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 2

1.1 General comments

1. *The paper entitled, 'High resolution wind speed measurements with quadcopter UAS: calibration and verification in a wind tunnel with active grid' presents the validation results of UAS-based wind estimates obtained by performing flight experiments in an open-section wind tunnel with an active grid. The UAS wind estimation performance was assessed by varying flow conditions and the aircratis sideslip angle. This work is important to understand the reliability of UAS in measuring wind speed and turbulence within the planetary boundary layer. However, the authors need to address the following points before I can recommend publication in AMT.*

   Many thanks for the positive feedback and the acknowledgement of our research's importance for turbulence measurement in the boundary layer. We hope that we can address the concerns satisfactorily with our answers below.

1.2 Specific comments

1. *Line 1: The manuscript states, "As a contribution to closing observational gaps in the atmospheric boundary layer (ABL), the SWUF-3D fleet of unmanned aerial systems (UAS) is utilized for in situ measurements of turbulence." Here it would be helpful to tell the reader what scales of turbulence is the SWUF-3D platform able to resolve.*

   Resolvable length scales can be found in Table 2 in Wetz et al. (2023), where fleet measurements are discussed in more detail. The largest resolvable length scales depend on the maximum flight and thus measurement duration, which in turn depends on atmospheric conditions. The smallest resolvable length scales depend on the maximum measurable frequency, which also depends on the flow conditions, which is a subject of the manuscript. A benchmark here is that scales in the order of magnitude down to approx. 5 m can still be resolved. We find it questionable whether these explanations should be added to the content in line 1, especially since the focus of the manuscript is not on this aspect of turbulence measurement. Nevertheless we agree that it would be helpful to the reader, and therefore mention this in the introduction of the revised manuscript.

2. *Line 2: The manuscript states, "To date, the algorithm for wind measurement has only been calibrated in the free field." Here the authors need to specify which specific algorithm they are referring to. Additionally, it's unclear if the authors are using the words 'turbulence measurement' and 'wind measurement' interchangeably. If not, and therefore the authors need to make the distinction between turbulence and wind velocity measurement with more clarity.*

   The algorithm we use is that of Wetz and Wildmann (2022), which inputs the measured avionics data and outputs the converted wind speeds. We will add this information 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."

   The terminology 'wind' is used as the subordinate concept of flow velocity in the atmosphere. Turbulence is a derived parameter that is derived from the variance of the flow velocity in a range of scales that can be related to turbulent motion of the flow. In the context of the submitted manuscript, the turbulence measurement is based entirely on the wind measurement by analyzing the measured wind with regard to the turbulence it contains. Accordingly, properties of the wind measurement necessarily also affect the turbulence measurement, which is why a distinction is not required here.

3. *Line 10: The manuscript states, "our analyses show that the uncertainty depends on the wind speed magnitude and increases with higher wind speeds, resulting in an overall root-mean squared error (RMSE) of less the 0.2 m s-1." However, it is not explicitly stated which type of uncertainty the authors are referring to.*

   The uncertainty is referring to the overall RMSE of all wind speeds measured in the flights with steady held wind speeds and the UAS flying in weather vane mode, which is the standard setup. We will formulate this more precisely in a revised manuscript.

4. *Line 12: The manuscript states, "The maximal RMSE occurs in the most extreme velocity steps (i.e., a lower speed of 5 m s-1 and an amplitude of 10 m s-1) and exceeds 1.3 m s-1. This result seems to contradict the result reported in Line 10.*

   In our opinion, the fact that the resulting maximum RMSE is above the resulting overall RMSE is not a contradiction, but is in the nature of a maximum. However, we recognize that the upper wind speed of the most extreme velocity steps of 15 m s^{-1} is not the highest wind speed that we have reached in our measurements. Accordingly, additional information that the uncertainty is also higher during extreme gusts, and not exclusively at higher wind speeds, is helpful. Consequently, we will note this in the manuscript. Nevertheless, we do not see a contradiction, as we do not state that higher wind speeds are the only determining factor. Also, the term of the overall RMSE might cause confusion here. As stated under the comment above, we will also be more precise about this.

5. *Figure 1: It would be helpful for the authors to denote the distance between the points a, b, and c, as well as the position of all 7 CTAs and the Prandtl probe in Figure 1. Additionally, since the calibration experiments were performed in a wind*

tunnel with an open test section, were any experiments performed to quantify the wind field differences across points b and c?

In lines 140 and 121, we describe positions b and c in relation to position a. We find this less confusing than including it in Figure 1, where angles and distances may appear distorted due to camera optics. However, we will revisit the distance information in the figure description in a revised manuscript. Furthermore, we will label the positions of the CTAs on the schematic cross in the graphic.

Measurement setups for the reference sensors, such as those we used in our experiments, are used in a comparable manner in the multitude of wind tunnel tests at the ForWind Center. Accordingly, several investigations have already been carried out on differently placed measurement points within the measurement area of the open test section. These show that the wind field differences between points b and c are small and negligible for our measurements. This is particularly the case when the grid is constantly open, as is the case with the calibration experiments referred to by the reviewer. The area below 1.5 m distance to the outlet cannot be used for the measurements as the flow is not fully developed here; position b is 2.5 m behind the outlet.Also see the supplementary material from Neuhaus et al. (2021) which shows for the example of gusts that our measurement setup is still in the usable area of the open test section. Beyond this we checked the homogeneity of the flow with an independent flow probe, in longitudinal and lateral expansion at 9 evenly distributed positions in the wind field between the wind tunnel outlet and the CTA cross. The homogeneity in the wind field was sufficient, the standard deviation of the wind velocities in the longitudinal direction is 0.09 m s^{-1}.

6. *Line 100: The positional drift should be reported in units of distance (i.e., m) instead of units of speed (i.e., m s-1).*

   The drift is not a constant offset between the target and actual position of the UAS, but a continuous process by which the distance between the setpoint and actual position increases over time. We did manually correct the actual position during the flights. Accordingly, we consider a specification that includes the time factor to be more correct. Units of speed fulfill this condition in contrast to units of distance.

7. *Line 101: The manuscript states, "As wind speed increases, the intensity and direction of the drift change without a discernible systematic, which required constant adjustment counteracting the drift during the test flights. These adjustments were executed by the remote pilot through a manual trim." It would be useful for the reader to know if the manual trim remained constant across all test cases, and if any experiments were performed to quantify how the manual trimming affected the accuracy of wind estimates.*

   Since the intensity and direction of the drift change with changing wind speed (see quoted text), and the wind speed was not constant over the full period of any measurement, it was not possible to set a constant trim for all test cases to compensate for the drift.

   No dedicated experiments were carried out to specifically quantify the effect of trim on the wind measurement, but we carried out measurements in the open field using the optical flow sensor for position control while the GPS data was also logged. The drift was compensated in the same way as in the wind tunnel using trim, i. e. position hold was improved. The UAS moved horizontally a few meters in all directions, but remained above the hover position on average.

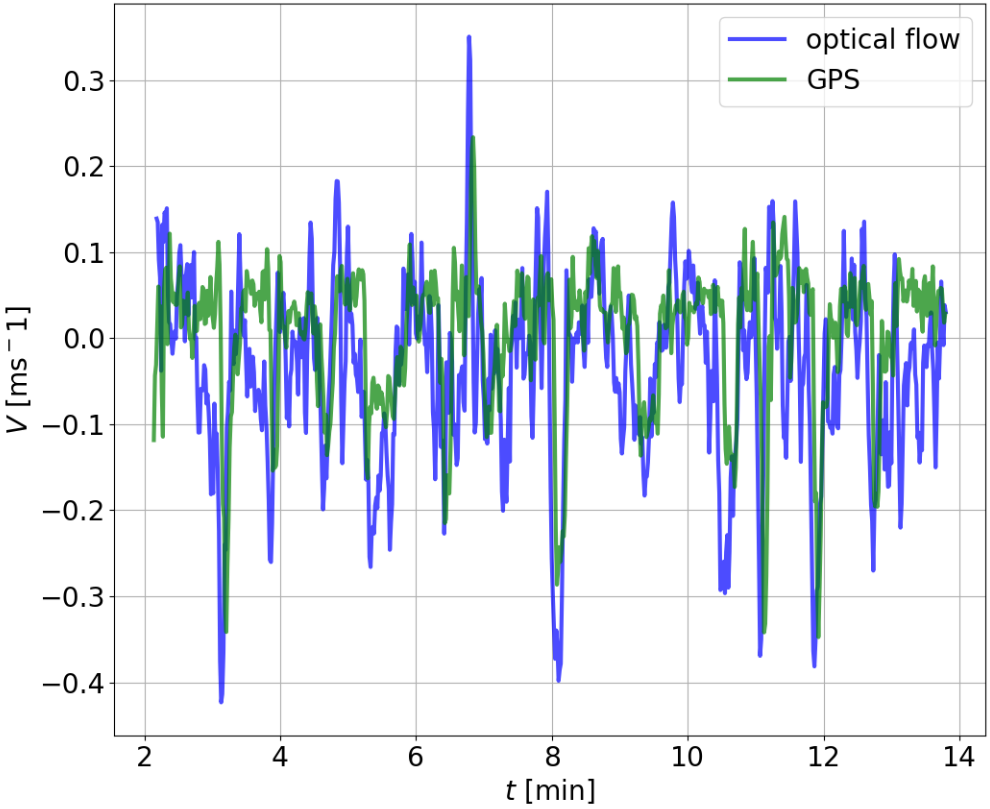

Figure 1. Time series of UAS speed in north direction measured via optical flow vs. via GPS.

The UAS speeds were logged for the optical flow and the GPS, in the north and east directions (Fig. 1, 2). These velocities are integrated up to the position (see Fig. 3). 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.

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 distance in the dimension of 10^2 m away from the hover position. This means that the GPS measurement is considerably closer to the actual movement of the UAS while the optical flow appears to overestimate the movements of the UAS. This means that measurements under optical flow are less accurate, both when flying with and without manual trim. However, trimming improves position hold, which in turn improves the wind measurement. Accordingly, when positioning via optical flow, more trimming leads to better comparability with GPS measurements. We agree that the reader should know how our

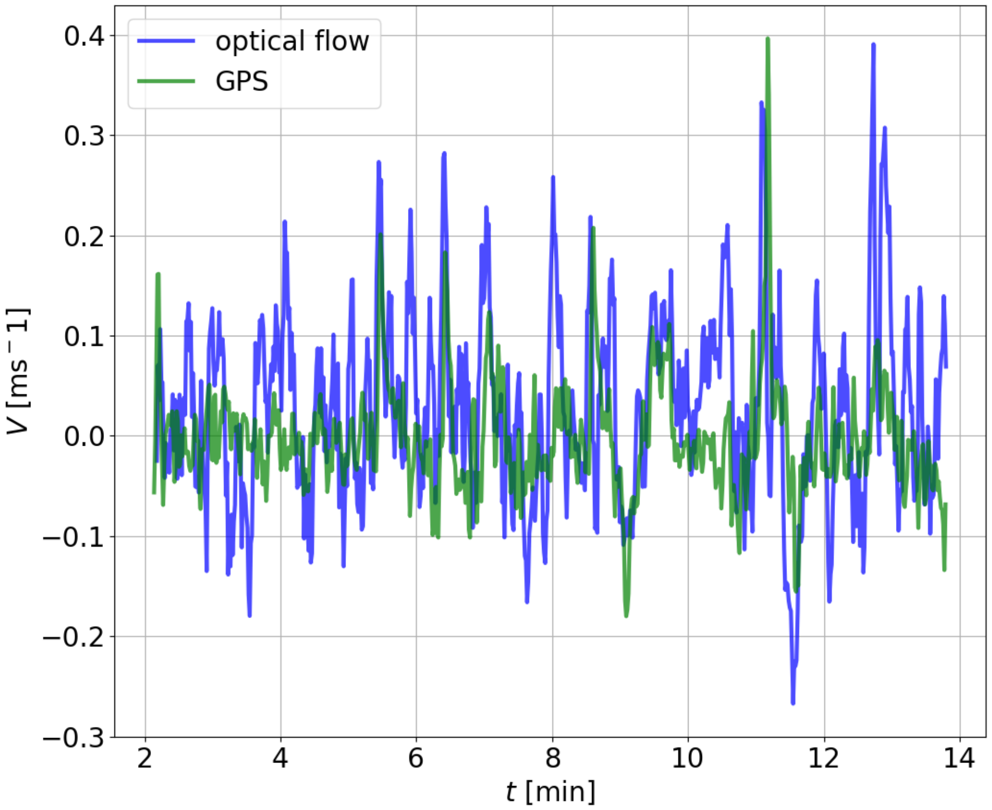

Figure 2. Time series of UAS speed in east direction measured via optical flow vs. via GPS.

method of optical flow and manual trimming affected the accuracy of wind estimates. We therefore will address this in a revised manuscript.

8. *Line 123: The manuscript states, "Test runs with no UAS show that all CTAs measure the equivalent wind speed with sufficient accuracy: the standard deviation of the measured wind speed of the individual CTAs is less than 0.05 m/s." Is there a figure showing these results? Why not use instead the absolute error or root mean squared error to compare the performance of CTAs? Additionally, did the authors perform an analysis to determine the error between the CTAs and the Prandtl probe?*

   The standard deviation is used to determine the dispersion of the data around the mean, while the RMSE is used to measure the deviation from the reference. The measurement capabilities of the individual CTAs is considered to be equally accurate, as they are all of the same type, which is why we determine the standard deviation between them.

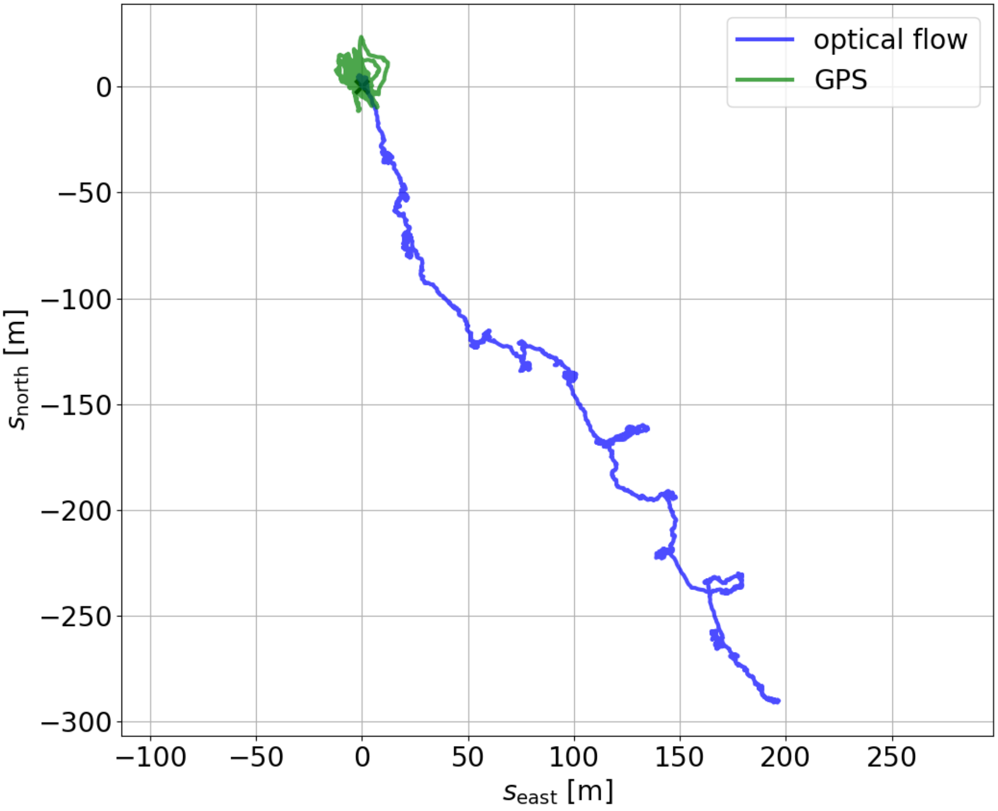

Figure 3. UAS position relative to initial hover position derived via optical flow vs. via GPS

However, with the CTA as the reference for the UAS measurement, the RMSE is more appropriate. We agree that for the mean wind, referencing the CTAs to the Prandtl probe using RMSE is an adequate method. Accordingly, we have attached a plot of the time series (Fig. 4), and an analysis of the standard deviation and RMSE (Fig. 5) which we will also include in the manuscript's appendix.

9. *Line 135: The manuscript states, "Careful quality checks were carried out for the CTA measurement data and corrupted data was sorted out." It would be useful for the reader to know the process or criteria that was used validate the quality of CTA measurement data.*

Initially, all test cases were carried out several times. In some time series, there was a clear offset between the wind speeds measured using CTAs and Prandtl for the same test cases, which meant that the measured wind speeds did not match the preset speeds. The measurements were therefore not reproducible. However, the UAS measurements were

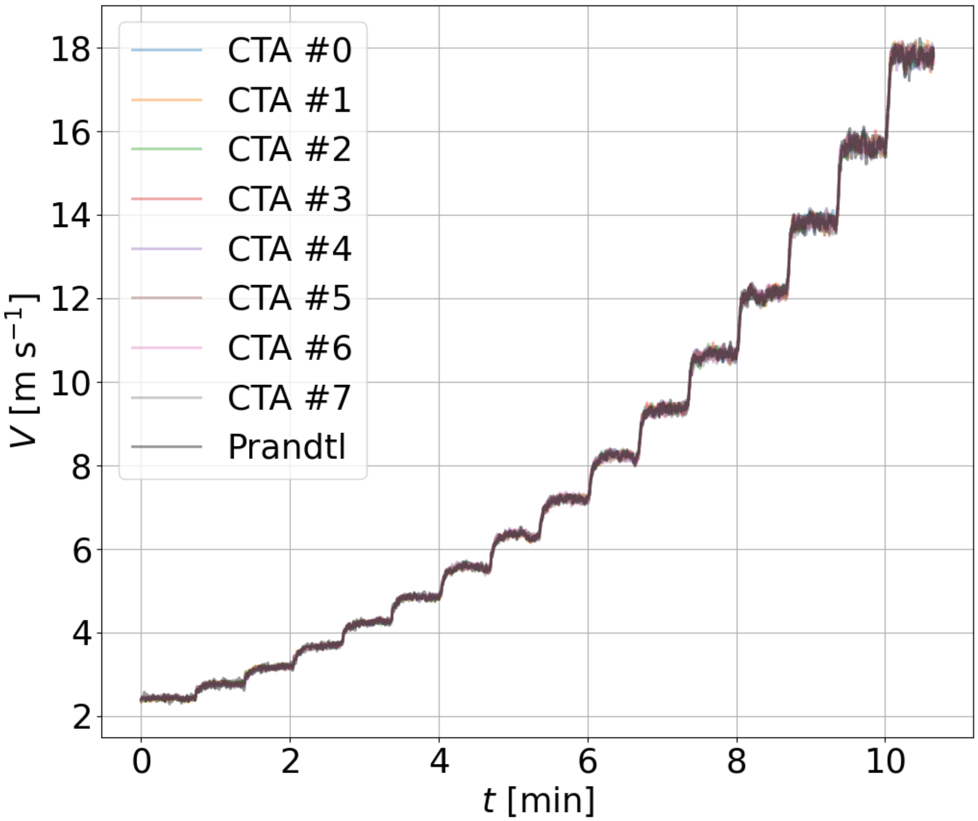

Figure 4. Time series of the logarithmically increased wind speeds measured with the individual CTAs and the Prandtl probe.

reproducible and show plausible velocities in relation to the preset wind speeds. The suspected source of error in these cases is related to the wiring and thus the grounding of the sensor box of the CTAs and Prandtl probe. In short, entire time series were disregarded when the reference sensors clearly output incorrect measurement data. Since this is a common and hardly noteworthy procedure for experimental work, we will delete the passage in a revised manuscript to avoid confusion. Only the measurements for 10/11/2022 morning were excluded.

10. *Line 148: It would be useful for the reader to know which specific optimization algorithm was used to estimate the calibration coefficients for Eq. 1.*

    We agree, this is useful information for the reader and will be added in the revised manuscript. We use the trust region reflective algorithm of the SciPy python library (https://docs.scipy.org/doc/scipy/reference/generated/scipy.optimize.curve_fit.html, last access: 27 May 2024).

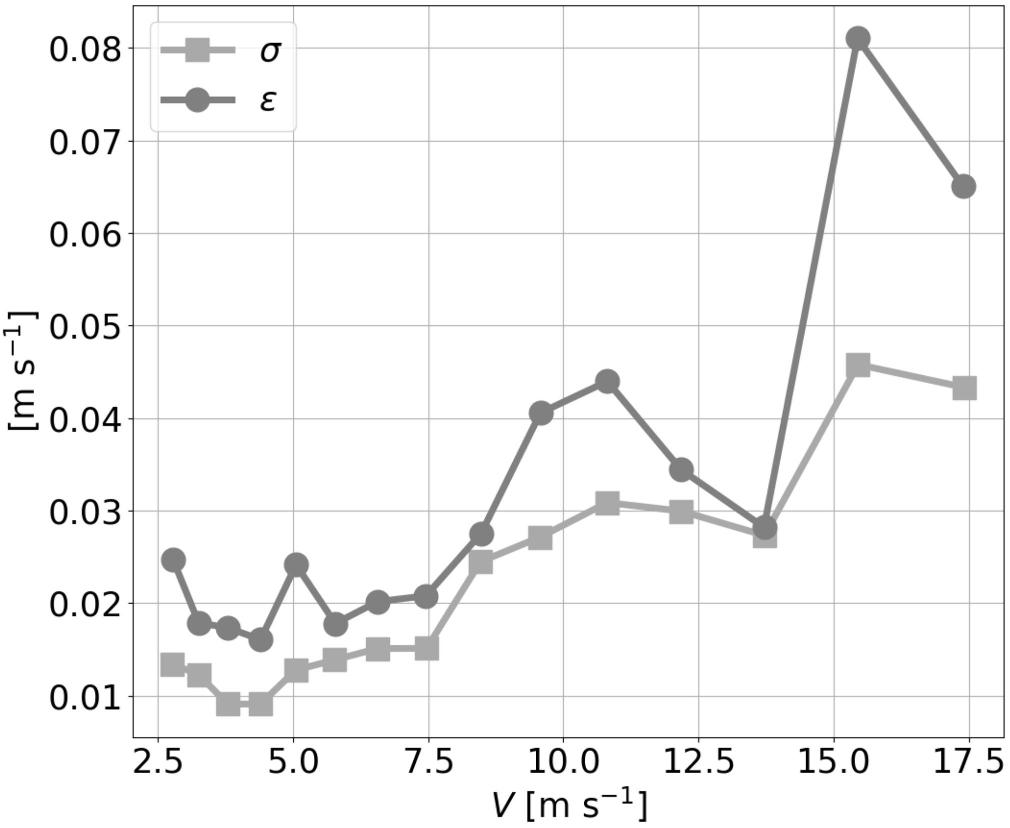

Figure 5. Standard deviation of the CTAs σ and RMSE of the CTAs to the Prandtl probe ϵ for the measurements without UAS, which were performed twice each day.

11. *Line 150: A reference is needed for ISO 17713-1:2007*

    We agree, although ISO 17713-1:2007 was listed in the bibliography, it was not linked in the body text. This will be added in a revised manuscript

12. *Table A1 is missing entries in column two Table A2 is missing entries in column one Table A4 is missing entries in column one*

    The first reviewer has already noted that "The way Table A1 is laid out is hard to understand especially with the empty cells". This also seems to apply to Tables A2 and A4. We will improve this for the revised manuscript.

References

Neuhaus, L., Berger, F., Peinke, J., Hölling M.: Exploring the capabilities of active grids, Experiments in Fluids, 62, https://doi.org/10.1007/s00348-021-03224-5, 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.

Wetz, T., Zink, J., Bange, J., and Wildmann, N.: Analyses of Spatial Correlation and Coherence in ABL Flow with a Fleet of UAS, Bound-aryLayer Meteorology, 2023. https://doi.org/10.1007/s10546-023-00791-4