Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2015-1060-AC3, 2016

Atmospheric Chemistry and Physics Discussions
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Interactive comment on "Observing local turbulence and anisotropy during the afternoon transition with an unmanned aerial system – a case study" *by* A. Lampert et al.

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Received and published: 6 May 2016

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Turbulence anisotropy during afternoon transition

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Answers to Referee 3: Observing local turbulence and anisotropy during the afternoon transition with an unmanned aerial system - a case study

May 6, 2016

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"

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.

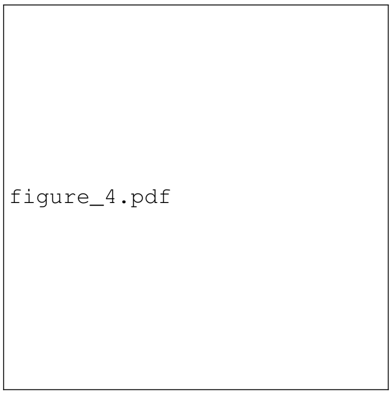
1 Answers to general comments

The afternoon-evening transition and anisotropy still need to be better understood so that they can be accurately represented in numerical models, so the topic is an important one and great contributions could be made. However, the presented analysis raises questions as to the methodology and significance of the results. As the authors claim themselves, many of the TKE estimates are not statistically significant. As such, it is expected that the anisotropy results are not statistically significant in of themselves, as well.
M^2AV observations of turbulence after sunset are only available for the day studied here (2 July, IOP10 of the BLLAST campaign). For this reason the data analysis is

limited to 2 July. Nevertheless, the results obtained here from M^{2}AV are now compared to those reported from other IOPs. It is found that during the IOPs with clear skies and weak wind conditions, the anisotropy ratio increases during the evening transition, but this ratio increases even more when the LLJ is present (only for IOP10, 2 July). Furthermore, TKE and anisotropy data derived from M^{2}AV are now compared to other data sources (tower and mesoscale simulations) and in all cases similar patterns are found.

Additionally, the analysis of the low-level jet is not adequate. Based on the data presented, it appears as though the LLJ itself did not develop until after the last flight was finished, and the main claim about a LLJ being present appears from suspicious data from the UAS itself that needs to be corroborated. Every major topic within this study has significant flaws, degrading the importance of the results.
Now section 4 is devoted to explain the observed LLJ through the mesoscale simulation results and a deeper analysis of soundings, UHF and tower data. According to the new Figure 4 (see below), UHF data indicates that the LLJ started at the begining of the last M^{2}AV flight and new Figure 5 shows the agreement of the different sources of data that are now analysed.

Additionally, much of the paper needs to be rewritten. The section on the M2AV data processing is confusing, as it presents multiple ways in which the data could be processed but ultimately states that only one of the methods was used.
We agree with your point. We completely revised the structure of the manuscript. Now it is as follows:
1. Introduction
2. Observations and model setup
2.1 Field site and instrumentation
2.2 M^{2}AV data processing



new Figure 4. Temporal evolution of the observed (UHF) and modelled vertical profiles for (a) UHF wind direction (in °), (b) UHF wind speed (in $\mathrm{m\,s^{-1}}$), (c) MesoNH wind direction (in colours) and wind speed (in lines, for values $\geq 4\,\mathrm{m\,s^{-1}}$, contour interval = $2\,\mathrm{m\,s^{-1}}$) and (d) MesoNH TKE (in $\mathrm{m^2\,s^{-2}}$). The time of sunset is represented with a black vertical line.

2.3 Model setup
3. Atmospheric situation
4. The nocturnal LLJ as observed and modelled
5. TKE and anisotropy during the afternoon-evening transition
6. Discussion
6.1 Turbulence properties
6.2 Low-level jet as source of enhanced turbulence
7. Conclusions
Particularly, the data processing in section 2.2 has been changed to:
"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 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 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."

Within the results section, too much text (and figures) are dedicated to describing how the mean profiles of wind and temperature changed over time and how they were observed by different instrumentation. Instead, the authors should dedicate more time and figures to the anisotropy discussion, as that (by the title) seems to be the focus of the paper.
We agree with your point. Now a deeper analysis on the turbulence and anisotropy is made in sections 5 and 6, as well as the comparison of the M^2AV results to those obtained from the 60 m tower and the model. Besides, the M^2AV profiles are compared to those sampled by tower, UHF and soundings and those obtained from the model (see new Figure 5 above). It is found that the different sources of data are able to reproduce similar patterns although the spatial and temporal scales of the eddies depend on the sources of data.

It might be helpful to include analysis of the spectra, especially since the authors mention that wave-like features were observed.
The inspection of the spectra for the flights shows that, as stratification develops in the evening, there is more energy at scales below 2 km, but there is no hint of the expected shape of the spectrum in the presence of gravity waves. Román-Cascón et al. (2015) indicated the likely presence of gravity waves that evening, but this is not

confirmed by our data at the levels where M^2AV was flown. Therefore inspection of the spectra is inconclusive and we prefer to refrain to develop this point in the manuscript.

The conclusions section needs to be expanded upon to focus on the main results of the study.
The conclusion section is re-written to incorporate the new analyses (extra sources of data and model 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 responsible for the increase of the anisotropy during the evening transition, being larger than if no LLJ is present.
(3) the anisotropy ratio A 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.

Throughout the whole paper, the authors need to more clearly define what the motivation for the study and their significant findings as they relate to previous studies.
Previous works devoted to turbulence measurements, anisotropy and LLJ are described in the introduction to properly describe the state of the art. Besides, the scientific questions addressed in this work 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:
(1) characterize the evolution of the ABL, specially in the lower levels (up to 400 m AGL),
(2) evaluate the changes in the turbulent scales and their implication in the isotropy and

(3) study the influence of a nocturnal low-level jet on the turbulence properties.

Considering all of the aforementioned problems with the manuscript, I recommend that the manuscript is not acceptable for publication in ACP. However, I recommend that the authors continue to strengthen their analysis of the data, and rewrite/restructure much of the paper for resubmission (these changes to be too significant for 'major revisions').
We improved the manuscript by analyzing additional data sets and we have also incorporated the results from a mesoscale simulation. The whole manuscript was revised and re-structured to strengthen the M^2AV findings.

2 Answers to specific comments

In addition to revising the paper to address the aforementioned issues, the author could significantly improve the manuscript by addressing the following specific concerns: Abstract:
a) Line 4: Define BLLAST within the abstract.
ok

b) Line 5: Either provide flight times in UTC, or sunset in local time (and throughout the entire paper, stick to one convention).
We use UTC throughout the text, as the longitude of the field site is the same as of Greenwich. Therefore, solar time is the same as UTC. We comment on this in the revised manuscript.

c) Line 10: Low-level jet is typically not capitalized

We changed to "low-level jet" throughout the text.

d) In general: Provide the main results/conclusions of the study within the abstract.
We have included in the abstract that the TKE derived from M^2AV is similar to the one obtained from other sources of data (UHF, sounding, 60 m tower and mesoscale simulations). Besides, all sources present an increase of anisotropy after sunset, related to the presence of a low-level jet. The differences of the values of the anisotropy ratio are linked to the different spatial and temporal scales sampled by the 60 m tower, M^2AV and model.

Introduction: e) Line 19: It is stated that there are different definitions for the afternoon-evening transition, but only one is given. Please indicate if the provided definition is the one used here (as the reader assumes it is). It could also be useful to provide an alternative definition, and why it would be used differently.
We now give the definition used in the study, and present an alternative definition:
"The afternoon-evening transition of the atmospheric boundary layer (ABL) describes the processes converting a convective ABL into a stably stratified nocturnal ABL. The afternoon transition (AT) is defined differently in the literature, depending e.g. on the observational 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, which is also used in this study. According to this definition, the AT begins when the surface sensible 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, 2012)."

f) Line 39 (and elsewhere): It is best to use 'larger' rather than 'stronger'.
We changed as suggested.

Also, the statement made here is not always true during unstable stratification. During daytime when the mean wind speed is large, the variance in the horizontal wind is often greater than the variance in the vertical. So please rephrase this statement.
That's true. We modified the text to:
"During unstable stratification and low wind speed, turbulence is mainly generated by buoyancy induced by heating of the Earth's surface, therefore the variance in the vertical wind component is larger than in the horizontal wind components."

g) Line 50: Change terminology from 'turbulently mixed' to 'convective', as turbulent mixing still continues (albeit weaker) during stably stratified conditions.
Thanks for the suggestion. It was taken into account.

h) Line 55: It would be useful to provide information about how the meteorology of the days in this study and the Darbieu case study is different.
We added in the text:
"The two case studies had similar weather conditions (clear skies, high pressure system nearby western Europe and weak wind speed) but the large-scale wind during the day (1200 UTC) was from W on 20 June and from NE on 2 July.

i) Overall, the introduction needs to be better structured. I suggest using the first paragraph to outline the main features of the AT leaving out the discussion of anisotropy (line 31).
The introduction section is also re-written. Firstly, the processes that take place in the afternoon and evening transitions are introduced, together with previous studies

and main findings. Afterwards, the description is centered in describing the main processes related to a low-level jet and finally the eddies and turbulence scales are mentioned to introduce the anisotropy ratio. At the end the main scientific questions are stated.

In general, the first paragraph should be rewritten, as it seems unstructured at the moment.
We rewrote the paragraph, taking into account the comments of the three referees.

In the second paragraph, where the idea of isotropic/anisotropic turbulence is discussed, it would be beneficial to talk about past research and observed anisotropy during these conditions.
We included a paragraph about the LLJ and its influence on wind speed and wind speed variance. The next paragraph is about anisotropy in the atmosphere. Then we added a paragraph about numerical simulations and the capability to reproduce turbulence data.

The last paragraph should also be rewritten, as it does not flow well. I suggest first briefly describing the primary meteorology conditions of the day and the dataset used in this study. At the end of the paragraph, then you can compare and contrast instrumentation, meteorology, etc. between this and the Darbieu study.
As suggested, we conclude the Introduction with an overview of the atmospheric situation of the case study, and compare to the Darbieu conditions. The research questions and the scope of the manuscript are finally given. A more detailed description of this case is made in new section 3.

Background:

j) Line 69: Please provide an explanation for why this day was chosen as a case study.
We changed the text to:
"The analyses focus on a case study for 2 July 2011, for which M^2AV flight data after sunset are available."

k) Line 77: Provide a number for how slowly the second radiosonde typically descended. Also, what was the typical ascension rate? These may be important in determining how well they can resolve a developing inversion.
We added in the text:
"The mean ascent speed of the frequent radiosondes during BLLAST was 5.35 m s^{-1}, the mean descent speed was 3.55 m s^{-1}."

l) Line 93: State what quality about the M2AV has been validated against other datasets, as this statement seems vague.
We have included in the sentence that the parameters measured by the M^2AV (profiles of temperature, humidity, wind speed and wind direction, as well as turbulent fluxes of sensible heat) have been validated extensitvely against other airborne data sets (Spiess et al., 2007), as well as in situ meteorological tower and remote sensing observations (Martin et al., 2011). These intercomparisons show that M^2AV observations are similar to those observed from other sources, specially during the transitions, when temporal changes of the fields are slower than during the fully developed convective boundary layer.

m) Line 120-123: Which method was used for each variance? Why were they not computed using a similar method?
The variances were all computed with the same method. The sentence is misleading, indeed. We changed to:
"The variances of wind speed σ_v^2 and σ_w^2 were calculated by employing a high pass

Butterworth filter of third order". These variances are comparable to values computed from the 60 m tower observations (see figure below). It is seen that the increase of the anisotropy after sunset is related to a decrease of sigma-w. As suggested from rewiewer 2, these results are now described in section 5.

n) Line 127: So a high pass filter was used for the calculation of the horizontal wind variance? This sounds opposite of what is stated earlier that the variance values were simply detrended.
A high pass filter is applied, yes. We changed the text to:
"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 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 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."

o) Lines 120-133: This section needs to be rewritten, as it is currently very unclear how these calculations were performed differently for v2 and w2.
We agree with the referee that this paragraph is confusing. We omitted the tests we performed, and just present the method that was used in the end (text see above).

p) Line 135: Explain why you assume isotropy in the horizontal direction. With the data from the 5-hole probe, it's possible to calculate u2 as well. Do these values (compared with the v2) support your statement of horizontal isotropy? Previous research shows

that this assumption is not valid (see Luhar (2010), Banta et al. (2006) among many others). This may cause a large overestimate of the TKE.
We agree that a sheared boundary layer does not have isotropic turbulence, since the eddies are elongated following the direction of the main wind, as described in Mason and Thomson (1987). However, since the eddies in the transversal direction have scales of the order of the kilometer, which is comparable to the leg size of M^2AV, we may assume that isotropy apply for the sampled scales. This information will be included in the paper.

q) Line 157, 158: Note that the maximum/minimum are local, not absolute.
We changed the text to "The LLJ consists of a local maximum in the vertical profile of horizontal wind speed in the ABL, followed by a local minimum of wind speed."

r) Line 169: Include citation to Bonner (1968) as he was one of the first to come up with criteria for a LLJ to be classified.
Thanks for the suggestion. It is now included in the manuscript.

Atmospheric Situation:
s) Line 185: How is the residual layer lower than the boundary layer height? By definition, during a well-developed convective boundary layer, the temperature inversion is at the top of the ABL. Thus, what here is referred to as the bottom of the residual layer is likely the ABL height.
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.

t) Figure 2: The title and text within Fig. 2 should be in English. It would also be useful

to put a symbol on the map marking where BLLAST took place.
We replaced this figure with a topographic map. The BLLAST sites are marked, and the flight path is included.

Results:
u) Section 4.1: This section could be substantially condensed, as the level of detail is not necessary in the context of the rest of the manuscript.
We agree with the suggestion, the section is now much shorter (see the new organization of the sections above).

v) Figure 3 (and in text): Potential temperature is typically provided in K. It would also be helpful to indicate the time of the flights in either the legend or caption, making it easier for the reader to understand the evolution.
We now provide potential temperature in K. The time the data was obtained is now indicated in the legend for each figure. See new figure 5 shown above.

w) Line 198: Move sentence ('Note that : : : may influence the temperature profiles') to after 'They were all obtained during a descent'. Also, indicate how long the descents took? Was it long enough for the boundary layer to evolve during the time, or was stationarity safely assumed?
We added in the description of the M^2AV
"The profiles were performed with an ascent rate of about $3.5\,\mathrm{m\,s^{-1}}$, the descent rate was about $8\,\mathrm{m\,s^{-1}}$."

x) Line 220 and Fig. 5: The 10 m/s wind speed at 40 m seems to be a large outlier, when compared to the rest of the profiles (and the rest of that profile itself). The authors should carefully evaluate this measurement before reporting it, to ensure that

it is a valid measurement. It looks like an outlier, and that there may have been an issue with the measurement. With such a large number of instruments taking data during BLLAST, there should be another source that corroborates this measurement.
We removed this particular wind speed profile from the figure, as the other profiles of this flight are in better agreement with the other data sets. However, we consider that we observed a process of changing conditions, as also the wind direction changes dramatically within the same profile. These changes probably occurred on a time scale below minutes, which were not captured by the other observations, and which might be caused by the horizontal translation during the ascent (probing different air masses). See also answer to Referee 2, page 6.

y) Fig 4/6/8: With so much information provided on these plots, it is difficult to see much of the data that is actually being plotted. I suggest only plotting times that are actually used in the analysis (1500-2000) and using a similar color scheme to those used in the M2AV plots, for comparing radiosonde profiles with those of similar times. For example, color the 15:01 radiosonde blue, 19:03 magenta, etc. For those that don't have similar times, use separate colors / line types in the two plots.
We now provide profiles of wind speed, wind direction and potential temperature for each flight separately, including in the same figures also simultaneous data of radiosonde, UHF, tower and numerical simulations (see new figure 5 above).

z) Fig. 8: I suggest changing the x-axis to be similar to that in Fig. 7, to make the plot easier to see. Also, x-label should be 'wind direction' not wind speed!
We now plot all data (tower, radiosonde, UHF, M^2AV and numerical simulations) together in one figure for each time of a flight. This makes the data easier to compare.

aa) Fig. 10: Use a line to mark sunset instead of a dot, as a line would be much easier to see.

For all the time series included in the new version of the manuscript a vertical black line indicates the sunset time. See for instance new figures 3 and 7 below.

bb) Section 4.3: Based on the discussion and results in Fig. 11-13, it appears as though the LLJ did not really develop until after 21 UTC. Additionally, the altitude of any developing LLJ appears to be much higher than the flight levels, especially during the 20:30 UTC flight where it is claimed that the flight is affected by a LLJ. As mentioned earlier, the high wind speeds recorded by the M2AV at the low height seem suspicious, and it appears that the claim that the flight was affected by a LLJ is mostly supported by that measurement. In fact, at site 1 (which is closer to the flight track), no LLJ was really observed until after 00 UTC, well after the last flight. With the data presented, I question whether a LLJ was apparently during the flight periods and affected the results.
A further inspection of the UHF observations and the model results for Lannemezan show that the initiation of the LLJ took place after 2000 UTC and the last M^2AV flight could sample under the influence of the LLJ. We agree that the vertical profile of wind speed at 20:30 seems not realistic (comparing to other sources of observations and model) and for this reason we have not considered it in the analysis.

Discussion:
cc) The authors correctly identify that the TKE measurements are likely not representative and statistically insignificant. If the TKE measurements are statistically insignificant, than other parameters such as the anisotropy likely are as well, since they are computed from the same variables. The authors should further discuss these limitations as well. These issues are significant, and cast doubt on the conclusions drawn from this study.
We do not consider the TKE measurements as non-significant, we just state that with a flight length of 1 km we are not able to draw conclusions about eddies of sizes larger

than 2 km. Anyway the ABL height is below 2 km, which should limit the size of the largest eddies. Both TKE and anisotropy measurements are in good agreement with data from tower and the output of numerical simulations, which now strengthen our observations.

dd) Fig 11-13: Consistently use the same colorbar across all of these images, as it makes it much easier to compare the wind speeds across locations/times.
We replaced the figures with new ones comparing model results and simulations, and used the same colorbars. See new figure 4 showed above.

Conclusions:
ee) The conclusions section is very short and not very informative about the main results of the study. It reads as if it was written in a very rushed manner. I suggest rewriting the section to include the specific results, and relate the results to previous research to highlight any new findings. As it stands now, it reads as if no new results are found, as all of the findings are within previous research to some extent.
The conclusion section is re-written to incorporate the new analyses (additional sources of data and model 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.
(3) the anisotropy ratio A 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.

3 Answers to ediorial corrections

a) Line 50: TKE instead of 'turbulent kinetic energy'. b) Line 60: BLLAST already defined earlier in manuscript. Just put 'BLLAST' here. c) Line 70: Change 'to determine' to 'determination of'. d) Line 73: Use 'launched' or 'taken off' instead of 'started'. e) Line 109: Temperature misspelled. f) Line 325/330: Unitalicize m s-1.
We considered all editorial corrections.
Additional references to consider:
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We thank the referee for the suggestions, and included all references in the manuscript.

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Figure 5. Vertical profiles of wind direction (in °) on the left, wind speed (in m s^{-1}) in the center and potential temperature (in K) on the right, from M^{2}AV (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. M^{2}AV 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). Legend indicates the corresponding times to each data source.

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sigma-v and sigma-w separately computed from M^2AV and the 60 m tower observations.

figure_3.pdf

Figure 3. Modelled and observed time series for (a) wind speed (in $m\,s^{-1}$), (b) wind direction (in °), (c) temperature (in °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. 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.

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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 time of sunset is represented with a black vertical line. Note the logarithmic scale on the y-axis.