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Atmospheric Chemistry and Physics Discussions

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 #2

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Review of "Observing local turbulence and anisotropy during the afternoon transition with an unmanned aerial system – a case study" by Lampert et al. 2016

The authors use data from an unmanned aerial vehicle (UAV), two ultra-high frequency wind profilers (UHF) and frequent radiosondes to investigate the afternoon transition (AT) of the atmospheric boundary layer. Profiles of temperature and horizontal wind speed obtained from UAV and frequent radiosondes were used to characterize the atmospheric conditions. Turbulence quantities, such as turbulent kinetic energy per unit mass (TKE) and anisotropy (ratio between variance of horizontal wind speed and variance of vertical wind speed) were calculated from UAV measurements. With decaying convection during AT the anisotropy increased.

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Discussion paper

General comments: 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 fulfill my expectations. I am missing a clearly formulated research question and motivation for this study. The quality of the analysis is fairly poor. The authors describe profiles of temperature and horizontal wind in an unnecessary longish way (separately for the various instruments without a critical discussion/explanation of the existing differences). 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. 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. The quality of the figures is not sufficient and English should be checked by a native speaker. 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.

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.

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. 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? Minor comments: Lines 20 to 23: The surface sensible heat flux normally decreases shortly after noon. I.e., it is more than 160 min until sunset. Line 25: why does the mixing ratio increase at this time? Lines 31-35: Why do you mention turbulent flux profiles in Oct and Febr at all? Additionally, BLLAST was in summer. 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). Also the impact of the different filtering methods could be illustrated using figures. What are these slowly changing structures? Gravity waves require stable stratification? Is this given? 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. As mentioned above the section about low-level jets is out of place here and should be moved to the introduction. Minor comments: Refer to the launching time: is this valid for the descents, too? Line 112: what kind of spline function? Line 115: how good do the flight track and the mean wind direction agree? Line 121: you mention the method: 'removing the mean value' but you did not discuss the results compared to the other filter methods. Line 130: Do you really eliminate advective contributions? Or large scale contributions? A spectral analysis would be helpful which could be compared with the spectrum of the UHF profiler. Line 158: Do you need a minimum or just a reduction of the wind speed?

There are a lot of typos: attitude instead of altitude, temperatur instead of temperature, as instead of at (line 77)

4. Analysis of the measurements Sections 3 and 4 should be reorganized. 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 analysis. In my opinion, a brief narrative description of synoptic conditions is sufficient. - As AT is defined via the surface sensible heat flux, the authors could start with showing a time 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. - 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 profiles separately for each instrument, the different instruments (UAV, UHF and frequent radiosondes) 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. 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 radiosondes. 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. 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. - When showing profiles of potential temperature the evolution of the boundary layer height zi could be illustrated. (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 layer is indicated by the top of the surface inversion) - The evolution of a low-level jet could be shown in this paragraph as well where the mean conditions 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.

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. - The figure of TKE profiles should be improved as it is hard to see the different dots. It might also be interesting for the reader to see a time series of instantaneous wind measurements 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. Currently, the authors calculate the Richardson number from the race track patterns. Why do they not use the profiles of wind and temperature? The turbulence characteristics could also be related to the surface fluxes. 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. - 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. Minor comments: Line 199: How heterogeneous are the surface conditions? 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. Line 265: What means 'the lowest value of A'? Figure 3 and 4: Either you should use temperature or potential temperature but not both. I would prefer potential temperature (in Figure 4, too).

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. 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. Minor comments: Line 340: 'An LLJ increases the horizontal wind speed': the LLJ is the horizontal wind speed! 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? 6. Literature The reference list should be adapted more precisely to the research done in the manuscript. 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. 8. As the manuscript deals with observations sensitive to the time in relation to sunrise, I highly recommend using local time instead of UTC.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2015-1060/acp-2015-1060-RC2-supplement.pdf

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