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 #3

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

The authors documented two wave events, and both are associated with the flow from the mountains in the south. They analyzed the wave and turbulence characteristics and their relationship with a shallow drainage flow. The case analyses here are similar to the ones in Sun et al. (2015, JAS, 72, 1484) with a different dataset. The paper is well organized and clear. The dataset is unique. It seems to me that the authors could say more about connections/interactions between the shallow drainage flow, gravity waves, and turbulence.

We do agree, the case presented by Sun et al. (2015) is very similar to this one, with wave-related oscillations observed in surface meteorological parameters, especially very close to the ground, where the stratification is higher. Regarding a deeper analysis, we think that the main aim of the paper is to show a general overview of all processes and not to focus **deeply** on wave-turbulence interactions. The different stages are very clear in this case study and the processes could be tracked quite well. However, after deep analyses of many parameters at different sites of the BLLAST sites and due to the high heterogeneity of the area (and non-perfect nature of waves), we were not able to obtain more specific conclusions about the connections among processes. This is mainly due to the interactions of these processes with the terrain, which makes the study more and more difficult.

In any case, 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.

Figures 9-12 show the temporal variation of turbulent fluxes as a function of time scale, but the maximum time-scale was capped below the shortest gravity wave period identified in Table 3.

MRFD figures were mainly included to analyse changes and features of turbulence, but not to perform a deep analysis of GWs frequencies. MRFD is a very nice tool to analyse higher frequencies (turbulence), but it has a limited utility if we want to analyse lower frequencies (see for example Viana et al. (2010, JAS, 67, 3949) for a deeper discussion on this issue). In fact, other methods (as wavelet) are more appropriate to deal with longer periods. For this reason, MRFD was truncated at the temporal scale N=14, which is equal to $2^N * \Delta T$ (= 2^{14} *0.05 = 819.2 s = 13.65 min). Where N has to be an integer and ΔT is the frequency of measurements (0.05 s^{-1}). Thus, the next possible analysis would be N=15, equal to 1638.4 s (= 27.3 min), which is larger than the period of observed GWs. Therefore, we lose the information between 13.65 min and 27.3 min, which is where the periods of observed GWs are. In fact, if we plot MRFD until 27.3 min (N=15), the contour figure is going to interpolate between 13.65 and 27.3 min and the colours in the middle could show something not real in this relatively long interval of periods. For this reason, we preferred to truncate the MRFD in N=14. However, for shorter periods (higher frequencies, turbulence), the obtained resolution is much better and the analysis shows interesting features.

One question I have is the relationship between wave propagation direction and the direction of wind convergence. It looks like the drainage flow for either the early shallow one or the later deep one was from the mountains in the south, which opposes the ambient weak wind from north. Thus, the wind convergence in the approximate north-south direction could lead to the displacement required for the buoyancy wave. However, the wave propagation direction is about 90 deg (either from or toward) as listed in Table 3. Is this common that the wave propagation direction is approximately perpendicular to the wind convergence direction?

We think that convergence can cause propagation towards all the directions from the source, but maybe only some of them are favourable (mainly depending on wind profile) for GWs propagation. It seems reasonable to expect also a propagation perpendicular to the wind convergence. However, we do not have the conviction that GWs were formed by the convergence between these flows. Considering the 4 cycles observed from 1900 UTC to 2030 UTC (see Figure 6 or Figure 7), they were also observed at SS2 (which is approximately 5 km to the south from edge site). However, the local character of SDFs was demonstrated, formed at some places but not effectively at others. Besides this, they were very shallow and consequently, the vertical displacement caused by the crash between SDF and the previous N-NE wind is not expected to be very important (at least in the BLLAST area). However, we do not have a better explanation about the genesis of GWs. We think that maybe the katabatic (deeper one) could be observed some time before near the mountains. The interaction of this flow with the complex orography to the south (southwest) from BLLAST area is another possible explanation, but we cannot ensure anything with the available data.

L. 25 on P. 12831. The deeper wind? Maybe the strong wind over a deep layer?

We do agree. This sentence has been changed.

The last sentence before section 3.2.1. The next two sections.

We do agree. This sentence has been changed.

The second line on P. 12834. It seems to me that the depth of a duct layer decides the depth of the wave layer. I am not sure how the depth relates the amplitude of the pressure perturbations.

This is something previously discussed among co-authors. In some cases (Román-Cascón et al. 2015. QJRMS, DOI: 10.1002/qj.2441), we have observed a relation between a narrowing of the duct layer and changes in period/wavelength (the shallower the duct layer is, the smaller the period of the wave). In fact, maybe an analogy with tsunamis in the sea is possible. When these oceanic waves (with longer periods, longer wavelengths, small amplitudes and high speeds in open sea) arrive to shallower waters close to the coast they transform into waves with shorter periods, shorter wavelengths, higher amplitudes and slower speeds.

We are not sure about the analogy, but we think that the depth of the layer could cause changes in the GWs features, including the amplitude. In any case, this is just a hypothesis and for this reason we write in the text "could be due to ..."

If the reviewer considers it appropriate, we can remove this sentence from the paper, since its information is not strictly necessary.

L. 17 on P. 12835. It is hard to see the lack of turbulence generation the middle (2m?). Maybe the authors can consider showing the momentum and heating fluxes integrated over the relevant time-scales too.

We thought about the possibility of including these type of figures (Figure 1*) in the first version of the manuscript. Figure 1* shows the contribution to the friction velocity from different scales during the SDF stage (average of values from 1900 UTC to 1955 UTC). It is true that in this figure it can be observed better the "lack of turbulence in the middle (2m)". However we decided not to include these kinds of figures because they are a lot of figures (4 figures of MRFD x 3 subfigures x 4 different stages (well-mixed part, near calm, SDF, katabatic). So, in total there are many figures and all this information is indeed shown and can be inferred from the actual MRFD figures.

In any case, if the reviewer considers it appropriate, we can include some of them in the new version of the paper.

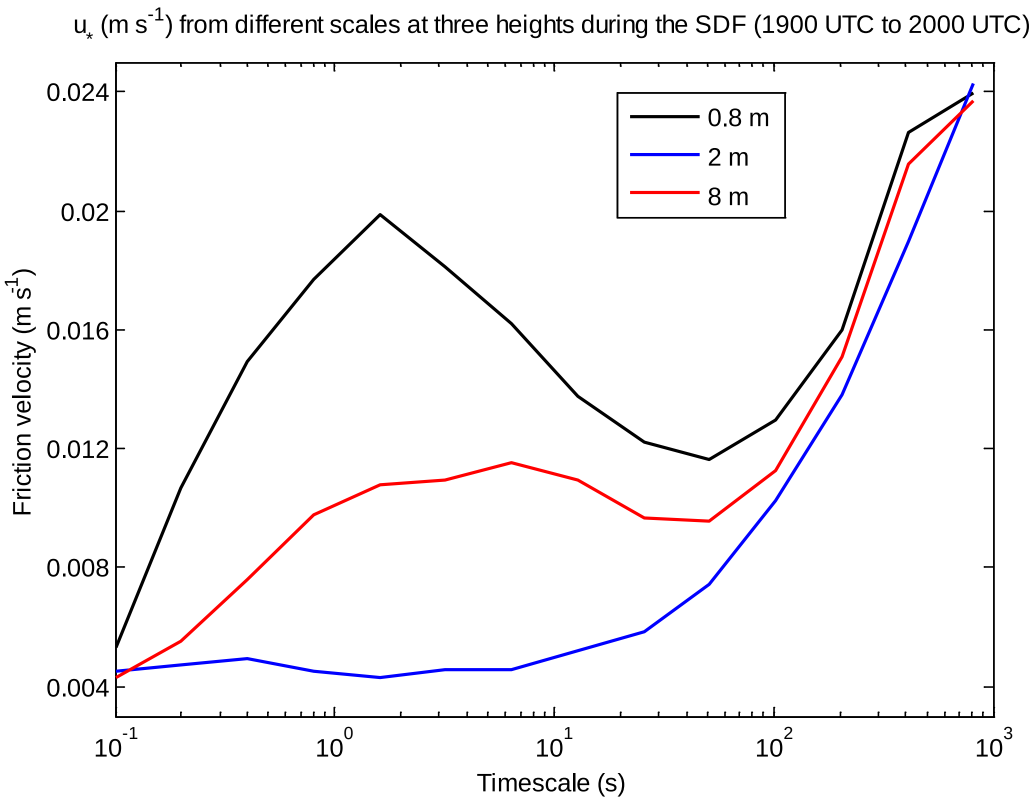

Figure 1*. MRFD of the friction velocity (m s^{-1}) at 0.8 m (black line), 2 m (blue line) and 8 m (red line) at the divergence site from 19.00 UTC to 1955 UTC (SDF stage). Note the lack of turbulence at 2 m, coinciding with the SDF wind maximum.

Table 3. Are the wave propagation directions here the directions waves propagation to or from? The second time period, 2005-2025 UTC, has only one wave cycle if the wave period is 22-24 min. Any justification to divide the wave event 1 into two periods? The wavelet signal of p for this period in Fig.6c could be the signal for wave event 2 extended over depending the size of the window where the wavelet is performed.

They indicate gravity wave propagation (towards). Now, it has been clarified and specified in Table 3.

We think that GWs features of wave event 2 are different from those of wave event 1. In fact, it can be appreciated by looking at Figure 6b. The wavelet signal associated with wave event 1 is centred in periods of 20-25 minutes, while wave event 2 has shorter periods.

The wave events have been divided in two different periods each one (in Table 3) for the calculation of wave parameters. This is done because it is quite difficult to obtain a short range of wave parameters if this calculation is done for relatively long time periods. Therefore, this calculation should be done over short time periods and where the energy of the wavelet is high (see for example Terradellas et al., 2001 or Viana et al., 2009). For this reason, these events have been divided in two. In fact, for the first part (1925 to 2000 UTC) this calculation of wave parameters is not able to provide a clear propagation or clear parameters. On the other hand, it is clearer for the second part (2005 – 2025 UTC).

Figure 1. Since the drainage flow is associated with topography, it would be better to have a topographic map too.

We do agree. We have prepared a new figure (Figure 2*) to be included in the manuscript instead of current Figure 1. Note that we also include the previous image (from Google Earth), since we think that it is also very representative of the heterogeneities in the area.

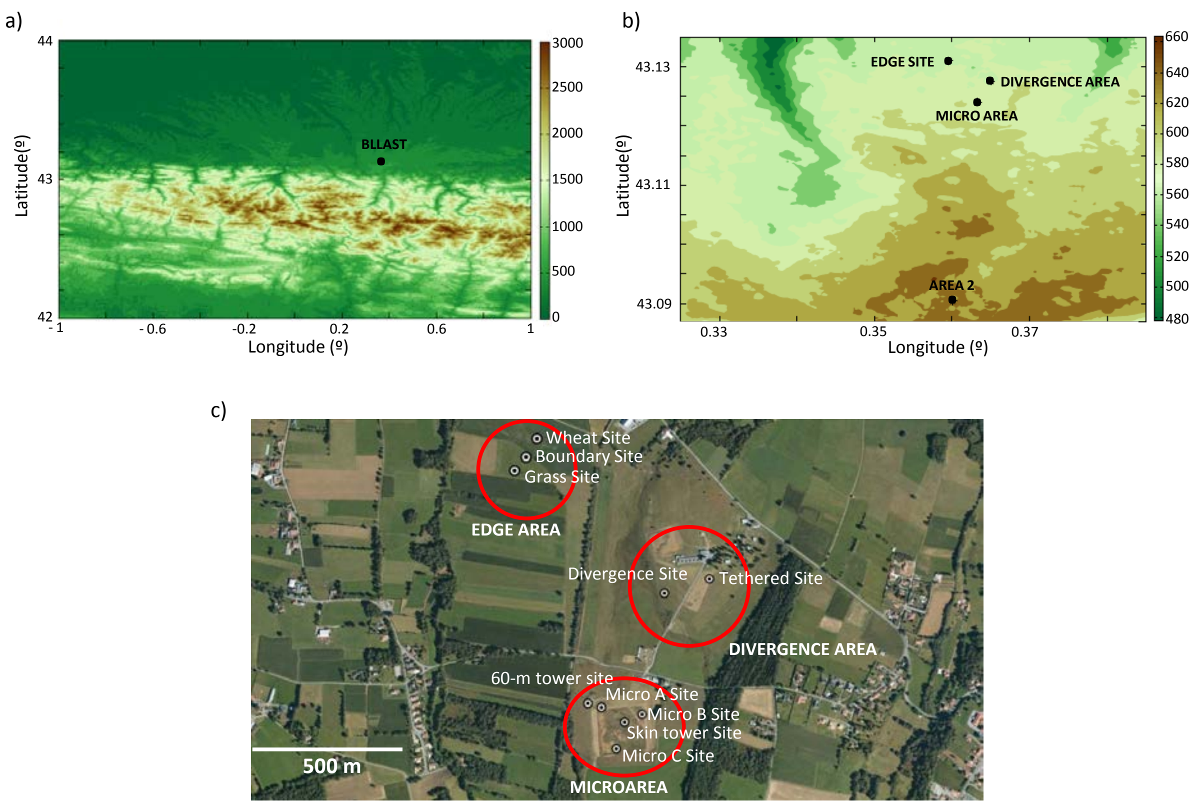

Figure 2* (new Figure 1). a) Topographic map of Pyrenees area around BLLAST. b) Topographic map of BLLAST area. c) Aerial view of BLLAST sites (except Area 2). (NOTE - Figures a and b from Routine ASTER Global Digital Elevation Model from NASA Land Processes Distributed Active Archive Center (LP DAAC). Figure c from Google Earth).

Figure 7. As I understand, Edge area has the lowest elevation, and Area 2 has the highest elevation. However, the pressure at Edge area has the lowest value. This could be real, but different from what I expected.

In fact, the values in brackets in the legend (+4.45 hPa at Area 2 and -3.6 hPa at Edge Area) indicate the value that has been added/subtracted from the original value. That is, at Area 2, 4.45 hPa have been added. However, at the Edge Area, 3.6 hPa have been subtracted from the real values of pressure (for plotting reasons). At the beginning of the plot (1800 UTC), the pressure is of 943.18 – 4.45 = **938.73 at the Area 2** and 943.08 + 3.6 = **946.68 hPa at the Edge Area**. That is, the pressure is higher at the Edge Area (as expected).

Maybe, it was not clear in the legend. In the new version of the manuscript, this is clarified in the figure caption.

Figure 8. Are the sharp changes of wind-speed and direction at 100 m and 200 m real? The temperature profile does not have any signal at these levels.

It seems that there is a slight LLJ around 100 m a.g.l. At 200 m a.g.l., the decrease in wind matches with a narrow unstable layer. The temperature data has now been averaged every 20 meters, following recommendations of reviewer #1 and #2 to obtain a smother N_{BV} profile. This temperature profile indicates also a small unstable layer around 200 m a.g.l, coinciding with the signal obtained in wind at this height. We think that these changes in wind are real.