

## ***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.***

**Anonymous Referee #3**

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Within the manuscript, the authors present a case study of an afternoon transition with an unmanned aerial system (UAS) from data collected during the Boundary-Layer Late Afternoon and Sunset Turbulent (BLLAST) field campaign. Supplementary information was also used from a wind profiler for analysis. Profiles of temperature and horizontal wind were provided by the UAS, as well as turbulent kinetic energy and anisotropy at several flight levels. With these data, the authors show that stability increased during the afternoon transition, and anisotropy increased with the development of a LLJ.

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 im-

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portant 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. 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.

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. 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. It might be helpful to include analysis of the spectra, especially since the authors mention that wave-like features were observed. The conclusions section needs to be expanded upon to focus on the main results of the study. 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. 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’).

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:

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- a) Line 4: Define BLLAST within the abstract.
- b) Line 5: Either provide flight times in UTC, or sunset in local time (and throughout the entire paper, stick to one convention).
- c) Line 10: Low-level jet is typically not capitalized
- d) In general: Provide the main results/conclusions of the study within the abstract.

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.
- f) Line 39 (and elsewhere): It is best to use 'larger' rather than 'stronger'. 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.
- g) Line 50: Change terminology from 'turbulently mixed' to 'convective', as turbulent mixing still continues (albeit weaker) during stably stratified conditions.
- 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.
- 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). In general, the first paragraph should be rewritten, as it seems unstructured at the moment. 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. The last paragraph should also be rewritten, as it does not flow well. I suggest first briefly describing the primary meteorology conditions

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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.

Background:

- j) Line 69: Please provide an explanation for why this day was chosen as a case study.
- 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.
- l) Line 93: State what quality about the M2AV has been validated against other datasets, as this statement seems vague.
- m) Line 120-123: Which method was used for each variance? Why were they not computed using a similar method?
- 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.
- o) Lines 120-133: This section needs to be rewritten, as it is currently very unclear how these calculations were performed differently for  $\sigma_v^2$  and  $\sigma_w^2$ .
- 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  $\sigma_u^2$  as well. Do these values (compared with the  $\sigma_v^2$ ) 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.
- q) Line 157, 158: Note that the maximum/minimum are local, not absolute.
- 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.

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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.

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.

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.

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.

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?

x) Line 220 & 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.

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

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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.

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!

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

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.

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.

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.

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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.

Editorial 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  $m\ s^{-1}$ .

Additional references to consider:

Acevedo, O., and D. R. Fitzjarrald. The early evening surface-layer transition: Temporal and spatial variability. *J. Atmos. Sci.*, 58, 2650-2667.

Banta, R. M. Y. L. Pichugina, and W. A. Brewer. Turbulent velocity-variance profiles in the stable boundary layer generated by a nocturnal low-level jet. *J. Atmos. Sci.*, 63, 2700-2719.

Bonner, W. D. Climatology of the low level jet. *Mon. Wea. Rev.*, 96, 833-850.

Luhar, A. K. Estimating variances of horizontal wind fluctuations in stable conditions. *Boundary-Layer Meteorol*, 135, 301-311.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, doi:10.5194/acp-2015-1060, 2016.