question was asked by reviewer #2 as well. In fact, the sentence was not correctly expressed and it has been changed following suggestions from both reviewers. We meant that the increase in turbulence caused a reduction in the temperature gradient (mixing), and therefore, the heat flux was reduced.