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Interactive comment on "Proof of concept for turbulence measurements with the RPAS SUMO during the BLLAST campaign" *by* Line Båserud et al.

Anonymous Referee #1

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1 General comments

I want to thank the authors for the interesting paper. It is a great achievement to enable a RPA of less than 1 kg to carry a flow probe and thus make it possible to measure the turbulent three-dimensional wind vector. Such an instrument is of great value to the scientific community in boundary-layer meteorology.
The manuscript describes how data of a multi-hole flow probe is fused with inertial measurements of the RPA to achieve a measurement of the three-dimensional wind vector. The authors address specific problems of the SUMO system and present solutions to some identified problems in order to measure turbulent kinetic energy (TKE).

The results are compared to a sonic anemometer mounted on a 60 m tower.
Despite the good structure and presentation of their work, I have some strong concerns with regard to the methods that the author's applied to the measured data. I consider especially the high-pass filter on the measured vertical wind component as not applicable for turbulence measurement. I want to advice the author's to get to the bottom of the measurement error that leads to the oscillations in the vertical wind and correct the error where it occurs instead of applying a filter to the final wind measurement that will delete an important part of the measurement. More details are given in the specific comments. Other issues are the missing measurement of a true yaw angle, which is a major concern that needs to be addressed in more detail, as well as missing information about in-flight calibration (Lenschow maneuvers), which is mandatory for flow probe measurements.
In Sect. 5 I miss some more comparison to the multiple other instruments that were available in the BLLAST campaign and are a unique possibility to do validation on the measured data. Section 6 is a nice summary of the causes for uncertainties, but no attempt to quantify these uncertainties is done.
I suggest a major revision of the manuscript before it can be considered for final publication in AMT.

2 Specific comments

2.1 Abstract

The abstract is extremely pronouncing the problems with the measurement system, and all the things that did not work well. I strongly suggest to focus on the achievements in the abstract and briefly describing the necessary steps that were taken to in the process.
Example: "The main shortcomings were the use of two different, unsynchronized data

loggers..."
Change to: "In order to be able to measure the three-dimensional wind vector, measurements of the flow probe were synchronized with the autopilot's attitude and velocity data in post-processing."

2.2 Introduction

p. 2, l.28: "Profiles... " change to "Vertical profiles of turbulent kinetic energy (TKE) ..."
p.3, l.60: There are more recent publications of the application of turbulence probes, even at the BLLAST campaign, that could maybe be added or replaced where appropriate:

- Lampert, A., Pätzold, F., Lobitz, L., Martin, S., Lohmann, G., Canut, G., Legain, D., and Bange, J.: Observing local turbulence and anisotropy during the afternoon transition with an unmanned aerial system – a case study, Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-1060, in review, 2016.
- Wildmann, N., Rau, G., Bange, J., 2015. Observations of the early morning boundary-layer transition with small remotely-piloted aircraft. Boundary-Layer Meteorology 157 (3), 345–373.
- Reineman BD, Lenain L, Statom NM, Melville WK (2013) Development and testing of instrumentation for uav-based flux measurements within terrestrial and marine atmospheric boundary layers. Boundary-Layer Meteorology 30(7):1295–1319, DOI 10.1175/JTECH-D-12-00176.1
- Miranda Braam, Frank Beyrich, Jens Bange, Andreas Platis, Sabrina Martin, Björn Maronga, Arnold F. Moene, On the Discrepancy in Simultaneous Observations of the Structure Parameter of Temperature Using Scintillometers and

Unmanned Aircraft Boundary-Layer Meteorol, 2015, DOI 10.1007/s10546-015-0086-9

- Sabrina Martin, Frank Beyrich, Jens Bange: Observing Entrainment Processes Using a Small Unmanned Aerial Vehicle: A Feasibility Study, Boundary-Layer Meteorology. Volume 150, Issue 3, pp 449-467, DOI: 10.1007/s10546-013-9880-4

2.3 The SUMO platform

p.3, ll.81f: There needs to be some additional information about the control strategy of SUMO: Is the aircraft controlled for constant ground- or airspeed? What is the cruising velocity and how accurate is it maintained during a straight and level flight. This information is relevant to the flow-probe measurement. At which velocity was the probe calibrated?

2.4 Data Processing

p.7, l.156: How was the up-sampling done? Linear interpolation? More information needs to be given.
p.8, l.180f: It might be worth to look into the simplified Lenschow equations (see e.g. Lenschow, 1970). Using these simplifications, w is only dependent on the angle of attack α, the pitch angle θ, vertical velocity $v_{\mathrm{g,z}}$ and true airspeed U_a ($w = U_a \sin(\theta - \alpha) - v_{\mathrm{g,z}}$). Possible errors from wrong yaw/heading will be omitted, and it might be easier to get to the bottom of the actual error in the vertical wind measurement.
p.8 ,l.187f: The measurement of the yaw angle of the aircraft is crucial, especially for cross-wind measurement, but also for the other wind components, as can be seen from Eq. 1-3. I strongly encourage the authors to do a sensitivity study similar to what was

done in van den Kroonenberg (2008) and estimate the error in wind measurement with regard to the error in yaw.

p.8, l.189ff: This statement, and Fig. 6 are evidence that there must be a significant error in the calculation of w, or a drastic measurement error in the angle of attack, pitch angle or true airspeed. The root for this error needs to be found in order to understand and work with the measured data. Naturally, the oscillations are based in the control of the aircraft, but if all parameters are measured with sufficient accuracy, a correct vertical wind will be measured. I want to emphasize that tuning the flight controller to eliminate oscillations in flight will not eliminate the measurement error. Also, one of Lenschow's in-flight calibration maneuvers is the pitch maneuver, which intentionally does what is seen in Fig. 6 in order to calibrate the offset between IMU and flow probe. Has this calibration been done for SUMO?

p.9, l.200ff: The treatment of the measured data with a high pass filter as it is done in this paragraph is absolutely not sound and must not be done if true turbulent kinetic energy is to be measured. High pass filtering w with a cut-off frequency of 1 s at an aircraft speed of 20 m s^{-1} means to filter out all eddies larger than 20 m. In a convective boundary layer, eddies can be two orders of magnitude larger and in any case it is especially the large eddies that contribute to the variance and thus the TKE.

p.10, l.217ff: Integral length scales should be calculated from the sonic in order to evaluate if 10 minutes are long enough. Autocorrelation functions of the SUMO-measurements can also be calculated to see if a proper integral length scale can be calculate and the 1 km was long enough to cover the whole turbulent regime. I can imagine that in the morning and late afternoon this should be fine, but it would be interesting to assess this also for the highly turbulent regimes.

p.10, l.228f: You state that sonic and SUMO spectra show differences, but Fig.7 does not show the sonic spectrum.

p.10, l.231ff: A compensation of two independent measurement errors does not yield a correct measurement. Actually, this information disqualifies the data as it is for any quantitative analysis.

2.5 Results for the evolution of TKE

p.12, l.251 and Figure 10: I trust that the evolution of TKE is qualitatively well captured with the SUMO measurements. However, there need to be error bars with the estimated uncertainties in the graph. Also, it would be very good to include the tower sonic measurements at the lowest levels. If the graph becomes overwhelming, the number of shown profiles can maybe be reduced.

p.12, l.255: It would be worth mentioning here how the BL height was determined.

p.12, l.261ff: Are there maybe measurements at the tower for shear stress/ heat flux/ Richardson number that can be given to support the statement?

p.12, l.272: The largest spread between individual legs means that the statistical errors (random and systematic, according to Lenschow and Stankov, 1986) are largest, because of unsifficient sampling of the largest eddies.

2.6 Uncertainty analysis

p.13, l.284f: "...can cause some uncertainty" How large is the expected uncertainty?

p.13, l.286f: "... can change the spectral behaviour ..." How so?

p.13, l.291: " might cause an error ..." This error should be quantified by a sensitivity study, possibly similar to van den Kroonenberg (2008) as mentioned above.

p.13, l.295f: " errors resulting from an inaccurate yaw angle are leveled out." This is only true for a constant offset between GPS track and yaw angle. If there are variations in the yaw angle that are not measured in the GPS course (due to variations in the wind direction, which is to be measured), all wind components and thus also TKE is concerned.

p.14, l.318f: " ... being more affected by surface heterogeneity." Which makes the RPA measurements in heterogeneous terrain so valuable, because they capture a more realistic average of turbulent transport in the area!

p.15, l. 325ff: I have already pointed out my concerns above.

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