Answers to Referee 2: Observing local turbulence and anisotropy during the afternoon transition with an unmanned aerial system - a case study

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The authors would like to thank the anonymous referee for his/her suggestions. According to your and the other reviewers' requests, a further analysis of the BLLAST observations during the afternoon and evening transition has been made for 2 July 2011. Now UHF, soundings and tower measurements (all of them in site 1) are taken, apart from observations from M^2AV (site 1) and frequent soundings (site 2) already shown in the previous version of the manuscript. Furthermore, results from high-resolution mesoscale simulations are used to better describe the evolution of the LLJ and the anisotropy ratio during the evening transition of 2 July 2011 in Lannemezan. As a result, find below the main changes in the revised version of the manuscript, some of them directly answering your requests.

– The 3 new co-authors (M.A. Jiménez, J. Cuxart and D. Martínez) have performed the numerical simulations and data analysis, apart from contributing to the discussion of the new results.

– The manuscript has a different organization. The introduction is re-written according to the reviewers' requests (now the turbulence and anisotropy during the evening transition is described, as well as the low-level jet, LLJ). The next section is devoted to the observations and model setup. The atmospheric situation and the features of the observed LLJ are described in sections 3 and 4, respectively, and in section 5 the anisotropy is evaluated. Finally, the discussion of the results is shown in section 6 and the conclusions in section 7.

– The title has now changed into "A study of local turbulence and anisotropy during the afternoon and evening transition with an unmanned aerial system and a mesoscale simulation"

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Find below the answer to your requests point by point. The text from the review is given in italic letters, the answers are provided in normal letters. Changes to the text of the manuscript are indicated in quotation marks.

1 Answers to general comments

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The research topic as well as the available data could provide an interesting contribution to the field of boundary layer research. From the title I expected a comprehensive analysis of the conditions and processes during AT relating turbulent and mean quantities. Unfortunately, the presented analysis and discussion do not fulfil my expectations.

30 We appreciate that the referee considers our research topic and data as interesting. In the revised version of the manuscript a deeper analysis of the processes that take place during the AT is made. Data from other sources (soundings, UHF and tower) and a mesoscale simulation of this case further characterise the mean quantities and the turbulence in the lower atmosphere.

35 *I am missing a clearly formulated research question and motivation for this study.*
The scientific questions are now stated at the end of the introduction. M^2AV measurements taken during the BLLAST experimental field campaign, together with other sources of data and results from a mesoscale simulation, are used in this work to further understand the processes involved during the afternoon and evening transitions with special attention to:

40 (1) characterize the evolution of the ABL, specially in the lower levels (up to 400 AGL),
(2) evaluate the changes in the turbulent scales and their implication in the isotropy of the fields and
(3) study the influence of a nocturnal low-level jet on the turbulence properties.

The quality of the analysis is fairly poor. The authors describe profiles of temperature and hori-
45 *zontal wind in an unnecessary longish way (separately for the various instruments without a critical discussion/explanation of the existing differences).*
In the revised version, the description of the ABL development is shortened substantially, but it includes additional data and numerical simulations with the model MesoNH. The differences between the datasets due to their location (there were two main sites during the BLLAST experimental field
50 campaign) is now included in the description of the results.

The turbulence quantities, which are the new and interesting part of the analysis, are described very briefly. The discussion of these should be more detailed.
Now sections 5 and 6 are devoted to describe the observed and simulated turbulence. Turbulent
55 M^2AV observations are now complemented with the tower observations and the mesoscale simu-

lation results, with the three sources of data producing consistent results. On the other hand, the anisotropy ratio (A) is now computed from the tower and the model results and they are compared to those already computed from the M^2AV observations.

We see that in the daytime the anisotropy ratio (A) provided by the sources are very similar and slightly above 1, in coherence with a dry sheared convective boundary layer. At the evening transition, A takes larger values (a factor between 2 and 5), very likely because the contribution of the convection weakens significantly and the eddies become progressively shallower and more elongated. The beginning of the Inertial Subrange (IS) of the spectrum shifts to the right, and after sunset the eddies have relatively shallow dimensions and are elongated along the main wind direction (Mason and Thomson, 1987), showing large values of anisotropy at these scales. If a LLJ is present (maximum of wind speed at lower levels and close to the top of the temperature inversion), eddies are even more elongated and therefore A increases. At night the values of A diverge depending on the scale and the source of data (new figure 7a). The model resolved motions at the height of the LLJ is just as anisotrope as in the transition, not feeling significantly the effects of thermal stratification at those levels. Instead, the M^2AV and the tower, that measure at smaller scales, provide much higher values of anisotropy, indicating that at the smaller scales variability in the horizontal is significantly larger than at the vertical, therefore indicating that thermal stratification may be playing an important role, moving the upper limit of the IS to very small eddies.
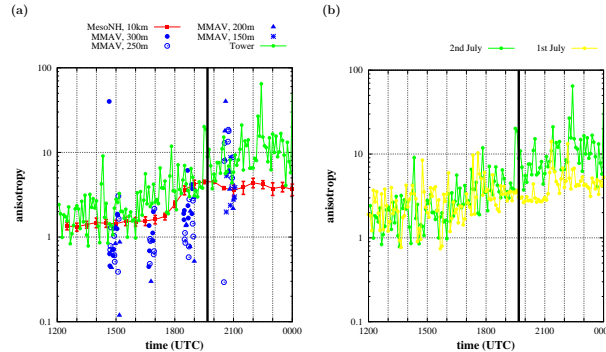

Figure 7. (a) Time series of the anisotropy computed from different sources: M^2AV flight observations during the four flights for different heights (in blue); tower measurements at 60 m AGL every 5 min (in green); and model results averaged between 150 m and 300 m AGL to be close to the altitudes of the M^2AV observations considering a spatial area of 10 km x 10 km centered at Lannemezan (in red). The same in (b) but computed from tower observations during the IOP9 (1 July, no LLJ) and IOP10 (2 July, with LLJ). The time of sunset is represented with a black vertical line. Note the logarithmic scale on the y-axis.

The authors claim that a nocturnal low-level jet develops after sunset and affects TKE and anisotropy. Although this might be the case, the analysis of the jet including its formation and spatial inhomogeneity is not complete and raises more questions instead of answering them.

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We agree with your comment. Now the analysis of the low-level jet and the anisotropy is extended and sections 5 and 6 are focused on this. Now Figure 7b (see above) shows the anisotropy ratio computed from tower observations of the studied case (2 July) and the one of 1 July (no LLJ during the evening transition). It is found that the anisotropy is larger when the LLJ is present, although in both cases the anisotropy increases during the evening transition when the eddies elongate in the wind direction, as it was described in Mason and Thomson (1987).

The quality of the figures is not sufficient and English should be checked by a native speaker.
All figures have been replaced according to the detailed suggestions of the three referees, and also incorporating the additional data and simulations. A native speaker has gone through the English.

Much to my regret, I think that the current version of the manuscript is not suitable for publication in ACP. However, I encourage the authors to revise their manuscript introducing a clear research questions and significantly improving the presentation quality. Below I provide specific comments to the major flaws of the manuscript, which could help the authors to provide a new version.
We would like to thank the referee again for his valuable and detailed comments and the encouragement to submit an improved revised version.

2 Answers to specific comments

1. The title does not match the analysis conducted in the manuscript. As the title is promising I recommend revising the manuscript to match the title.
We suggest to change the title to "A study of local turbulence and anisotropy during the afternoon and evening transition with an unmanned aerial system and mesoscale simulation" The new version of the manuscript includes numerical simulations to expand the airborne observations to a larger scale. With these additional results, the analysis of the observations are better embedded in the atmospheric context, and more concise conclusions can be drawn from the additional data sources.

2. Introduction and motivation
If the authors analyse the relation between a low-level jet and turbulence I would expect a paragraph on previous work related on this topic in the Introduction. Instead Section 2.3 could be removed as it just recalls text-book knowledge. The authors should keep in mind that with the reduction of surface friction in the evening wind speed above the inversion increases and that not always a low-level jet involves.
According to the deeper analysis of the observations and the mesoscale simulations for the afternoon and evening transitions of 2 July 2011, the structure of the manuscript has now completely changed

to:

Now in the introduction the low-level jet and turbulence are introduced and the current work is put in context regarding the previous ones. Some parts of old section 2.3 are now in the introduction. We agree with your comment on the shear in the lower atmosphere but for the studied case observations and model agree that during the evening a low-level jet was generated at the foothills of the Pyrenees due to the combined effect of the eastern large-scale wind and the mountain-plain circulation. This is further described in section 3.

Furthermore, a clear research question needs to be formulated, .e.g. how do turbulence characteristics change during AT and in the stable boundary layer? Or, what is the relation between turbulence characteristics and stability and/or energy balance at the Earth's surface?

The scientific questions are already listed on page 2 of this document.

3. Section 2

The use of UAV measurement to derive turbulence quantities in the boundary layer is a promising approach. As the technique is still fairly new and not every reader is familiar with UAV measurements, I would wish for a rather detailed description of the method and especially giving information about flight legs (length, duration, ground speed) ideally including clear figures or a scheme of the conducted flights. At the moment, some information about the flight legs are given at various parts in the manuscript which makes it a little confusing (e.g. l. 313).

We agree with the referee that all information on the M^{2}AV should be in the same place. It is now all included in the Section "Field site and instrumentation". The figure illustrating the flight path is now included in a topographic map used for the simulations, as also suggested by another referee. The information about the UAV is now as follows:

"The M^{2}AV is an unmanned aerial vehicle with a wing span of 2 m and a weight of 6 kg. It is started

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150 and landed manually, and can be fully controlled during the mission by an autopilot system. For this
case study, most ascents and descents as well as the race track pattern with straight horizontal legs
were flown with the autopilot. The flight track is shown in Fig.1b. The M^2AV is equipped with a
miniaturized turbulence measurement payload comprising a 5-hole probe for deriving the angle of
attack and sideslip in the aerodynamic coordinate system. The data can then be converted to the 3D

155 wind vector in the geodetic coordinate system using precise information on position and attitude of
the aircraft obtained by GPS and an inertial measurement unit (IMU). The application of the method
for unmanned aircraft is demonstrated by van den Kroonenberg et al. (2008). Further, the payload
comprises a slow and accurate as well as a fast temperature sensor, and a capacitive humidity sensor
(Martin et al., 2011). The parameters measured by the M^2AV (profiles of temperature, humidity,

160 wind speed and wind direction, as well as turbulent fluxes of sensible heat) have been validated
extensively against other airborne data sets (Spiess et al., 2007), as well as in situ meteorological
tower and remote sensing observations (Martin et al., 2011; Cuxart et al., 2012). The system has
been deployed for high resolution atmospheric profiling (Martin et al., 2011; Jonassen et al., 2015)
and for deriving turbulent parameters (van den Kroonenberg et al., 2012; Martin et al., 2014; Martin

165 and Bange, 2014) worldwide at various locations.
The analyses focus on a case study for 2 July 2011. For this study, the M^2AV performed vertical
profiles, and followed race track patterns of about 1 km length for deriving turbulent parameters of
the 3D wind vector. The profiles were performed with an ascent rate of about $3.5\,\mathrm{m\,s^{-1}}$, the descent
rate was about $8\,\mathrm{m\,s^{-1}}$. The race track pattern consisted of three legs at one altitude (300 m) oriented

170 in East-West and West-East direction, then three legs at a second altitude (250 m) and two legs at a
third altitude (200 m). The same pattern was repeated three times for each flight. Note that the time
for one flight leg is only about 45 s at the aircraft speed of $22\,\mathrm{m\,s^{-1}}$, therefore providing an instanta-
neous snapshot of the turbulence properties. The same flight track was employed during four distinct
flights starting around 1430 UTC, 1630, 1830 and 2030 UTC. A single flight lasted approximately

175 40 min. Only Flight 2 was shorter due to a failure of the autopilot around 20 min after takeoff. The
exact times for takeoff and landing are given in Table 1. During the last flight, the altitudes for the
race track pattern were reduced by 50 m in response to the lower ABL height."

Also the impact of the different filtering methods could be illustrated using figures. What are these
180 *slowly changing structures? Gravity waves require stable stratification? Is this given?*
To avoid confusion, we just present the method that was used for data processing in the revised ver-
sion:
"Visual inspection of the time series of $v\prime$ and $w\prime$ revealed several cases with wave like slowly
changing structures (wavelength around 2 km) of relatively large amplitude compared to the fast

185 fluctuations. They have a high impact on the variance calculation. The variances of wind speed σ_v^2
and σ_w^2 were calculated by employing a high pass Butterworth filter of third order, with different

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cutoff frequencies tested, which resulted in more realistic values than linear detrending. By the high pass filtering technique, these longwave features disappeared. In any case, the flight legs were not long enough for obtaining statistically relevant information about wavelengths larger than the double of the flight leg. Therefore, a 0.01 Hz high pass filtering was finally applied in this case study, instead of removal of a linear trend from the wind components."

About your question of the presence of gravity waves, during the first 3 flights, stably stratified conditions were not present yet in the ABL (they are during the afternoon and early evening transition, when surface cooling starts) and therefore no ambient conditions were favourable to develop gravity waves in the ABL. However, during the last flight (about 2100 UTC) cooling is stronger and Román-Cascón et al. (2015) found gravity waves between 2030-2130 UTC close to the surface (they are analysing surface pressure and other atmospheric magnitudes up to 2 m AGL). Figure 8 in Román-Cascón et al. (2015) (vertical profile of the observed Brunt-Vaisala frequency) shows that gravity waves can be found up to about 100 m AGL but not at higher levels (where the M^2AV sampled). Later on (from 2100 UTC to midnight), gravity waves could be found but the presence of the LLJ does not allow a strong nocturnal cooling (strongly stably stratified conditions), departing from the favourable conditions for the development of the gravity waves.

The model is not able to capture these observed gravity waves (Román-Cascón et al. , 2015) since they are attached to the ground. In fact, the model does not see this maximum of wind at 2 m AGL that they report since the first grid level is at 1.5 m. A comment on the presence of gravity waves is included in section 4, when the description of the processes during the studied transition is made. We do not consider that the possible presence of Internal Gravity may alter the discussion on the other relevant issues treated in the paper and we just mention them and refer to (Román-Cascón et al. , 2015) for further information.

The contents of Figure 1 which aims to give and overview of the measurement area are hard to see. I recommend providing two subfigures, one giving a general overview of the area including UHF site 1 and 2 and the launch site of the frequent radiosondes and the UAVS and the other one zooming in on the UAV site showing the flight tracks.

We changed former figure 1 to a new one with two panels. One is showing the topography as represented in the model and the other one the observation sites and the flight tracks. We think that now the sampled area is better described with the plots and the corresponding comments in section 2.

As mentioned above the section about low-level jets is out of place here and should be moved to the introduction.

We agree with your comment and we have re-structured the manuscript (see above), and the LLJ

section is now part of the introduction, as suggested.

225   *4. Analysis of the measurements Sections 3 and 4 should be reorganized.*
We agree with the referee, see new structure presented above.

In the following I propose a possible outline for an analysis:
- The detailed description of synoptic conditions even including a figure is not necessary for the
230   *analysis. In my opinion, a brief narrative description of synoptic conditions is sufficient.*
We agree with your comment and we have removed the figure of the synoptic conditions. The description of the synoptic conditions is made in section 3.

- As AT is defined via the surface sensible heat flux, the authors could start with showing a time
235   *series of the energy balance components at the Earth's surface. At the moment it is not quite clear*
which of the flights are within AT and which are not.
We agree with your comment. The time series of the model results, together with the M^2AV and the
60 m tower observations, are shown in the new figure 3 (see below). The sunset time is also indicated.
By inspecting the temporal evolution of the wind, temperature or TKE it is possible to characterise
240   the period when the M^2AV flights were performed and we think that it is not necessary to add the
observed energy balance components at the Earth's surface.

- To show the evolution of the boundary layer during the afternoon and evening, the authors could
show profiles of potential temperature and horizontal wind for specific times. Instead of showing
245   *profiles separately for each instrument, the different instruments (UAV, UHF and frequent radioson-*
des) could be shown together in one plot. This would allow to compare the instruments as well as
eventual spatial differences. So, Figs. 3-8 could be combined in a few precise figures combining the
information.
We composed new figures as suggested. For each M^2AV flight time, subplots are shown that include
250   data of UAV, UHF, radiosondes, tower, and numerical simulations. We did this for potential temperature, horizontal wind speed, and wind direction. See below new figure 5.

For better visibility wind direction could be plotted as dots or even as vectors (Figure 8 is a mess).
Also, the data should be quality checked and outliers should be removed, e.g. for the frequent ra-
255   *diosondes. It is hard to believe that the wind speed of more than 10 m/s near the ground measured*
by the UAV at 2027 UTC is realistic, given that it is not measured by any of the other instruments.
As mentioned above, these plots are now put together in the new figure 5. We agree that the M^2AV
observed wind speed at 2027 UTC seems not realistic in comparison to the sounding and UHF observations, although the wind direction is comparable to the other sources of data. Therefore, only
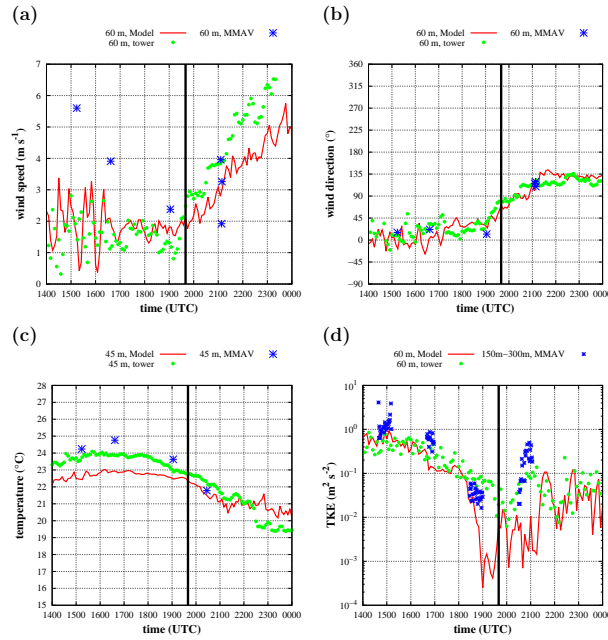
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Figure 3. Modelled and observed time series for (a) wind speed (in $m\,s^{-1}$), (b) wind direction (in $^\circ$), (c) temperature (in $^\circ$C) and (d) TKE (in $m^2\,s^{-2}$) from 15:00 UTC until midnight on 2 July 2011. Tower observations are in green circles, model results in red lines and M^2AV data in blue asterisks. The temporal evolution of wind and temperature data from M^2AV is constructed with the values of the vertical profiles taken at the corresponding height of the tower measurements. For TKE, all the M^2AV legs where TKE is derived, at 150 m, 200 m, 250 m and 300 m AGL, are included in the plot. The time of sunset is represented with a black vertical line.

the last M^2AV profile at 2110 UTC is used (new figure 5d) where the observations are comparable to the other sources of data and the model results.

Wind speed measured during the ascent and descent of the frequent radiosondes vary a lot, which suggests an impact of vertical motion of the sonde on the measurements. This should be discussed.

We included in the text:

"Differences between profiles of an ascent and consecutive descent might arise from a temporal development of the atmosphere, with a typical time span of 10 min between half of the ascent and half of the descent, and the different location (up to 10 km from launching to landing site during BLLAST). In contrast to the standard radiosonde technique, the use of double balloon systems tends to stabilize the ascent of the payload, as it reduces pendulum motion (Kräuchi et al., 2016)."

- When showing profiles of potential temperature the evolution of the boundary layer height zi could be illustrated.

During the afternoon and evening transitions z_i changes as time progresses and for this reason the
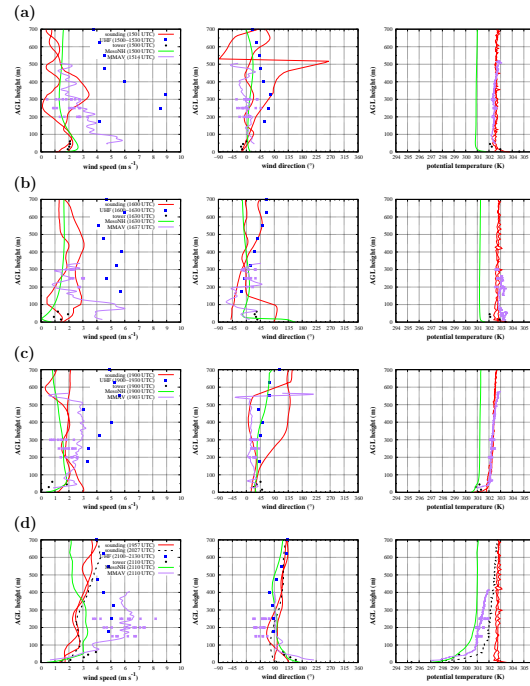
Figure 5. Vertical profiles of wind direction (in $°$) on the left, wind speed (in $\mathrm{m\,s^{-1}}$) in the center and potential temperature (in K) on the right, from $\mathrm{M^2AV}$ (in violet) for the four flights of 2 July 2011: (a) 1500, (b) 1630, (c) 1900 and (d) 2110 UTC. Vertical lines and dots correspond to instantaneous values from the vertical profiles and to mean values for each horizontal leg, respectively. $\mathrm{M^2AV}$ data are compared against instantaneous observations from UHF (blue squares), tower (black dots), and frequent (red) and standard soundings (black) together with mesoscale simulation results (green). The legend indicates the corresponding times to each data source.

275   height where $\mathrm{M^2AV}$ sampled is indicated with "m AGL". More information about the evolution of z_i for the different IOPs during BLLAST are shown in Lothon et al. (2014).

Line 183-185: what does it mean that the residual layer is lower than the ABL top? Normally, the top of the residual layers coincides with the top of the former ABL top. The bottom of the residual
280   *layer is indicated by the top of the surface inversion*
We agree that the text was unclear and this sentence is removed in the revised version. The height of the residual layer and the depth of the surface inversion are clearly seen in Figure 5.

- The evolution of a low-level jet could be shown in this paragraph as well where the mean con-
285   *ditions are analysed. In this context the wind profiles should be checked for the criterions used to identify low-level jets (e.g. Stull, 1988), as a wind speed of 6 m/s is not very strong.*
The description of the observed low-level jet features is made now in section 4, together with the mesoscale simulation results. We agree that a wind speed of $6\,\mathrm{m\,s^{-1}}$ is not very strong. However, we

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explain now in the introduction that we use the criteria of Baas et al. (2009), where a LLJ is defined according to the wind speed difference between the maximum wind speed and the minimum wind speed above:

"There are different criteria in the literature for identifying a wind profile as LLJ, e.g. taking into account the maximum wind speed, or a specific decrease of the wind speed above the altitude of the maximum wind speed (Bonner, 1968; Banta, 2008). In this article the definition of Baas et al. (2009) is applied, with the following LLJ criteria: the difference of the wind speed between the maximum and the closest minimum above has to exceed $2 \, \mathrm{m \, s^{-1}}$ and has to be larger than 25% of the maximum wind speed. These criteria have to be met for at least $30 \, \mathrm{min}$ in time."

Fig. 11-13 should be optimized using the same scale on the y-axis and the some color scale. Also Fig. 12 and 13 should be combined. Why do the authors not show measurement of the UHF at site 2 for the whole night? The differences between the various wind speed profiles should be discussed more thoroughly. For me the LLJ establishes only after midnight. Around 2000 UTC there is much temporal variation of the wind speed (e.g. Figure 12) ? quite similar to the period between 1000 to 1900 UTC.

These figures are plotted with the same color scale and vertical axis in the new figure 4. Besides, the same plot is made with the mesoscale model results, indicating that both UHF and model are giving similar patterns. The attention is now focused on the period from 1500 to 0000 UTC on 2 July 2011 and all the temporal series shown in the manuscript are now within this interval.

- The figure of TKE profiles should be improved as it is hard to see the different dots.

We agree with the referee. The figure was replaced by a new figure, including also TKE from tower observations and numerical simulations for the same times as the M^2AV flights.
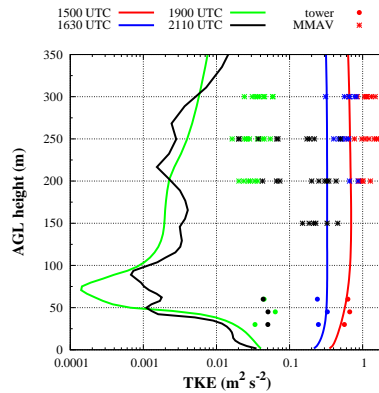

Figure 6. Vertical profiles of the simulated TKE (in lines) at different instants during the M^2AV flights (see legend). Tower (in dots) and M^2AV (in asterisk) observations are also included. Note the logarithmic scale on the x-axis.

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It might also be interesting for the reader to see a time series of instantaneous wind measurements

315 *which were used for TKE calculation. The turbulence characteristics could then be described relating them to the different mean conditions in the ABL, e.g. to stability, wind speed or the Richardson number.*

The time series of $v\prime$ and $w\prime$ revealed several cases with wave like slowly changing structures (wavelength around 2 km) of relatively large amplitude compared to the fast fluctuations. They have a high

320 impact on the variance calculation. The variances of wind speed σ_v^2 and σ_w^2 were calculated by employing a high pass Butterworth filter of third order, with different cutoff frequencies tested, which resulted in more realistic values than linear detrending. By the high pass filtering technique, these longwave features disappeared. In any case, the flight legs were not long enough for obtaining statistically relevant information about wavelengths larger than the double of the flight leg. Therefore, a

325 0.01 Hz high pass filtering was finally applied in this case study, instead of removal of a linear trend from the wind components.

Currently, the authors calculate the Richardson number from the race track patterns. Why do they not use the profiles of wind and temperature?

330 We agree with your point that Ri could be also computed from the M^2AV profiles to have a better description of the turbulence in the lower ABL (see figure below). Nevertheless, in the current version of the manuscript the observed vertical profiles from TKE (tower and M^2AV) and those obtained from the model outputs are further described (see section 5) and for this reason the Ri number to identify when and where turbulence takes places is not longer used. The definition of Ri is removed

335 from old section 2 and the lasts sentences of sections 4.1 and 4.2 are adapted to describe the turbulence without the use of Ri (and in some cases it says Ri, not shown).
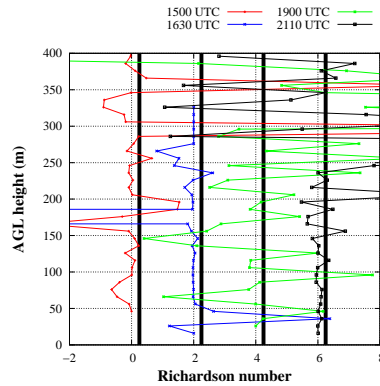

Profiles of Ri computed from the M^2AV profiles. To have a better representation, the x-axis is shifted by 2 to the right every instant. The vertical black line indicated Ric=0.25.

The turbulence characteristics could also be related to the surface fluxes.

We agree with your point. The M^{2}AV TKE is now compared to model outputs and tower observations (see new figure 6 showed above).

Additionally, it would be helpful to provide sigma-w and sigma-u separately in order to see what causes the change in anisotropy sigma-w decrease or sigma-v increase or vice versa. This allows also to see if sigma-w becomes too small at all.

Find below a time series of sigma-u and sigma-w separately computed from M^{2}AV and the 60 m tower observations. It is seen that the increase of the anisotropy after sunset is related to a decrease of sigma-w. This figure is not included in the current version of the manuscript but this result is mentioned in section 5.
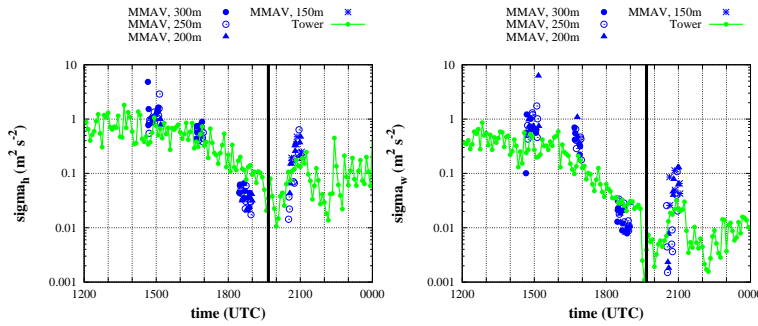
sigma-u and sigma-w separately computed from M^{2}AV and the 60 m tower observations.

- Why do the authors not use information on turbulence from the UHF? It would be interesting to compare them to the turbulence parameters derived from the UAV. Also the UHF data are available during the night and could provide turbulence parameters during the time of the fully developed low-level jet.

UHF provides a qualitative estimation of turbulence, but not TKE to compare to. Data from the 60 m tower and mesoscale simulation are now used instead.

5. Discussion and conclusions

The discussion section could include differences in mean profiles derived with different instruments, problems with the UAV measurement when deriving turbulence quantities, on the dependency of TKE to mean conditions on spatial inhomogeneity in atmospheric conditions, etc.

Differences in mean profiles and turbulence properties are now discussed in the manuscript based on a larger data set of UAV, tower, radiosonde, frequent radiosonde, UHF data, and numerical simulations. The organization of the flow at lower levels is now further explored from the mesoscale simulation with a special attention to the locally-generated winds that are specially important in such a complex terrain region. Besides, problems with the UAV measurement when deriving tur-

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bulence quantities are also mentioned. To improve the understanding of anisotropy probably flights well defined along and across the turbulent elongated structures of longer track would bring much more supplementary information. To do so, efficient use of available information should be made beforehand and determine the high of the flight to use the batteries as optimally as possible to get
370   long tracks. This is now mentioned in conclusions as a suggestion of further work.

The conclusions should be precise and pick up the research questions formulated in the introduction. The authors could also combine the Discussion and Conclusion section.
The conclusion section is re-written to incorporate the new analysis (extra sources of data and model
375   outputs). The main findings of the work are:
(1) during the afternoon transition, TKE decreases as time progresses and it is minimum close to sunset. Afterwards it increases due to the presence of a LLJ.
(2) the shear generated by the presence of a LLJ is the responsible of the increase of the anisotropy during the evening transition, being larger than if no LLJ is present.
380   (3) the anisotropy ratio computed from difference sources of data (model and observations) shows that A depends on the scale (spatial and temporal) of motions included in the data.

6. Literature
The reference list should be adapted more precisely to the research done in the manuscript.
385   We agree that the reference list in the previous version of the manuscript could be improved substantially. Since the introduction is re-written, the references are also updated, for instance those related to turbulence observations, isotropy conditions and studies of LLJ, in all cases from the observation and modelling perspectives.

390   *7. The quality of the figures has to be improved significantly. At the moment, they are not sufficient for publication in a peer-reviewed journal.*
We agree with your comments. Now all figures are improved, adding the new analysis considering extra observations and the mesoscale simulations results and also taking into account the reviewers' suggestions.
395

8. As the manuscript deals with observations sensitive to the time in relation to sunrise, I highly recommend using local time instead of UTC.
We explain why we prefer to use UTC instead of the official time:
"Throughout the article, times are given in UTC, as the study area has the same longitude as Green-
400   wich and therefore the same solar time. The official local time is UTC + 2 h."

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3 Answers to minor comments

Introduction

Lines 20 to 23: The surface sensible heat flux normally decreases shortly after noon. I.e., it is more
405 *than 160 min until sunset.*

You are right. We changed the text to:

"The afternoon transition (AT) is defined differently in the literature, depending e.g. on the obser-
vational techniques and available data sets. Lothon et al. (2014) use the definition of Nadeau et
al. (2011) for the BLLAST (Boundary-Layer Late Afternoon and Sunset Turbulence) experiment,
410 which is also used in this study. According to this definition, the AT begins when the surface sen-
sible heat flux starts to decrease, and ends when the surface sensible heat flux becomes negative,
corresponding to the time before sunset. Another definition of the afternoon-evening transition is
based on the time from the beginning of a decrease in wind speed variance until the beginning of the
building of a temperature inversion, which takes around 160 min during summer (Busse and Knupp,
415 2012)."

Line 25: why does the mixing ratio increase at this time?

The phenomenon is described in detail in Fitzjarrald and Lala (1989). He explains it as the conver-
gence of turbulent moisture fluxes in the surface layer, with a simultaneous heat flux divergence. An
420 easy explanation is that vegetation stops photosynthesis and starts breathing around sunset. As we
do not at all discuss humidity in the article, we prefer not to explain the phenomenon in detail, but
we added another reference in the text:

"The transition usually includes several consecutive changes of near surface parameters: a decrease
of the vertical and horizontal wind speed variance, temperature, thermal fluctuations, and wind
425 speed, as well as a rapid increase of the mixing ratio (Fitzjarrald and Lala, 1989), and finally the
formation of a temperature inversion, e.g. Acevedo and Fitzjarrald (2001); Busse and Knupp (2012);
Bonin et al. (2013)."

Lines 31-35: Why do you mention turbulent flux profiles in Oct and Febr at all? Additionally,
430 *BLLAST was in summer.*

We agree with the referee the sentence is now removed.

Section 2

Refer to the launching time: is this valid for the descents, too?

435 The time of the soundings refer to launching time. Regarding the soundings on site 1, they reach
levels of about 12 km AGL and the sonde was lost afterwards. As a result, only ascents are taken
in the analysis here. On the other hand, frequent radiosoundings on site 2 reach typically heights
of about 2 km with vertical wind speeds of 6 m s^{-1} for the ascent and 3 m s^{-1} for the descent.

Soundings typical last 15 min and for this reason the ascents and descents are both considered in the
plots of figure 5, labelled with the same time. This is now stated in section 2.1.

Line 112: what kind of spline function?
We used a linearly interpolated spline function to smooth the data.

Line 115: how good do the flight track and the mean wind direction agree?
The mean wind direction varies around N during the first three flights. The legs of the race track
patterns were oriented directly in E-W-direction. This is now better indicated in new figure 1.

Line 121: you mention the method: 'removing the mean value' but you did not discuss the results
compared to the other filter methods.
These paragraph is clarified and now read as: "Visual inspection of the time series of vɪ and wɪ
revealed several cases with wave like slowly changing structures (wavelength around 2 km) of rel-
atively large amplitude compared to the fast fluctuations. They have a high impact on the variance
calculation. The variances of wind speed σ_v^2 and σ_w^2 were calculated by employing a high pass
Butterworth filter of third order, with different cutoff frequencies tested, which resulted in more re-
alistic values than linear detrending. By the high pass filtering technique, these longwave features
disappeared. In any case, the flight legs were not long enough for obtaining statistically relevant in-
formation about wavelengths larger than the double of the flight leg. Therefore, a 0.01 Hz high pass
filtering was finally applied in this case study, instead of removal of a linear trend from the wind
components."

Line 130: Do you really eliminate advective contributions? Or large scale contributions? A spec-
tral analysis would be helpful which could be compared with the spectrum of the UHF profiler.
You are right. With the method used we eliminate the larger scale contributions. This is now stated
(see answer above).

Line 158: Do you need a minimum or just a reduction of the wind speed?
The LLJ definition according to Baas et al. (2009) uses a local minimum.

There are a lot of typos: attitude instead of altitude, temperatur instead of temperature, as instead
of at (line 77)
We corrected the typo in temperature; however, it is really our intention to talk about the "attitude"
of the aircraft (roll, pitch, yaw), not the altitude, and we use the "second balloon as a parachute", we
do not employ a parachute for the frequent radiosonde technique.

16

Section 4

Line 199: How heterogeneous are the surface conditions?

Works of Lothon et al. (2014) and Cuxart et al. (2016) show that the surface characteristics and the topography are the responsible of the strong heterogeneity of surface fluxes and surface temperature
480   sampled during the BLLAST campaign. The references to these works are now included in section 2.1.

Line 257-260: Why not use TKE in combination of isolines of the wind speed. This allows seeing directly the zones with strong wind shear and high TKE.

485   Figure 6 is improved (now tower and model results are used) and together with Figure 5 it is clearly seen the evolution of TKE together with the wind speed and direction.

Line 265: What means 'the lowest value of A'?

We now use the word "anisotropy ratio" instead of the notion A.

490

Figure 3 and 4: Either you should use temperature or potential temperature but not both. I would prefer potential temperature (in Figure 4, too).

We now use a plot combining data of different measurement systems, and use potential temperature. See new figure 5.

495

Section 5

Line 340: 'An LLJ increases the horizontal wind speed': the LLJ is the horizontal wind speed!

That sentence was confusing. The description of the evolution of the LLJ is now further explained in section 4 with the extra data analysis and model results.

500

Line 370: When you say that the 'LLJ was distributed inhomogeneously on a small scale of few km' it would be interesting to know why. There must be a lot of divergence and convergence. Did you see that in the UAV and/or UHF data?

The mesoscale simulation is now used to better describe the spatial distribution of the LLJ. See for
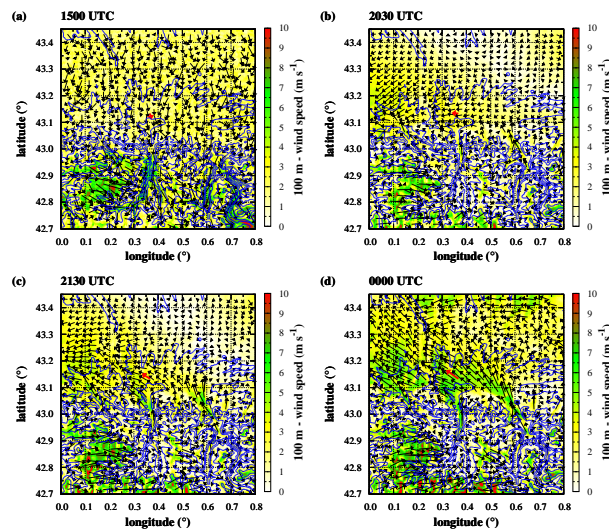505   instance new section 4 and figure 2.

Figure 2. Modelled 100 m AGL wind vectors together with wind speed (in colours) and the topography lines (in blue) at different instants (a) 1500 UTC, (b) 2030 UTC, (c) 2130 UTC, (d) 0000 UTC. The 60 m wind vector observed by the tower is plotted with a red arrow.

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