Towards vertical wind and turbulent flux estimation with multicopter UAS

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1 Review response

We want to thank the reviewer for their careful and valuable review. We hope that we can clarify our analyses and clear out some of the concerns with our response.

1.1 Review Comment 1

1. *Given that most of the analysis is centered around optimal fits of field data, which is inherently noisy, it would be informative to include some analysis of the uncertainty of these fits and the sensitivity of the final wind estimation to these uncertainties. This would also help to alleviate typical concerns about optimal fits not being based on physical principals.*

   We understand that the noisy dataset for the vertical wind estimation is a critical point and needs to be analysed in more detail. In order to reduce the noise a bit further, we now apply a five-second moving average on the data before calibration. This improves the visual perception of the fits, but does not change the calibration coefficients.

   In order to evaluate the uncertainty we show in Fig. 1 the exponential fit as it has been applied in the first version of the manuscript, compared to a linear fit as suggested by reviewer #1. It shows that in the range of small vertical wind speeds ($w < 1$ m s^{-1}), very small differences appear, but for higher vertical wind speeds, especially downward winds, the linear fit seems to overestimate vertical winds significantly, although only few observations are there to prove it. The difference between the two fits (dashed and solid red line in Fig. 1b) over F_z is shown in Fig. 2.

   For the derived average variances and fluxes in the presented dataset, the different fits have only a small influence, because most of the observed vertical winds are in the small wind speed range. Figure 3 shows the results of the validation (as in Fig. 8 of the manuscript) with a linear fit.

2. *The manuscript alludes to the fact that the wind-vane mode used in previous experiments was activated, but I don't see that explicitly stated anywhere.*

   Indeed, we are using a weather-vane mode. We did mention it in Wetz et al. 2021 and Wetz et al. 2022, but it is also true that we did not mention it in this manuscript. We agree that it is an important piece of information which we will include in the system description (Sect. 2.1) in the revised manuscript.

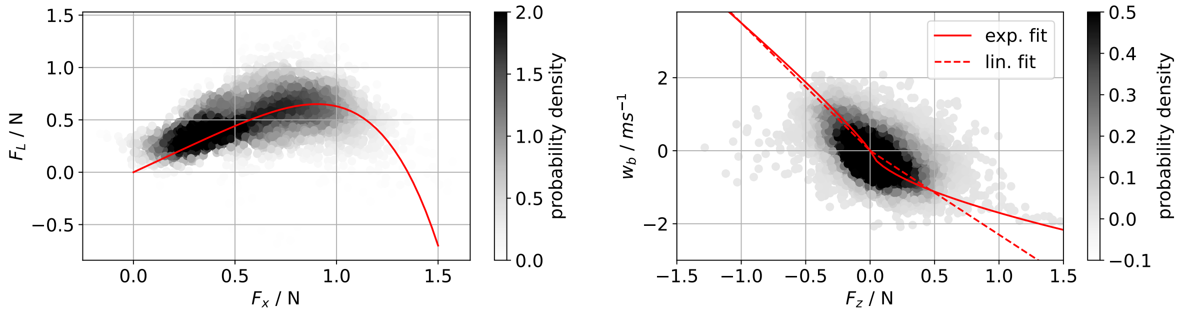

Figure 1. Fig. 5 (left) and Fig. 6 (right) with colorbars. The color coding of probability is limited to 2.0 and 0.5 respectively for best visualization of the scatter plot. All data points with values above are coded black. Different from the original manuscript, the dataset of vertical wind calibration (right) is smoothed with a 5 s windows and a linear fit is shown in addition to the exponential fit with a dashed red line.

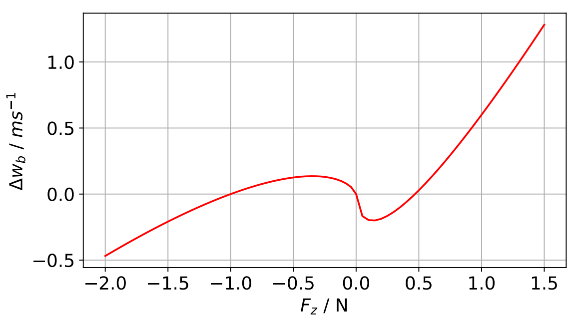

Figure 2. Difference between exponential and linear fit for vertical wind speed.

3. *For the profiling UAS in the case study, are the same coefficients determined while hovering being used to estimate wind while ascending/descending? Bell et. al. (2020) showed that different coefficients are likely needed for an ascending UAS using a more rudimentary method for wind estimation. Would you expect the same here?*

   For our system, we did not observe significant differences in hovering and ascents with the UAS. We set the ascent rate comparatively low to 1 m s^{-1}. We do not use descents, as the UAS has to fly through its own downwash and rotor-induced turbulence in that case.

4. *Was there any correction necessary to match up the UAS and sonic time series? If so, how was this done?*

   We are strictly using the system time of both systems without any further match up. Potential differences in the clocks are part of the uncertainty, but since both systems are synchronized to traceable clocks (GPS), we did not consider it necessary and could at least not observe any obvious bias in the data.

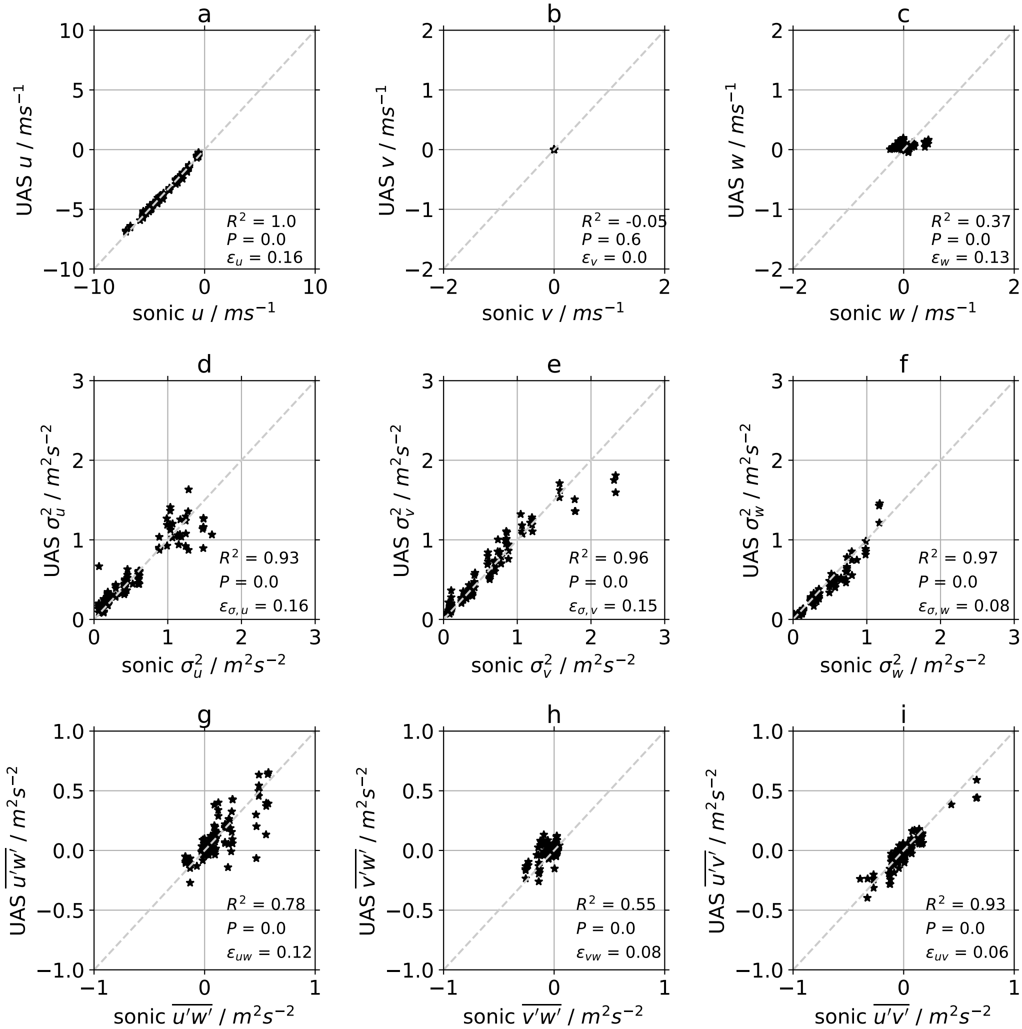

Figure 3. As Fig. 8 in the manuscript, but with linear fit of vertical wind speed.

5. *Were the winds from the sonic and UAS rotated into the mean wind independently of each other? In other words, was the sonic mean wind direction used to rotate to sonic winds, and vise versa? Or was one system used to determine the rotation for both?*

   For calibration, we rotated both systems into the yaw angle of the UAS. For validation, the systems are individually rotated into the streamwise coordinate system. From Wetz et al. 2022 we know that the average uncertainty of wind direction as measured by the UAS is below 5°. With such small angle offsets, the uncertainties in the wind components depending on which information is used for the rotation is very small. We will include the information in a revised manuscript.

6. *Though small, there does seem to be a consistent bias in wind direction in Fig 11. Would the bias be attributable to GPS errors? Was there a magnetometer calibration for the flight site?*

   As can be seen from Wetz et al. 2022, the uncertainty of UAS wind direction measurement compared to the mast is below 5°, but for individual systems it can of course occur that biases exist. The magnetometers of all UAS in the fleet were calibrated prior to the first flights of the campaign at the measurement site. As it can be seen in Fig. 11, the bias becomes largest with lowest wind speed, it is thus conceivable that the method to derive wind direction from yaw and the tangent of the streamwise and lateral wind component is prone to some error in such conditions. Throughout this morning transition, wind speeds are comparatively low.

7. *I recommend replacing 'unmanned aerial systems' with the more inclusive 'uncrewed aerial systems' or 'remotely piloted aircraft systems'.*

   In some of my previous studies I used the term 'remotely piloted aircraft systems' (RPAS), because ICAO chose the term as the standard in their nomenclature. Most recently, in 2019, the EASA released their new regulations using the term 'Unmanned Aircraft Systems' to separate it from 'manned' aircraft, which is still the official term in aviation. For that reason, I switched to UAS. I appreciate the effort for inclusive language and the ideas behind it, but on the other hand, I also want to try to reduce the confusion in terminology and comply to the terms which are used by the responsible authorities.

8. *The way the axis labels on the figures are formatted could cause some confusion at first. For example, on Fig 4 "revolutions / s-1" could be interpreted as "revolutions per s-1" instead of "revolutions with units of s-1".*

   It is actually not a confusion, but on purpose that the axis labels can be read as a math expression. The value divided by the unit only leaves the number on the axis. I know this is a matter of some dispute, but I am referring to the SI unit brochure that recommends treating units as mathematical entities and thus use the forward slash in tables and figures to separate variable names from units (https://www.bipm.org/documents/20126/41483022/SI-Brochure-9-concise-EN.pdf/2fda4656-e236-0fcb-3867-36ca74eea4e3, https://www.nist.gov/pml/special-publication-811/nist-guide-si-chapter-7-rules-and-style-conventions-expressing-values). Unless the reviewer or journal insists on a different style, I would like to keep the current style.

9. *Colorbars should be included on Figs 5 and 6.*

   We include the colorbars in the revised manuscript as shown here in Fig. 1.

10. *On Fig 7, it may be informative to also plot the differences of the time series.*

    Figure 4 shows the differences of the time series for the flight that was shown in Fig. 7 of the manuscript. With the original sampling rate, the results are very noisy and the strong gradients in vertical velocity which do not occur at the exactly same time at the location of the UAS and the sonic anemometer can cause large differences if the two time series are simply subtracted. Shown here is the time series with a 10-second moving average, but even here it shows that in periods with gradients as at 11:10 UTC, errors can become large. This can however not be interpret as an error of the measurement method, but an uncertainty of the experimental setup. For average values, which we use for validation, this uncertainty is much smaller. For this reason, we suggest to not show such a plot in the manuscript. The time series comparison tells the story in a better way in our opinion.

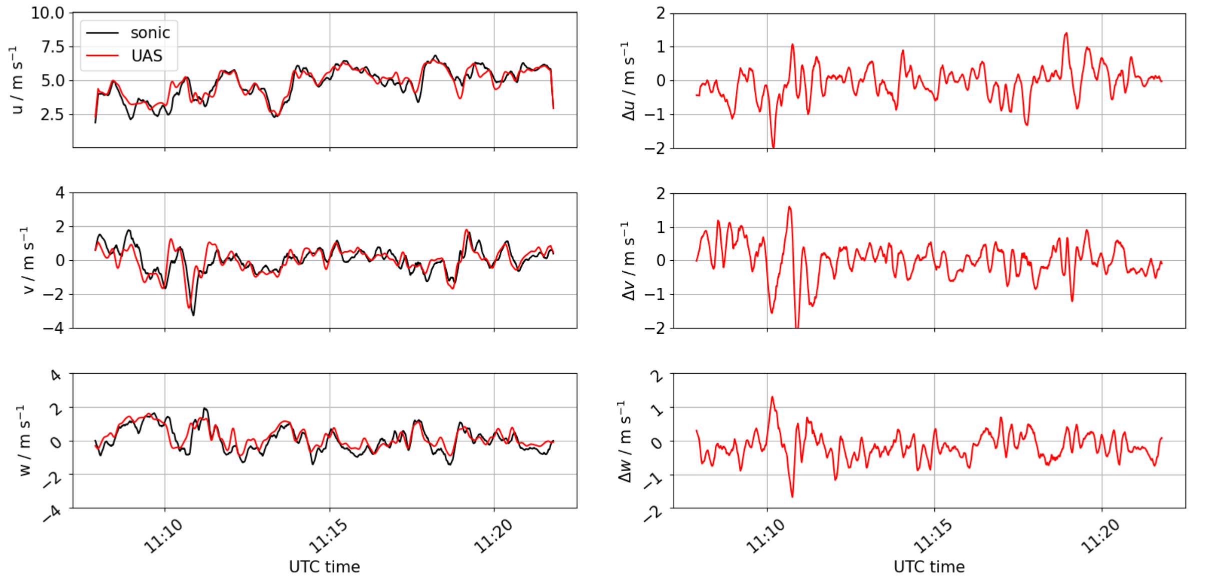

Figure 4. Flight 115, UAS#13 with delta (right).

11. *On Fig 11, the errorbars are pretty difficult to see since they line up with the grid lines. It may be worth putting the cap on the end of the error bars to make them more visible.*

We put a cap on all the errorbars for better visibility in a revised manuscript as shown in Fig. 5.

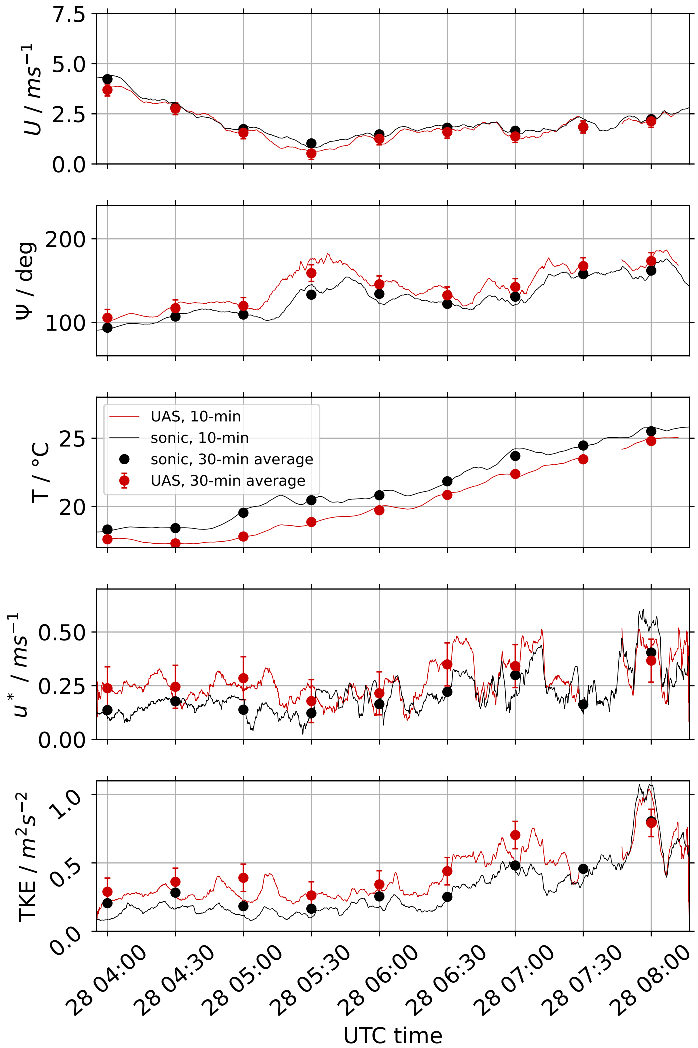

Figure 5. Time series of horizontal wind speed U, wind direction Ψ, temperature T, friction velocity u^* and TKE at 50 m above ground level during the morning transition of 28 June 2021 in comparison between UAS and sonic.

References

Wetz, T., Wildmann, N., and Beyrich, F.: Distributed wind measurements with multiple quadrotor unmanned aerial vehicles in the atmospheric boundary layer, Atmospheric Measurement Techniques, 14, 3795–3814, https://doi.org/10.5194/amt-14-3795-2021, 2021.

Wetz, T. and Wildmann, N.: Spatially distributed and simultaneous wind measurements with a fleet of small quadrotor UAS, Journal of Physics: Conference Series, 2265, 022 086, https://doi.org/10.1088/1742-6596/2265/2/022086, 2022.