High resolution wind speed measurements with quadcopter UAS: calibration and verification in a wind tunnel with active grid

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We want to thank the reviewers 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 Review Comment 1

1.1 General comments

1. *The manuscript describes the an approach to measuring wind speeds using a quadcopter UAS within a wind tunnel, emphasizing calibration and verification methods. The authors are looking to refine the calibration process for the wind measurement algorithm of their SWUF-3D UAS fleet within a controlled laboratory setting. This process is important for obtaining accurate in situ measurements of the atmosphere without waiting for favorable weather conditions for a proper calibration in the open field. Other researchers on the topic have been discussing this method to be a simple and obvious solution, but efforts must be made to connect this laboratory studies with the real scenarios in the open field. Overall I find the paper well structured and I'm happy to see this method providing a more robust wind calibration. However, the authors may need to correct some narrative to facilitate the reader to follow the study and other concerns which I discuss in more detail below. This concerns and others need to be addressed before I can recommend publication in AMT.*

   Thank you very much for the positive feedback, we are also happy to achieve the more robust wind calibration. As part of our replies to the specific comments, we made efforts to connect our laboratory study with real scenarios in the open field and to better describe this connection. The narrative was reviewed internally by a native speaking scientist, and their corrections will be implemented in a revised manuscript. We hope that this will make it easier for the reader to understand our study.

1.2 Specific comments

1. *In the abstract, it is not clear what is being calibrated. The authors mention "the algorithm for wind measurement" and "calibration coefficients" but this is not very specific. Is it for an onboard instrument? Is it for a dynamic model? Is it*

for drone's autopilot system? Consider making it clear since this is where the reader gets their first impression.

Our horizontal wind measurement is based on measuring the acceleration of the UAS using the avionics sensors to infer the wind acting on the UAS. This conversion is carried out using a wind measurement algorithm that is based on several transformation terms. These contain coefficients to be derived empirically, that we determine through wind tunnel tests, which we refer to as calibration of the wind measurement algorithm. We agree that the abstract does not contain a sufficiently detailed description and will use the following explanation in the abstract of a revised manuscript: "To date, the coefficients for the transformation terms used in our algorithm for deriving wind speeds from avionic data, have only been determined via calibration flights in the free field."

2. *Line 91-93: Please specify if you are using raw GNSS measurements for the correction. If this is the case, why not use the fused solution given by the IMU? Besides providing higher sampling rate, it should also be more precise since it is correcting the GNSS data with the accelerometers, barometers, and gyros. This also applies to the Optical Flow and rangefinder devices. Also, Optical flow works best when there are clear patterns on the floor. Have you tried painting or drawing patterns/grids on the floor to help increase the accuracy of the optical flow and decrease the drift?*

   We actually use the fused solution given by the IMU and will specify this in the revised manuscript as we expect it to be more precise especially for small movements. In field measurements, deviations of approx. $0.1\ \mathrm{m\ s^{-1}}$ between raw and fused output data can occur. Also, the fused solution has the advantage that it is available at a higher frequency than the 5 Hz raw GNSS output. For troubleshooting the optical flow drift, we tried different surfaces, regular vs. irregular, rectilinear vs. organic ground patterns and different lighting conditions. The drift was not affected, let alone reduced.

3. *Line 100: Have you considered using an indoor GPS repeater? This may allow you to reproduce similar positioning conditions as flying in the free field.*

   Yes, we considered using an indoor GPS repeater, but for practical reasons (effort of installation and cost) we decided in favor of optical flow.

4. *The way Table A1 is laid out is hard to understand especially with the empty cells.*

   We will improve this for the revised manuscript.

5. *Calibration section 3.1: I'm assuming that the drone was calibrated with the turbulent wind tunnel set to laminar flow as much as possible. Please clarify this in the experimental description.*

   Yes, this assumption is correct and we agree that this should be made clearer. In a revised manuscript, we will be more precise about this.

6. *Line 172-174: This is a statement given by the authors without much reasoning. Please show an equation or deduction where the accelerometer offsets are the only uncommon factor among the equally-build UASs for wind estimation. Also, the authors claim no wind tunnel is required. However, the way I understand this is that at least 1 drone needs wind tunnel calibration and then the rest of the fleet would get the coefficient by portability. Please clarify if this is the case.*

   The UAS in the fleet have the same mechanical and aerodynamic properties and the center of gravity lies in the same position for all UAS. The respective UAS therefore always has the same aerodynamic orientation in the wind, i.e. along the wind direction and with a corresponding pitch angle θ, in order to maintain its position against the wind. The formulas for the horizontal winds were already described by Wetz and Wildmann (2022):

$$F_x = mg\sin(\theta) + m\ddot{x}$$

$$F_y = mg\cos(\theta)\sin(\phi) + m\ddot{y}$$

The individual components and their mounting in the UAS are not perfectly identical, but the deviations can be neglected in view of the minor influence on the aerodynamic and mechanical behavior of the UAS. For example, Wildmann (2022) was also able to show that several equally built rotors show little aerodynamic difference, even with small damages on the propellers. Only the slight deviations in the orientation of the autopilots in relation to the respective UAS frame into which they are mounted have a relevant influence on the wind measurement. This is not due to any aerodynamic or flight mechanical effects caused by the autopilot; those deviations are as negligible as in the case of the other components. However, the acceleration sensors on which our wind measurement is based are installed in the autopilot. Accelerometers are calibrated in a manual procedure for each UAS. These calibrations can also lead to individual biases. Therefore, the wind measurement is very sensitive to the accelerometer offsets, which is why the offsets are the only relevant uncommon factor between the equally-built UAS for wind estimation. With regard to the extent to which wind tunnel measurements are necessary for determining the offsets and the calibration coefficients, the reviewer understood it as we meant it: if portability is given the coefficients can be applied to the measurement data of the other UAS of the fleet. A wind tunnel is necessary, or at least helpful, to determine the basic aerodynamic calibration. What we claim is that no wind tunnel is necessary for the calibration of the installation-related offsets of the individual UAS.

7. *Angles of sideslip section: If I understood well, the authors mean that the slow response of the weathervane function is not able to capture/resolve small-scale turbulence. For this reason, there are lateral perturbations not being considered in the wind estimation. Therefore, authors seek to determine these errors by manually adjusting the AoS and study the behavior in a wind tunnel. If this is correct, then why relevance only at low wind speeds? Is the intention here to just measure the error or to also correct for it?*

The slow response takes too long to capture wind direction changes on small scales, i.e. weak and sudden lateral wind components. Therefore, we use the roll axis to measure the lateral wind component, which we have also calibrated in the wind tunnel measurements (see section 3.1). Lateral perturbations are therefore considered in the wind estimation. The wind vector is calculated according to Eq. 1 in the manuscript, which is based on Wetz et al. (2021) and Wetz and Wildmann (2022) (see Sect. 2.1 of the manuscript). In the wind tunnel, we validated how well the wind algorithm performs if the UAS is not perfectly aligned with the main wind direction (section 3.2.2). The purpose is therefore to measure the error that we would have in the wind measurement during AoS. For this purpose, we have deactivated the weather vane mode in order to have a definite angle of sideslip to the main wind direction that is not corrected by the weather vane mode.

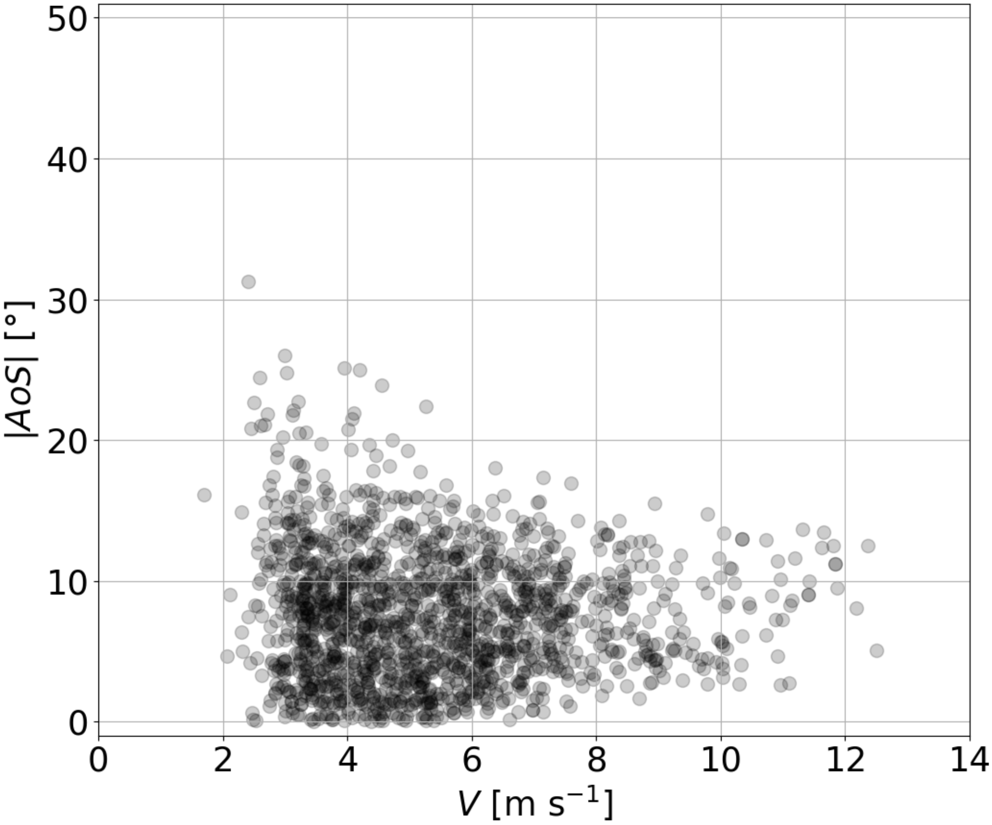

Figure 1. Absolute mean angle of sideslip $|\mathrm{AoS}|$ over mean measured wind speed V for 60-second segments of 209 drone flights with an average flight duration of over 8 min each.

At a considerable mean wind speed in the atmosphere, no rapid and large changes are expected in wind direction. The lateral wind component will be relatively small and decreasing until the UAS is facing the wind. As soon as the UAS is subject to side winds, it will correct and yaw. For these two reasons, the higher the wind speed and the greater the slip angle, the less likely is the occurrence. Therefore we stated "In this context, smaller angles and errors in the lower wind speeds are of particular relevance." By analysing the deviation between the heading of the UAS (measured by the compass) and the wind direction (measured via the wind measurement algorithm with calibration coefficients as set before the wind tunnel tests) during field tests, we were able to verify this: In Fig. 1 it can be seen that larger AoS occur at lower wind speeds, and the majority of AoS is below $10°$. The directional error is below $30°$, and below $20°$ for higher wind speeds above $4\ \mathrm{m\ s^{-1}}$, and generally tends to decrease with higher wind speeds. This is roughly the opposite behavior to the RMSE at different AoS in Fig. 4 of the manuscript.

8. *Line 215: What do the authors mean by timing accuracy? To me it looks like Eq.5 is taking the RMSE of the time response between the UAS and CTA.*

   This is correct, Eq. 5 is taking the RMSE of the difference in time response of UAS and CTA which is what we meant by timing accuracy. A more clear wording will be done for the revised manuscript.

9. *Turbulence section: Were the measurements taken with both the UAS and CTAs running at the same time or one at the time? I can imagine that the turbulent wake of the UAS will severely impact the CTA measurements, especially for the PSD. Please clarify the measurement process for the turbulence study.*

   It is correct that the wake of the UAS would strongly influence the measurements of the CTAs at flight altitude and below, not only for the turbulence measurements. As we explain in Section 2.3, we therefore choose CTA no. 2 as a reference for all measurements, as it is undisturbed by the UAS and it is valid to use a sensor for wind measurement without measuring at the exact same altitude as the UAS. We also carried out turbulence measurements without a UAS in order to compare the measurements of the reference CTA for measurements with and without a UAS in order to check whether the results match and whether the procedure therefore is reasonable (see Fig. 2). Implicit in the explanations in Section 2.3 is that we carry out the reference measurement with the CTAs and the UAS measurements at the same time. We recognize that an explicit clarification should be included, which we will include in the revised manuscript.

10. *Discussion section: Even though the authors saw some position drift using the optical flow, the optical flow should be more accurate than a GNSS system for a large margin. This alone could have been a contributor to the lower overall RMSE shown by the authors. However, by removing the GNSS uncertainties, the author's calibration coefficients and results may be more representative of the UAS's geometry, autopilot response, and propulsion system. This is a valid and usable result but I'm afraid it is not fair to compare these results with the drones set up for the open field, at least not in a straightforward manner. The authors should make an effort to discuss or investigate a way to translate this results (maybe using the optical flow and GNSS uncertainties as proxy) if the goal is to use this technique for wind measurements in the planetary boundary layer where a GNSS is most commonly used. If the position error of the Optical Flow is similar to the GNSS, then the comparison can be deemed as fair but please state it on the text.*

    We carried out measurements in the open field using the optical flow sensor for position control, but the GPS data was also logged. The drift was compensated in the same way as in the wind tunnel using trim; the UAS moved horizontally a few meters in all directions, but remained above the hover position on average. The plots below (Fig. 3, 4) show the comparison of the speeds that were logged for the optical flow and the GPS, in the north and east directions. Note that we use the raw GPS data in this case in order to obtain the GPS data without any bias caused by the optical flow measurement.

    It can be seen that the measured speeds differ to some extent. It is not possible to tell from this which measurement is the more correct one. For this purpose, the velocities are integrated up to the position (see Fig. 5).

    Both sensors overestimate the deviations of the UAS position from the hover position. However, while according to GPS the UAS moves on average within a limited range of 10^1 m, according to optical flow the UAS has moved a

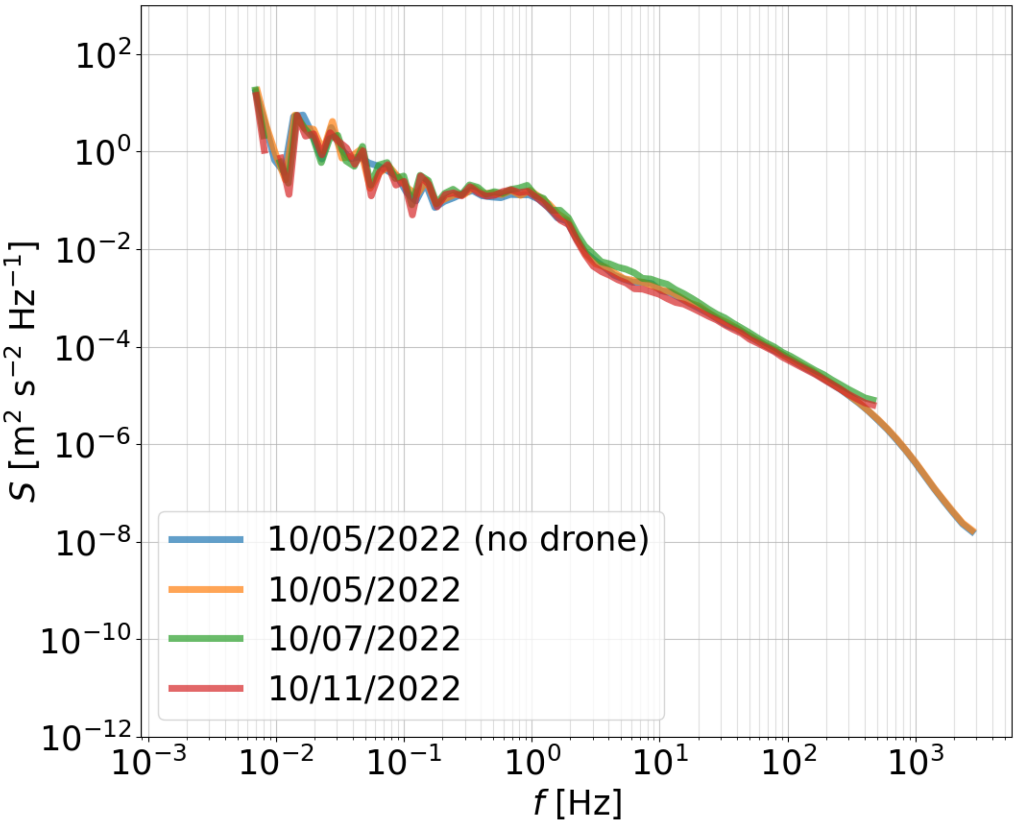

Figure 2. Comparison of PSD derived with the reference CTA during measurements with and without UAS flying upstream of the CTA cross.

distance in the dimension of 10^2 m away from the hover position. This means that the GPS measurement, which is - opposing to the optical flow data - not fused with other sensors to increase accuracy, is considerably closer to the actual movement of the UAS. The time series of the measured speeds using GPS are therefore more plausible than those of optical flow, which appears to overestimate the movements of the UAS (which shows in the drift that we mentioned as a source of error). While we do not claim from this analysis that GPS would be significantly more accurate than optical flow, it is certainly reasonable to assume that the wind measurement would not be more accurate simply by operating under optical flow. Consequently, we consider a comparison of the wind tunnel measurements with the UAS set up for outdoor measurements as fair. As long as the flight behaviour and the observed movement of the UAS are similar, the measurement accuracy will also be similar. However, we agree with the reviewer that we should address this subject in the manuscript, which we will do in a revised version.

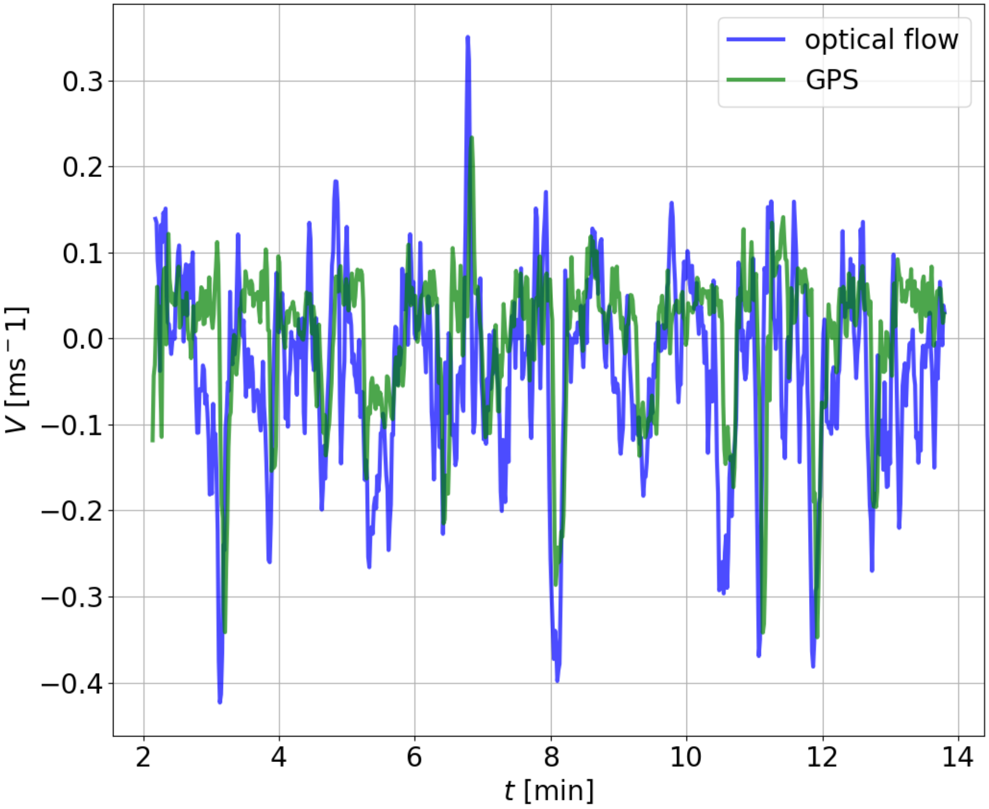

Figure 3. Time series of UAS speed in north direction measured via optical flow vs. via GPS.

1.3 Technical comments

1. *Brosy et al. 2017 – wrong DOI*

   We double checked the reference against the *How to cite* paragraph on the journal website, the DOI is correct. However, it can happen that when copying the DOI from the manuscript, the line number is also copied due to the line break within the DOI.

2. *Line 26: consider replacing "steady flight" with "vertical profile" or similar, since steady flight implies hover too.*

   We will change "steady flight" to "steady directional flight" to include both, steady vertical and horizontal flight.

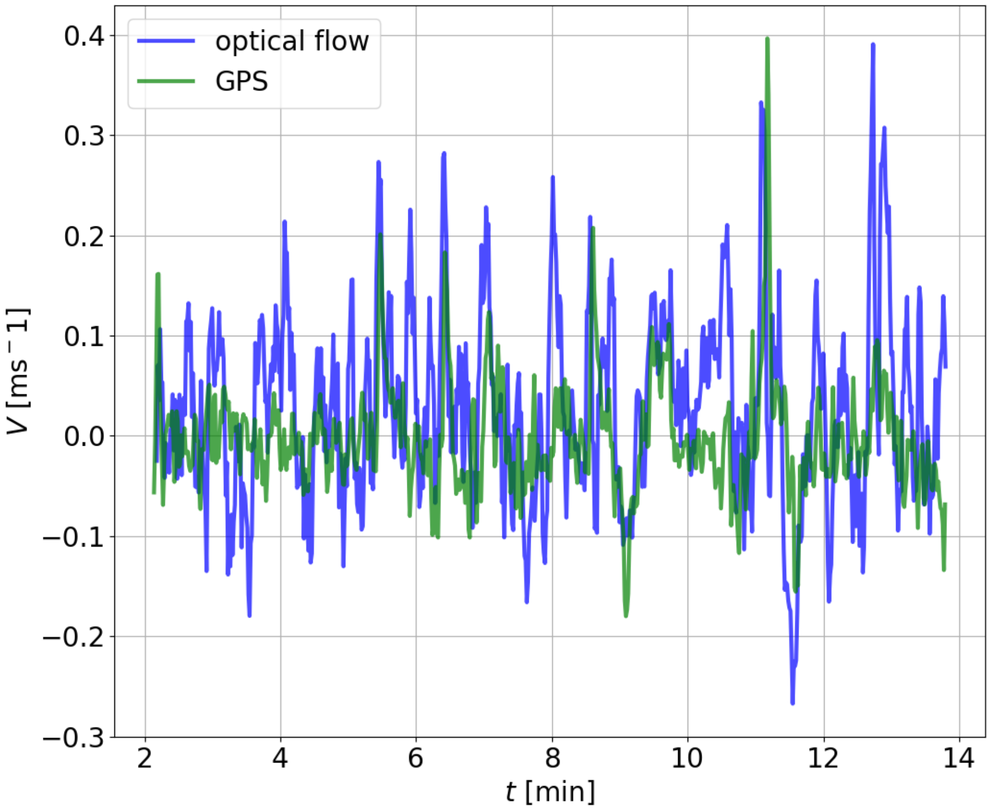

Figure 4. Time series of UAS speed in east direction measured via optical flow vs. via GPS.

3. *Line 60-61: Odd sentence, please reword it.*

   The sentence was reworded to "We highlight that this is the first time that wind measurements have been performed with a multicopter UAS in the reproducible turbulent flow fields of a wind tunnel with an active grid."

4. *Line 78: “from axis to axis” is not clear what axis. Do you mean the diagonal from rotor to rotor*

   This is what we meant, we will change the text in the manuscript accordingly.

5. *Table 1: Capitalize first letter of each first word on the list.*

   We will change the text in the revised manuscript as suggested.

6. *Line 81: Consider replacing with “Wind measurements are taken by hovering the UAS in one place . . .*

   We will change the text in the revised manuscript as suggested.

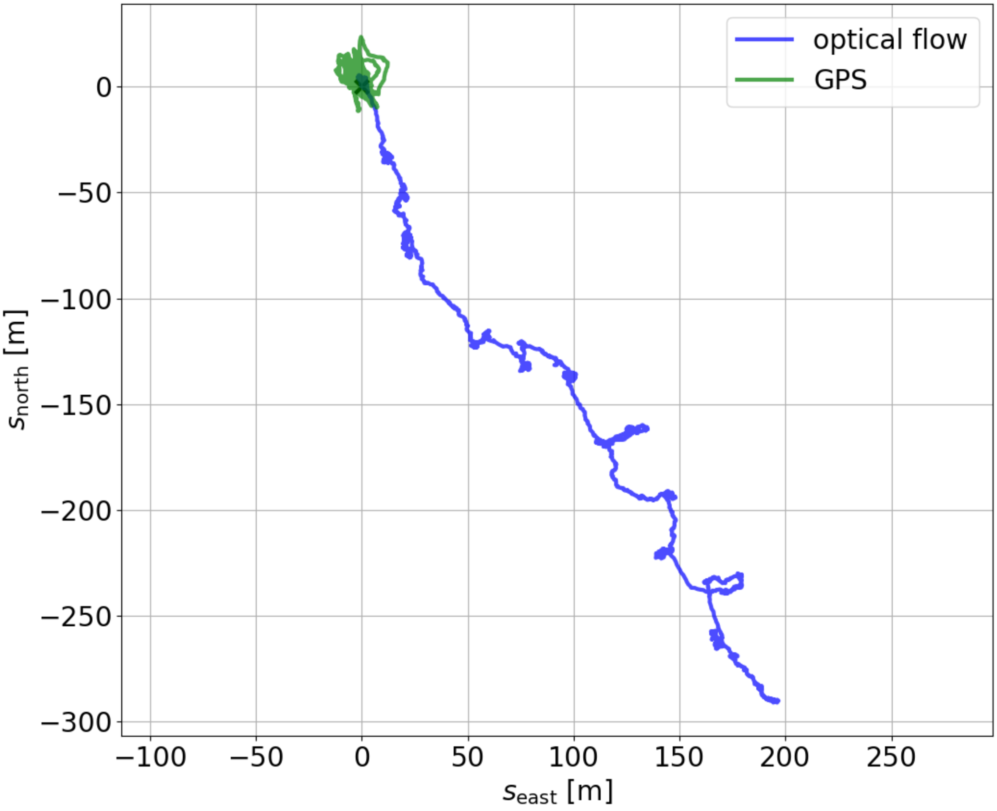

Figure 5. UAS position relative to initial hover position derived via optical flow vs. via GPS

7. *Line 84-86: Consider replacing with "The wind acting on the UAS during hover can be determined by applying the wind algorithm using the modified Rayleigh drag equation in Eq. 1 (citation) to the measured attitude and . . . "*

   We will change the text in the revised manuscript as suggested.

8. *Line 101: "discernible systematic" reads wrong. Please correct.*

   We will change "discernible systematic" to "identifiable pattern".

9. *Line 148-150: Sentence reads wrong. Consider replacing with "To compute the transfer function . . . in the modified Rayleigh drag equation (Eq.1) that best fit . . . "*

   We will change the text in the revised manuscript as suggested.

10. *Line 160: replace coordinate directions with axes.*

    We will change the text in the revised manuscript as suggested.

11. *Line 168 and 174: citations not in the right format*

    The citations will be corrected to the right format.

12. *Line 182: “30 s resp. 60 s” is confusing.*

    We will change "30 s resp. 60 s" to "30 s or 60 s".

13. *Line 190: the expression can be directly written Vp = Vo + Vg*

    We will change the text in the revised manuscript as suggested.

14. *Line 275: RMSE of “wind” speed. Remove determination*

    We will change the text in the revised manuscript as suggested.

15. *There are several grammatical and syntax errors throughout the paper. Although most of it can be understood, some readers may find it difficult to follow. I strongly recommend the authors revisit the narrative of the paper and even use grammar/spell checkers if needed.*

    We apologize for any trouble understanding our text. Grammar, spelling and the narrative were checked internally by a native speaker. The corrections will be implemented in a revised manuscript. Remaining errors are hopefully caught by the professional copy-editors of the Copernicus publisher.

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.

Wildmann, N. and Wetz, T.: Towards vertical wind and turbulent flux estimation with multicopter UAS, Atmospheric Measurement Techniques, 15, 5465—5477, https://doi.org/10.5194/amt-15-5465-2022, 2022.