

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 #2

* 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 manuscript presents a rather nice case study from the BLLAST field campaign demonstrating the interaction between shallow drainage flows, gravity waves and turbulence in the hours around and just after sunset. Understanding, and being able to model, the complex small-scale processes which are important in stable boundaries layers over non-homogeneous terrain remains a challenging problem, and detailed observational studies such as this are an important part of talking these challenges. The MRFD technique offers an interesting way of studying the contributions to the flux from different scales in the flow. The material is certainly worthy of publication, however I do have some queries and suggestions which I hope will clarify and improve the presentation of this work.

The authors appreciate the effort done by Referee #2 reviewing this manuscript. His/her comments will improve the quality and clarity of the manuscript. In the following answers, we will try to answer to all the queries.

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.

MAJOR COMMENTS

1) **p12832 and table 3.** The parameters here seem to demonstrate quite a bit of variability from one time period to the next. While this is not surprising given the complications of "real world" flows, it would be useful for the reader to 1) have a clearer idea how they were obtained and 2) give some estimate of the uncertainty in these parameters.

Sometimes we present these values with figures, which is maybe a more illustrative way than using tables (see example in Figure 1* (below) for period 20.35 to 20.55 UTC).

These figures are obtained through the calculation of phase differences between filtered surface pressure from 3 microbarometers (knowing their exact position). Thus, for different wave periods, we obtain different values and the same for different times. The range of wave periods is previously selected from regions of high energy in wavelet analysis. From this information, we plot the contours of the values and we obtain a figure like Figure 1*. In these kinds of figures, the uncertainty is determined by the range of values obtained. For example, a perfect wave (ideal) of 20-min period will have a constant value of 20-min period for the whole time period (without superimposition of other motions).

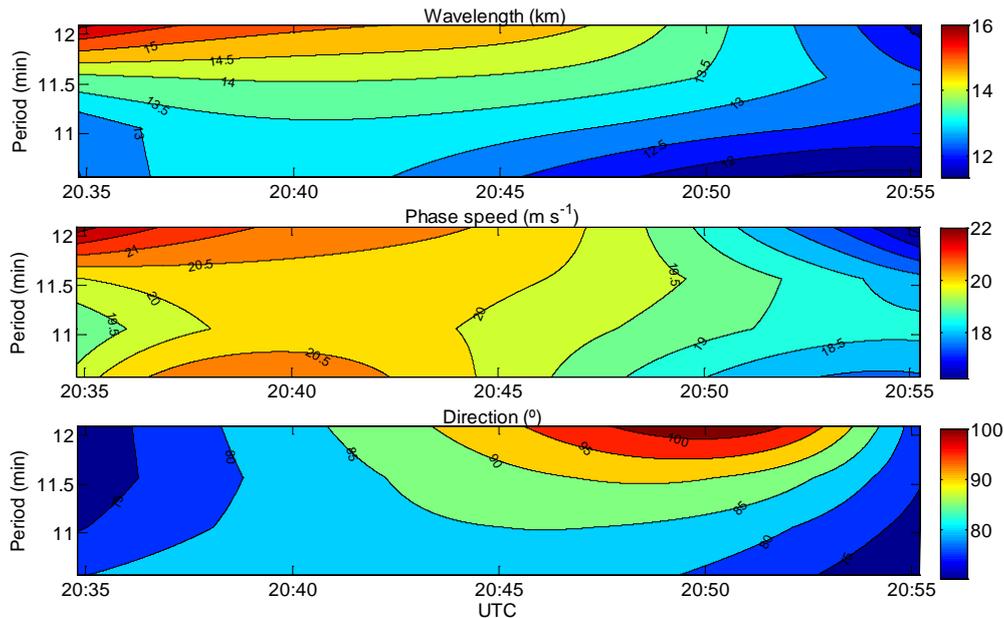


Figure 1*. Wave parameters evaluated from 20.35 UTC to 20.55 UTC and for wave periods between 10.5 and 12 minutes: a) Wavelength (km). b) Phase speed (m s^{-1}). c) Direction of propagation ($^{\circ}$).

As stated by the reviewer, in the real world is very difficult to find near monochromatic waves, but, the shorter this range of values is, the more monochromatic a wave is.

Due to the high variability found among different periods, it is not appropriate to show a figure like Figure 1* for the whole period (approximately from 19.30 UTC to 21.30 UTC) in the manuscript, because we lose detail in the contours (large range of values for larger time periods). On the other hand, it is not the best idea to show 4 figures like Figure 1* for each period, since that would be too many figures. Thus, we concluded that the better way to show the information was in a table, where the range of values (given in “[]”) show the variability for different times and periods. For the last two periods (20.35 to 20.55 UTC and 21.05 to 21.30 UTC), we found a relatively small range of values compared to the two first periods and to other cases previously analysed by authors. This is indicating quite clear wave parameters (short range of values). To highlight this, we gave in Table 3 (in the first manuscript) exact values for the two last periods (instead of giving a range), although it is true that there are uncertainties. We propose to include these uncertainties (range of values) for wave event 2 in a new Table 3 (see below):

| | Time (UTC) | Period (min) | Wavelength (km) | Phase speed (ms^{-1}) | Direction of propagation ($^{\circ}$) |
|--------------|------------|--------------|------------------|----------------------------------|---|
| Wave event 1 | 1925–2000 | 20–25 | not well defined | not well defined | not well defined |
| | 2005–2025 | 22–24 | [23 - 30] | [17 - 19] | [80 - 90] |
| Wave event 2 | 2035–2055 | 10.5–12 | [12 - 15] | [18 - 20] | [75 - 95] |
| | 2105–2130 | 16–21 | [7 - 10] | [6 - 9] | [32 - 42] |

Table 1* (new Table 3) → Gravity waves parameters evaluated from filtered surface pressure records of three microbarometers. Uncertainty is indicated inside brackets (range of values). Note how uncertainty is lower for wave event 2.

Therefore, we include now the uncertainty inside brackets in the form of range of values for all periods. In addition, a better explanation of how we calculate this is proposed to be included in the main text (at the beginning of Section 3.2.1 p12832). We also include references to Viana et al. (2009) and Terradellas et al. (2001):

“Wave parameters have been evaluated from phase difference analysis (see Section 2.2.1), knowing the exact position of each microbarometer (Terradellas et al., 2001; Viana et al., 2009). This method is based on the differences between wave phases of the three filtered pressure records (one for each microbarometer). These differences are calculated for a determined time period and attending to different wave periods. Thus, for selected ranges of time and wave periods, we obtain specific ranges of wave parameters. The shorter this range of values is (for example for wavelength), the more monochromatic a wave is. This evaluation indicates not well defined values for the first part of Event 1 (Table 3, from 1925 to 2000 UTC). This means... (Continuation in p12832 line 5)”

2) **p12833**. The authors seem to have a conceptual picture of the gravity wave here and how it propagates, but this is not very clearly communicated to the reader. I assume you are picturing some form of trapped gravity wave? Do the profiles of temperature and wind given in figure 8 give rise to trapped waves of the right kind of wavelength? You mention the role of shear at around 100m. Would plotting the Scorer parameter profile in figure 8 demonstrate possible wave trapping?

In the first version of the manuscript, we considered gravity waves trapped in the layer from the surface to approximately 100 m agl, where the conditions are stable without any doubt. The minimum N_{BV} found in this layer was 0.01 s^{-1} , which indicates that gravity waves of smaller frequencies can form and propagate in the layer. The maximum frequency of the analysed gravity waves is 0.0017 s^{-1} (corresponding to a period of 10 minutes). Thus, in theory, our waves can exist and propagate in the layer. Having plotted the new N_{BV} (see answer to next question), we are not really sure about the depth of the stable layer (from surface), and in the new version of the manuscript, we do not give an exact layer where GWs can be propagating (since we are not really sure).

In fact, we tried to plot the vertical wavenumber (m^2) to show possible wave trapping. However, we only have the temperature and wind profiles around 19.55 UTC (tethered balloon sounding). At this time, we were not able to obtain clear wave parameters with cross-correlation measurements. The wave parameters shows a relatively “large range” of values, indicating that the features of the waves are not very clear (see also wave parameters for wave event 1 in Table 1* (new Table 3)).

For example, if we use $\lambda=20 \text{ km}$; and phase speed of 15 m s^{-1} , we obtain the vertical wavenumber (m^2) profile shown in Figure 2*. In this case, the m^2 profile indicates trapping between the surface and approximately 100 m agl, with another favourable layer for wave propagation from 120 m to 250 m agl approximately. This profile is highly influenced by the dynamical term of the m^2 equation, since the wind direction changes a lot from surface (from SE) to 100 m agl (from NE), and in addition there is a LLJ around 100 m agl. In fact, around 100 m agl, the wind is blowing from NE to SW, while gravity waves are supposedly propagating to E (90°), i.e. they are almost in opposition with wind and the duct layer is not favourable (apart from the influence of the light LLJ). However, we cannot be sure that this height acts as a critical level for wave reflection (ducting from surface to 100 m agl). As said before, GWs parameters are not clear enough to calculate m^2 , and therefore, we cannot ensure details about GWs propagation. For this reason, we did not add the m^2 profile to the manuscript. In

this new version, we add a paragraph to let the readers know that we cannot say more about propagation with the available data.

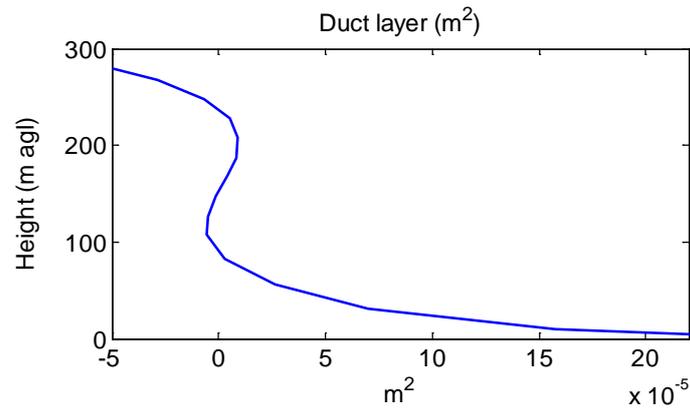


Figure 2*. m^2 calculated with wind and temperature measurements at 19.55 UTC. Positive values indicates favourable duct layer for gravity waves of $\lambda=20$ km; $c=15$ m s^{-1} and phase speed of 15 m s^{-1}

We have the feeling that the layer where GWs can propagate is constantly changing (there is a SDF close to the surface, a LLJ around 100 m agl, then the deeper mountain-plain wind changes the wind profile...). Therefore the GWs features are influenced by these changes (let's say that the duct layer is changing and affecting the GWs parameters, such as phase speed, wavelength...). In any case, this is just speculation and it cannot be demonstrated with the available data.

3) Figure 8. The profiles of N look quite noisy, while the temperature profile appears relatively smooth. Why? Is this a result of how N is being calculated?

Yes, it is. Reviewer #1 asked the same question and the response is similar for both reviewers.

N_{BV} has been calculated from temperature measurements (potential temperature) at different heights. In fact, the temperature profile shown in Figure 8c is not as smooth as it seems, since it includes narrow unstable layers (and therefore some narrow layers have $N_{BV} = 0$ s^{-1}). 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. However, the heights of these measurements are obtained through GPS and include some uncertainty. These two reasons cause the noisy behaviour of N_{BV} profile. In addition, in two cases, the calculation of these mean values includes several measurements taken at approximately the same height (5 data in the same height). It resulted in uncertain unstable layers at some heights (at 100 m agl for example) (the tethered balloon was stopped for some seconds).

To solve this, a new figure has been prepared (Figure 3*, 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_{BV} becomes 0, located around 200 m agl. We have changed the main text and, according to response to last question (ducting), 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 above 200 m agl is not going to be favoured, since the thermal profile is not stable in a shallow layer at that height.

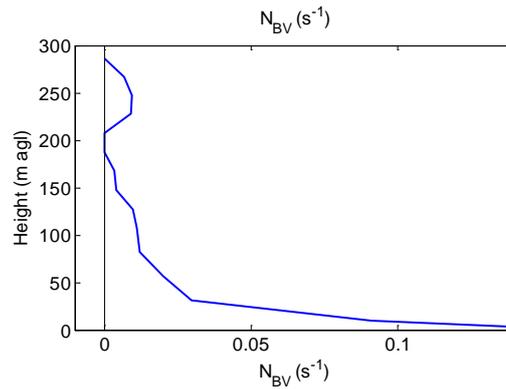


Figure 3*. Brunt-Väisälä frequency (N_{BV}) (s^{-1}) calculated every 20 meters after averaging tethered balloon data.

4) p12834. You may not have measurements of N from the field, but you do have the WRF simulation here. How do the profiles through the deep drainage flow look in the model, and are they consistent with the observed waves during event 2? This may be difficult, depending on how good the model is, but it would be worth checking.

It is indeed a nice suggestion, but maybe this mesoscale model is not appropriate to detect GWs propagation of such characteristics (relatively small wavelengths and periods). Figure 4* shows the evolution of N_{BV} from surface up to 500 m agl from 12.00 to 24.00 UTC of 2nd July. Red colours show $N_{BV} > 0.0017 s^{-1}$ and blue regions $N_{BV} < 0.0017 s^{-1}$. This frequency is the highest frequency of the detected waves during the whole period (~ 10 minutes of period). It can be observed how these gravity waves (and GWs with larger periods) can exist and propagate in the whole layer from approximately 17.00 UTC onwards. Thus, the model is consistent with the existence of GWs of this type, but maybe it is not appropriate to detect narrow layers with possible ducting.

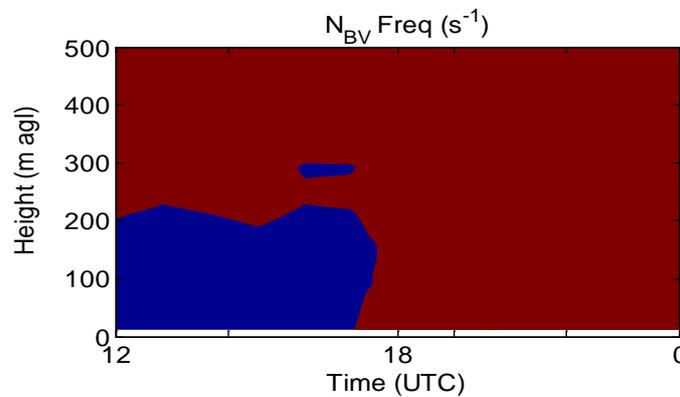


Figure 4*. Brunt Väisälä frequency calculated from potential temperature output of WRF model over Lannemezan. Red colours indicate $N_{BV} > 0.0017 s^{-1}$ and blue colours $N_{BV} < 0.0017 s^{-1}$. This frequency is the highest frequency of the detected waves during the whole period (~ 10 minutes of period). Thus, red colours indicate regions where the propagation is possible for gravity waves with periods larger than 10 minutes.

5) p12835. Throughout you discuss analysis of the surface fluxes, and plot up friction velocity. A pedantic point perhaps, but friction velocity is not a flux, but (depending on definition – not given in this case) the square root of the absolute value of the momentum flux. There are advantages and disadvantages to using friction velocity rather than the momentum flux. If you decide to stick with friction velocity, then please make sure the text does not imply this is a flux.

We do agree. We have revised completely the text to avoid misunderstandings. We are going to include also the definition of friction velocity (Eq. 1) in the new version of the manuscript:

$$u_* = ((-\overline{u'w'})^2 + (-\overline{v'w'})^2)^{0.25} \quad \text{Eq. 1}$$

6) p12836. The heat flux values given in the captions of figures 10 and 12 are K m s⁻¹. If this is correct, then these are not heat fluxes, but temperature fluxes. Can you confirm which they are, and ensure the correct term / units are used throughout.

The units are K m s⁻¹, which are indeed temperature fluxes (mean of w't') or kinematic heat flux (Stull, 1988, page 48), since we do not include the multiplication by density and c_p. The term has been revised and changed (kinematic heat flux) in the whole text, figures and caption of new version.

7) Section 3.4. There are some intriguing differences here. You mention differences in moisture, but I wonder if the canopy nature of the wheat plays a role here? See for example the literature on nocturnal drainage flows in canopies. I was also interested in the strong differences over the edge site. I do not have a clear enough picture of what the edge site was like to draw real conclusions, but assuming it is hedge like then this perhaps points to the strong impact of features like this on drainage flows, cooling and turbulence in stable boundary layers. In such shallow flows hedges can have an important radiative and thermodynamic impact, as well as a significant dynamical impact on the wind and turbulence. I've seen examples of this myself.

We do agree. The differences found between sites are quite surprising and this manifests the important effect of these small heterogeneities of the terrain during SBL and especially during the formation of very shallow and local drainage flows.

In the first version of the manuscript, we wrote *“the heat flux changes from upward to downward considerably later at the Wheat Site than at the other sites. The wheat was drier in this season and therefore the daytime convection is more intensive and the decay takes longer”*. It is true that not only the humidity is involved, the “characteristics of the wheat canopy” can also play a role, since the radiative cooling is hampered by the height of the wheat.

Regarding the second query, it is true that the hedge at the boundary site is influencing the flow (it was a small ditch composed by a different kind of vegetation, although there was a line of three trees close to the boundary tower, see Figures 5a* and 5b*).

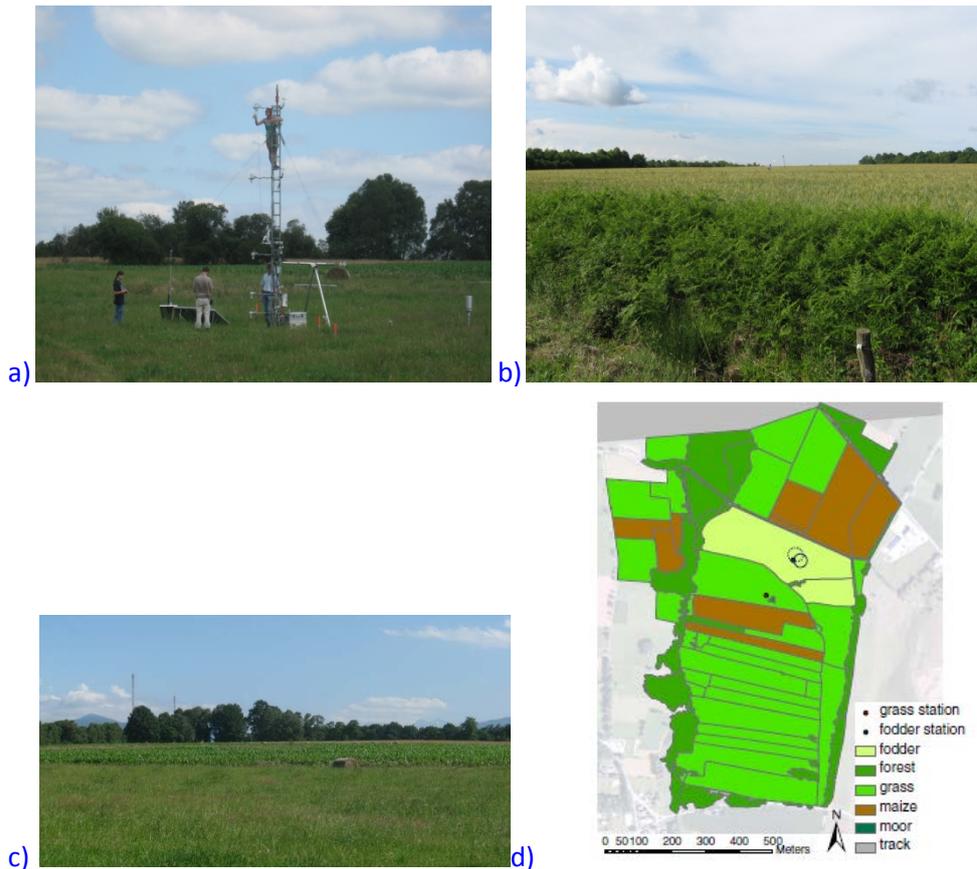


Figure 5*. a) Grass tower with line of three trees in the background (to the SE). b) Vegetation composing the boundary site (note that this kind of vegetation is harder than wheat, located in the background). c) Maize field located to the south of the grass site and line of trees at the background (SE). d) Land-use map from van de Boer et al. (2014).

On the other hand, the maize field located to the south (upwind) of the grass site (see Figure 5c* and 5d*), could also be influencing the low wind measured at the grass site. Then, turbulence is increased by collision of the flow with the boundary site and then the flow is different at the wheat.

These considerations have been taken into account and all these hypotheses will be included in the text (Section 3.4), also following recommendations from Reviewer #1.

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.

8) The MRFD technique is certainly a nice way of looking at the contributions of different scales to the flow. Do you see some of this scale separation in other techniques, such as more traditional ogive plots?

In fact, MRFD is calculated through the differences between cumulative multi-resolution fluxes for different scales, which is similar to ogive plots. These differences are calculated to obtain the contribution of every range of scales instead of cumulative fluxes. The only difference between ogives and the multi-resolution method (cumulative) is that ogives are calculated using the spectral decomposition of Fourier (sin and cos) and multi-resolution is calculated using the spectral decomposition of Haar basis set. Therefore, we think that the results should be quite similar.

9) I found the conclusions about what eddy covariance averaging time you should use in this case a bit unsatisfactory. It read a bit as if "you should definitely not use an averaging time of more than 60s, so you don't include the wave contributions, unless the wave contributions are wave-generated turbulence, in which case you probably do want to include them." This is not terribly useful for the user who wants to process their flux data. Can you provide more discussion on this? There is a body of previous work on wave - turbulence interactions, included papers by some of the co-authors. While it is still certainly an open question, placing these findings in the context of other work might be useful. You might want to look at including Durden et al. (2013), *Biogeosciences*.

We do recognize that our discussion did not provide specific recommendations. However, after long discussions among co-authors, we concluded that we cannot ensure that fluxes contribution from scales larger than 60 s is not turbulent (at least, we have not a common opinion). Any definite conclusions that might come from this site, may not apply to other datasets.

It is true that there is a relatively clear spectral gap around 60 s (see for example Figure 9) and two maxima (one around 5 s (turbulent) and the other centred in larger scales (between 3 and 14 min)). The waves that we detected with the array of microbarometers have periods between 10.5 and 25 minutes, therefore, contributions from periods below these wave periods are not strictly related to the oscillations due to gravity waves. We think that these motions could be caused by transfer of energy from gravity waves to the larger turbulent scales (wave breaking). These potentially turbulent motions are separated (spectral gap) from smaller-scale turbulence generated by other mechanisms (shear, SDF...). This is just a hypothesis and it cannot be demonstrated in the present study. Thus, we are not sure if these motions are non-diffusive (strictly due to gravity waves) or diffusive (turbulent motions but generated by gravity-waves breaking in a chaotic complex atmosphere).

The suggested reference of Durden et al. (2013) presents an interesting case study of influences of gravity waves on fluxes. In this study, the authors concluded that gravity waves are always causing an overestimation of the fluxes (as in Nappo et al. (2008)) if the wave contribution is not removed. We do agree with this theory, however, a question arises when considering periods higher than turbulence but lower than the detected gravity waves. In any case, in our opinion (and in our case study), it is not clear enough if these contributions from scales larger than 60 s are turbulent or not.

We have included this discussion in the conclusion section of the new manuscript, but we include it as an open question, since it could not be demonstrated for our cases. Additionally, we include these two new references (Durden et al. (2013) and Nappo et al. (2008)) along with this discussion.

REF -- Durden, D. J., Nappo, C. J., Leclerc, M. Y., Duarte, H. F., Zhang, G., Parker, M. J., and Kurzeja, R. J.: On the impact of wave-like disturbances on turbulent fluxes and turbulence statistics in nighttime conditions: A case study, *Biogeosciences*, 10, 8433–8443, doi: 10.5194/bg-10-8433-2013, 2013

REF -- Nappo, C. J., Miller, D. R., and Hiscox, A. L.: Wave-modified flux and plume dispersion in the stable boundary layer, *Bound.-Lay. Meteorol.*, 129, 211–223, 2008.

MINOR / EDITORIAL COMMENTS

10) p12825, lines 1-10. In this discussion of recent work on drainage flows, it might be worth mentioning several significant recent field campaigns (PCAPS - Lareau et al 2013 BAMS, METCRAX - Whiteman et al 2008 BAMS and COLPEX - Price et al, 2011 BAMS) focussing on cold air pooling at different scales and their interaction with other processes.

The references to these campaigns have been included in this part of the text.

11) p12830, lines 25-28. "Nevertheless, surface heterogeneities and differences in local slope between BLLAST sites led to differences in thickness and persistence of the SDFs from one location to another (Fig 4), ..."

This sentence has been changed following the suggestion.

12) p12836, line 6. "these contributions ... are clearly separated..." I didn't find this very clear. I don't know if it is the color scheme used in the contour plots, but a number of the features of these MDFD plots were not as obvious to me as the text implied.

We agree that maybe it is not so obvious looking at the MRFD plot and we give an approximate description (in order not to add more plots). What we want to highlight is that there is a maximum centred in turbulent scales (around 2 s) and the other centred in larger scales (around 300 s). In the middle, there is a minimum (spectral gap), which is located around 20 s. It is true that this gap is fluctuating (between 10-60 s). Figure 6* shows the MRFD averaged for the period from 2000 UTC to 2130 UTC. In this graphic, this spectral gap is observed around 20 s (note that this figure is showing the same as Figure 9 (MRFD), but calculated for only for the shorter time interval from 2000 UTC to 2130 UTC).

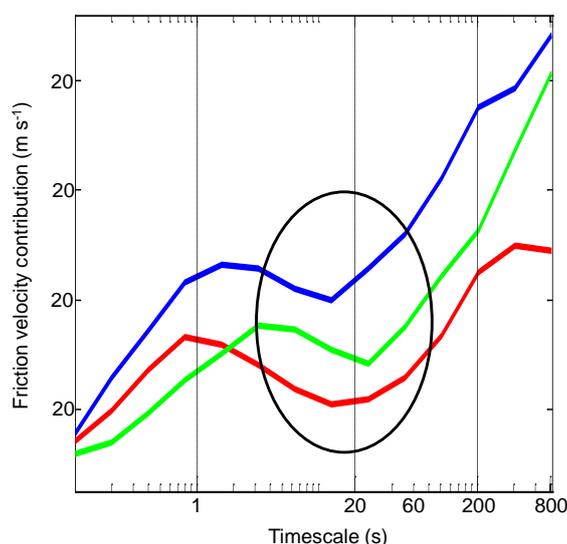


Figure 6*. Friction velocity MRFD (m s^{-1}) averaged for the period comprising from 2000 UTC to 2130 UTC at 0.8 (blue), 2 (red) and 8 m (green) agl. The spectral gap is highlighted with a black ellipse.

13) p12836, line 23. ".. as a consequence of the increase..."

This sentence has been changed following the suggestion.

14) p12838, line 3. "Again, the selection of a larger averaging window..."

This sentence has been changed following the suggestion.

15) p12838, line 14. The phrases "a different kind of vegetation" is not very helpful. Please include a better description of this edge site since this is rather important in interpreting the results. Is it a hedge? Trees? What height? How dense?

It was a small ditch composed by denser vegetation (harder), although there was a line of three trees close to the boundary tower (see Figures 5a and 5b).

16) p12840, lines 22-24. I didn't understand this sentence. How does an increase in mechanical turbulence related to a reduction of large eddies above? Is it not perhaps that the increased mechanical turbulence leads to a reduce temperature gradient and hence a reduced heat flux?

Yes, it is. The sentence was not correctly expressed and it has been changed following the suggestion.

17) p12842, line 16. "MRFD" not "MRDF"?

It has been changed.

18) Figure 3. The caption does not say where these profiles were taken. Also, I would mark on the data points with a symbol so it is clear at which heights the observations are taken rather than just plotting a solid line.

This information has been included in the caption. Profiles are calculated with measurements from the divergence site tower (below 8 m) and from the 60 m tower. Marks have been added to the lines.

19) Figure 5. Again, the caption does not say where this profile is taken.

It has been added. It is over Lannemezan.

20) Figure 6. I found it almost impossible to distinguish the red and purple lines on top of the color contour plot. Perhaps choose a different color, or just stick with solid / dashed lines?

It has been changed in the new version. Now we use a solid red line and dashed black line

21) Figure 8. It is not at all clear from the figure or the text how the measurements at 8 and 60m are integrated in with the tethered balloon profile. Can you plot these as point symbols on these figures? Presumably the line is from the tethered balloon?

In the new figure, we have added marks to the lines (showing where a measurement is taken) and two horizontal lines are included separating the 8m tower, 60m tower and tethered balloon measurements.

22) As a general comment all the figures had rather small labels on the axes / legend which made them hard to read. I would suggest using a larger font size for all these labels.

All the font sizes of axes, labels and legends have been changed.