

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

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Received and published: 7 July 2016

First of all we would like to thank both reviewers for their constructive feedback that helped to improve the manuscript considerably. In the following the reviewers comments have been listed together with our response and actions taken.

Reviewer 1: General comments:

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

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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:

We have identified the oscillations in the vertical wind to be the result of an internal time shift between the IMU and the GPS. Internal lags between sensors (in the order of 1s) have also been found by Dr  e and Heinemann (2013).

Insufficient cancellation between the contributing terms seems to be the reason for the oscillations in w . Looking at the magnitude and phase of our contributing terms for the simplified Lenschow equation for w , we see that our pitch angle has a large part in driving the phase of the first term of the equation and the second term is the GPS climb speed.

A range of time shifts was applied to each flight segment, and the value giving the minimum difference between the IMU pitch and the GPS climb angle was also giving the minimum in σw . Our range of time shifts was over several of the periodic oscillations in both directions, and the time-shift we found was the absolute minimum.

Shifting the IMU pitch forward in time (1-1.5s) yields a maximum correlation between the IMU pitch angle and the GPS climb angle, and a minimum σw for all flight legs. With these two terms in phase, the oscillations in the first and second term of the w -equation cancel out. We have now corrected for this time-shift, instead of applying a filter to the data.

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:

We are aware of the requirement of corresponding in-flight calibration maneuvers and

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have now also discussed this in the outlook section (p.19 I.465). In particular for the presented flights during the BLLAST campaign we do not expect a considerable effect on the yaw angle, as the wind speeds were very low (typically 3-5 m/s) and often in addition mainly as tail-/headwind for the selected flight legs so that the cross-wind component, responsible for a potential yaw error, is very small compared to the air speed of SUMO of more than 20 m/s (see also new columns in Table 2). We have also estimated the size of the errors from not having the correct yaw angle under these conditions. See also answer to general comments for Sect.6 below.

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:

Corresponding references to other measurement systems have been included in the text at several locations in the result and discussions, e.g. on p.14 I.332: Reference to TKE values in Lampert et al (2016).

Section 6 is a nice summary of the causes for uncertainties, but no attempt to quantify these uncertainties is done:

The uncertainty related to neglecting the terms involving the separation distance from the wind equations have now also been specified in this section (p17. I.391), in addition to being stated in Sect. 4 (p.8 I.195).

The integral length scales for the sonic (60m tower) have been calculated for the 10 min sample. A corresponding discussion is added to p.17 I.412.

We have estimated the maximum error in yaw based on flight track, wind direction, wind speed, and the reference to the error estimation in van den Kroonenberg et al. (2008). A discussion is added to p.16 I.373.

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.":

Changed to: "In order to be able to calculate the three-dimensional wind vector, flow probe measurements were first synchronized with the autopilot's attitude and velocity data."

In addition to this, the abstract has been rewritten to incorporate the new time shift approach to remove the wave structures in the vertical velocity component.

2.2 Introduction:

p. 2, l.28: "Profiles..." change to "Vertical profiles of turbulent kinetic energy (TKE)..."

Changed to: "Vertical profiles of Turbulent Kinetic Energy (TKE)..." (p.2 l.38)

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.

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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

The following publications have been added (p.3 l.70) Wildmann et al. (2015), Reineman et al. (2013), Braam et al. (2016) In addition, we added Lampert et al. (2016) to our discussion of TKE profiles (p.14 l.332).

2.3 The SUMO platform:

p.3, II.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?:

Additional information has been added for clarity on p.4 l.99.

p.4 l.115: The probe has been calibrated for 15 and 30 m/s by the manufacturer; a corresponding sentence has been added.

2.4 Data Processing:

p.7, l.156: How was the up-sampling done? Linear interpolation? More information

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needs to be given:

The up-sampling was done by linear interpolation. This information have been added to the sentence (p.7 l.179): "Furthermore, the IMU and GPS data, which were recorded at a lower rate, were up-sampled to the 100 Hz rate of the 5HP by linear interpolation."

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 $vg;z$ and true airspeed U_a ($w = U_a \sin(\theta - \alpha) - vg;z$). 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:

See answer to first comment.

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:

See previous answers.

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?:

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As stated above we identified a time shift between the data reported from the IMU and GPS sensors as cause of the incomplete motion correction and have therefore considerably changed the manuscript in this context.

With respect to the pitching maneuver we can use our legs that are showing the pitching/altitude variation, as basic pitching maneuver (but in the boundary layer, not in the lower turbulent free atmosphere). We have used this to quantify the time shift between the IMU and the GPS.

Offsets between the IMU and 5HP have not been calibrated directly, but when we first bring the two data loggers (probe/autopilot (IMU/GPS)) together by the max correlation between 5HP airspeed and the GPS ground speed, and then later applying the time shift to the IMU, we are in a way indirectly calibrating the probe in timing to the IMU. In addition, we have subtracted the mean angle of attack and sideslip for all legs as a way of correcting for misalignment errors for the probe.

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 ms⁻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:

No longer relevant.

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 calculated 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:

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Information concerning the integral length scale is added for the tower see p.17 I.412 and updated Table 2.

AMTD

For the SUMO we have added a discussion (p.17 I.415): "The SUMO legs (around 1 km length) are likely to be too short (and too few) to capture the largest turbulent scales. This is evident from the spread between individual legs, especially during the highly turbulent regimes (Figs. 8, 10 and 11). Figure 9 also indicates that SUMO has trouble capturing the turbulent production scales."

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

This figure is now out. The new Fig. 9 presents examples of SUMO spectra together with sonic spectra for all legs in one flight.

p.10, I.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:

No longer relevant.

2.5 Results for the evolution of TKE:

p.12, I.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:

The figure has been changed. The data is now processed with the time shift approach. Error bars have been added and the caption has been adapted accordingly.

With respect to the tower measurements should they, in our opinion, only be used for a comparison of the SUMO flights at site 1. Site 2 is several kilometers away and we

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are not confident that the tower measurements would be representative.

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

The sentence now reads (p.13 l.317): "It was growing fast in the morning and reaching a maximum of around 1200 m (observed with various measurement platforms like UHF wind profilers, radiosondes and RPAS) during less than one hour (around 14 UTC) before decaying even faster in the afternoon (Lothon et al., 2014)."

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?:

Again we are not sure that we use the mast data from site 1 for a comparison with the SUMO flights performed at site 2. However, we have added a sentence on the wind speed conditions from the surface flux stations at site 2 (p.14 l.329).

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 unsufficient sampling of the largest eddies:

The sentence now reads: "During this period we also see the largest spread between the individual legs, again indicating insufficient sampling of the largest eddies (Lenschow and Stankow, 1986)." (p.14 l.346)

A similar statement has also been added to the discussion of the TKE profiles from 27.06 (p.14 l.336). "The largest variation between individual legs is found for the flight at 12:30, 13:32 and 14:42 UTC. Especially at 13:32 UTC in 150magl, our straight legs are too short and too few to sufficiently sample the largest eddies (Lenschow and Stankow, 1986)."

2.6 Uncertainty analysis:

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

Added information for clarity (p.15 l.361): "...leaving us with a potential maximum un-

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certainty of 0.25s."

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

No longer relevant.

New discussion on spectra can be found on p.13 l.284.

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:

We have now estimated this error. See answers to general comments.

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:

The sentence has been taken out, and a new discussion is added (p.16 l.373). The effect on TKE will originate from uncertainties in the u and v component, as w should not be sensitive to yaw errors.

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!:

This is now stated in the sentence (p. 17 l.404): "...being more affected by surface heterogeneity (which makes the measurements 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:

No longer relevant.

Reviewer 2: I think the paper has merit for publication, but the authors should first

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address the concerns outlined in this review. Particularly related to the description of the vertical velocity treatment. In addition, the authors nicely describe various aspects of uncertainty. I would improve the article if these could be quantified more:

The vertical velocity treatment has been completely reworked. See details in the answers to reviewer 1. We also tried to quantify the errors where possible/applicable.

Specific comments:

Please carefully review the manuscript for grammar mistakes:

We tried our best to get this right and corrected for obvious typos, verb forms and commata.

Abstract, please quantify and report key error metrics in the abstract:

This has been included in the abstract.

Line 41, Consider referencing: Stevens, W. R., Squier, W., Mitchell, W., Gullett, B. K., & Pressley, C. (2013). Measurement of motion corrected wind velocity using an aerostat lofted sonic anemometer. *Atmospheric Measurement Techniques Discussions*, 6(1), 703–720.:

The reference has been added (p.2 l.53)

How do the path/leg directions compare to the wind direction (when considering Taylor's hypothesis applicability) for the various cases?:

This information is given for the 4 flight legs close to the tower in Table 2, we also added a corresponding sentence (p.13 l.298): "A comparison of the flight leg direction with the wind direction shows head and tailwind for the legs during flights # 27 and 29 and a weak side wind for flights # 30 and 31 (Table 2)."

Figure 1 - Please zoom in and label components inside the aircraft:

The figure has been changed and do now include labels for the different components

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shown.

Line 200, page 9: Please carefully explain the filtering of w. Something seems wrong. Generally, a moving average is a low pass filter, but if you subtract the original signal from the low pass filtered signal, you effectively create a high pass filter (high frequency fluctuations are left). The current document is unclear, but the data look correct. Please provide details on the filtering process. How was this filter chosen? Why? What is the impact of this type of filter on data generally in spectral space? Also, what is the impact of the filter on all scales? This component is the biggest contribution to the paper. It would be beneficial to provide more data and exploration of other treatment options:

This whole part of the data processing has been revised/changed. See also the very first comment to reviewer 1.

Figure 7, the spike at 2 Hz does not seem to be easily removed:

No, we have not done anything to remove this peak in the spectra. This is visible in all three velocity components (~ 1 Hz for u,v and ~ 2 Hz for w) and is believed to be related to the control of the aircraft. It is found for all SUMO flight legs. This peak could have a contribution to the variance. This will be investigated more in the future. Corresponding information is added to the text (p.13 l.289).

Around Line 210, page 10: Please mention the heights of the legs either in the text here or in the figure caption. I know they are in the Table 2, but it is easier for the reader:

The heights of the flight legs are now mentioned in the figure caption (Fig 3).

Figure 7, could an additional panel with the time series of w for the sonic and the spectra for the sonic be included here?:

The corresponding time series from the sonic are already presented in Fig. 4. For the spectra see Fig. 9 with spectra from sonic and sumo for all wind components (all legs from flight # 29).

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Lines 227-229, Page 10 - The text states "Looking at the spectral plot in the right panel of Fig. 7 it is clear that although the sonic and SUMO show a good agreement in the integral parameter of σ_w , a distinct difference in the underlying energy spectrum of the corresponding data sets remains." This is confusing. I don't see any sonic anemometer spectra in Fig 7 of my version of the paper. Just the raw signal and the filtered signals. Please do include the sonic spectra or u , v , and w :

The sentence is removed. See new Fig. 9 with example of spectra from sonic and sumo for all wind components. And the new discussion of spectra can be found on p.13 l.284.

Figure 10, can more discussion be given to the physical significance of the profiles?:

We have added corresponding statements in different places in the text:

(p.14 l.329): "This is supported by the increase in wind speed at the surface for the morning and evening (Lothon et al. (2014))."

(p.14 l.332): "Similar TKE values have been found by Lampert et al. (2016) for flights with the RPAS M2AV from Site 1 on July 2, with maximum values between 1.2-1.5m s^{-2} (200-300m agl) at 14:30 UTC, and minimum values below 0.3m s^{-2} 150-300m agl) at 18:30 and 20:30 UTC."

(p.14 l.336): "The largest variation between individual legs is found for the flight at 12:30, 13:32 and 14:42 UTC. Especially at 13:32 UTC at 150m agl, our straight legs are too short and too few to sufficiently sample the largest eddies (Lenschow and Stankov, 1986)."

Can more data from the 60 m tower be used to illustrate the performance of the filtering technique?:

No longer relevant.

The authors present a nice list of potential uncertainties. Can the magnitude of each

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be estimated and tabulated?:

See previous answers, the new discussion in Sect. 6 and the updated Table 2.

General/other changes applied when preparing the revised version:

p.3 l.84: changed the last part of the introduction to incorporate the time shift IMU/GPS approach when describing the structure of the paper.

Changed 60m tower to 60-m tower throughout the manuscript.

Changed the order of figures standard deviations/TKE and spectra to better fit the new discussion.

New Fig. 8 (standard deviations/TKE): Has been updated with new data after time shift for the IMU. The figure caption has been changed accordingly.

New Fig. 11 (TKE profiles 15 June): Has been updated with new data after applying the time shift for the IMU. Also changed markers/linestyle and added error bars for increased clarity and similarity to Fig. 10.

We moved the new Figs. 8 and 9. (standard deviations/TKE and spectra) to the results section for a better fit to the new approach. We therefore also changed the section heading from “Results for evolution of TKE” to “Results”.

Interactive comment on *Atmos. Meas. Tech. Discuss.*, doi:10.5194/amt-2015-407, 2016.

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