Towards sensible heat flux measurements with fast-response fine-wire platinum resistance thermometers on small multicopter uncrewed aerial systems.

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1 Review response

We thank the reviewer again for a detailed review and helping to improve the manuscript. There seem to be a view misunderstandings after the last review and also some very relevant new aspects that we try to address in this response.

1.1 Review General comments

1. *Although already suggested in the first reviews, the author's work has not yet been put in the right scientific context. Turbulence and heat flux measurements from rotary wing UAS are indeed a fairly new topic, and the novel approach presented in this study is very promising. However, at least one important article (Ghirardelli, et al. 2024) is not cited, although this is, to my knowledge, the first time the EC method has been applied in combination with a rotary-wing UAS and validated against mast-mounted ECs. Although, already suggested to include in my previous review. All relevant approaches for multirotor-based flux measurements deserve to be presented, and the proposed approach should be contrasted against the existing ones. Using a small UAS certainly has some advantages over larger systems, but it is likely to also have some disadvantages.*

   We have mentioned sonic-based measurements with drones in our manuscript and put our work into that context. We apologize that we missed this one latest publication and one of the citations was not compiled correctly in the last version. We assume that the reviewer means Ghirardelli, et al. 2025, which has only be finally published in May 2025, not Ghirardelli, et al. 2024? This is certainly the most suitable reference and we include it in the revised manuscript. Nevertheless, being able to do eddy-covariance flux estimations without carrying a sonic anemometer and thus being able to deploy larger fleets of UAS at lower cost and smaller weight is the novelty and unique feature of this work and is thus highlighted. The challenges for this approach are certainly much different from the challenges in carrying a sonic anemometer either fixed to the UAS or as a sling-load. Non-arguably, a sonic anemometer is an established instrument with well-known accuracy and uncertainty for flux measurements, whereas the approach to use the UAS avionics for wind measurements needs some more basic qualification as presented in this study. We extend our explanations in the introduction to further explain the differences of the approaches.

2. *The fact that the separation distance impacts the comparability needs to be addressed more adequately. Comparing raw time series or time-averaged data from several different locations in rather non-stationary conditions is challenging. E.g. a good correlation can only be expected when the time lag between two time series is much shorter than the "non-stationarity time scale". At least it has to be discussed how this impacts the results, that even if all systems capture turbulence perfectly, the proximity of the UAVs to one of the masts is expected to yield higher agreement compared to the two masts standing roughly twice as far apart. A time lag correction based on cross-correlation or based on mean wind speed and separation in the along-wind direction would reduce this problem, although the problem of non-coherent smaller-scale turbulence would remain.*

   We agree that the comparison is challenging and subject to uncertainties that we were trying to address in our manuscript. A "non-stationarity time scale" is not a very well defined terminology, so we are not completely certain what the reviewer is expecting here. We performed instationarity tests according to Foken and Wichura (1996) and find that for the standard 30-minute period for turbulence calculation, the periods of investigation pass the test, showing stationary conditions for heat flux calculation (see Fig. 1). We also ran an alternative test configuration with 15-minute full-period averaging (similar to the drone flight time) and 2.5-minute sub-period length (in contrast to 5 minutes for the standard test). A few outliers outside the stationarity threshold of 30% can be found in that case. This is not surprising, since ever smaller averaging periods will fail to capture a proper turbulence statistic. From the default test we conclude that on typical averaging time scales of 5-30-minutes, second-order statistics are stationary for the observation periods.

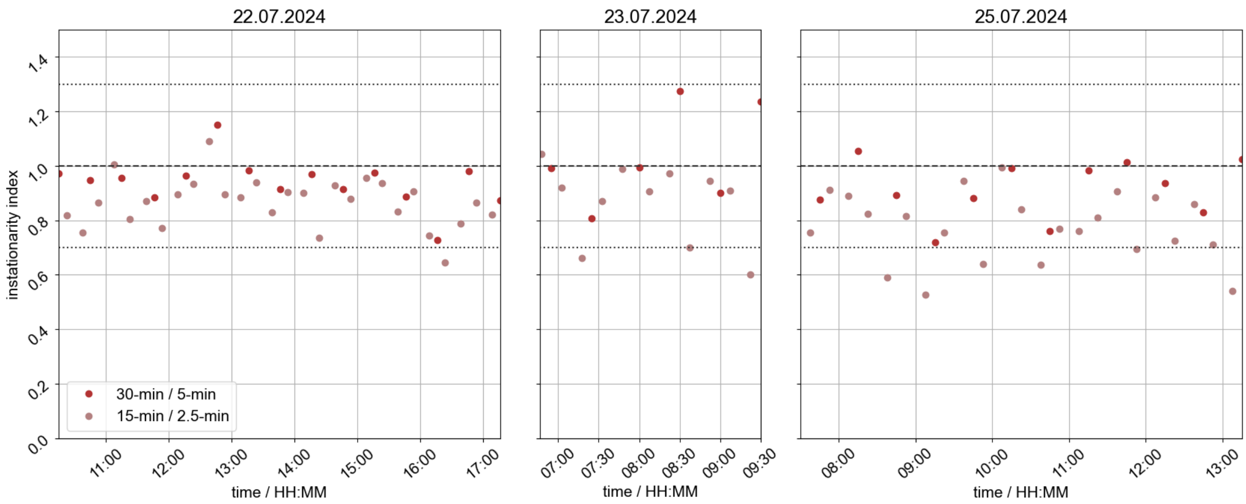

Figure 1. Results of instationarity test after Foken and Wichura (1996). Dark red shows the results of the default test with 30-minute full period averaging and 5-minute sub-period length. The light red is a variation with 15-minute full period and 2.5-minute sub-period length. The dashed line shows the line for an instationarity index of 1 and the dotted lines show the threshold for $\pm 30\%$.

As we wrote in our last review response, we did calculate the time lag based on mean wind speed and made the correction to reduce the problem of separation between mast and drone accordingly. The time lag is of the order of a few seconds (5...10 s). We also showed that the difference in variance measurements is negligible (Fig.1 of last author response). Data shown in the last version of the manuscript already incorporated this correction. Thus, the main uncertainty is from non-coherent smaller scale turbulence, which can indeed not be adequately corrected for. That is why we show the comparison of the masts to determine some experimental uncertainty, which remains. We believe that we are very transparent with that, but do not have any possibility to reduce that uncertainty. We discuss this issue more in the revised manuscript. Not only the further distance between the masts is a drawback, but also that the masts are not aligned in the same direction as the drone and the mast.

1.2 Review Specific comments

1. *I don't see why "sensible heat flux" has been changed to "heat flux". Maybe this is related to my previous comment on the difference between buoyancy vs. sensible heat flux, but the proposed method is suitable for measuring the sensible heat flux, so I suggest sticking to this.*

   Yes, we wanted to generalize this more, but agree that the main purpose of the fast temperature sensor is the sensible heat flux, so we change it back.

2. *L8-9: I still disagree with the statement that this is the first time sensible heat fluxes have been measured (accurately) using multicopter UAS. My previous comment on this has only been partially addressed by including two suggested citations (i.e., Fuertes et al. 2019; Greene et al. 2020). However, Ghirardelli, et al. (2024) is not cited, although this is to my knowledge the first time the EC method has been applied in combination with a rotary-wing UAS (in this case, measuring the buoyancy flux using a full-scale sonic anemometer). As mentioned, I agree that this is the first time the sensible heat flux has been measured with the specific method presented in this manuscript or, more generally, applying the EC method from small UAS, but the current statement simply ignores the work done by Ghirardelli, et. al. (2024). Given the rather high discrepancies compared to the sonic anemometers, the accuracy of the measured fluxes should be put into better perspective since the uncertainty is still rather high (also for the experimental setup).*

   As we wrote above, there was probably a misunderstanding, because we explicitly included the work by Ghirardelli 2023, but now it seems the reviewer rather wanted Ghirardelli 2025 as a reference, which admittedly makes more sense. With all due respect to that work, it is quite different from what we show in this study. While in Ghirardelli et al. (2025), the sonic anemometer remains the actual measurement instrument, we enable the drone itself with an additional fast-response sensor. This is shown for the first time. We will change the statement in the abstract, as in the introduction to explicitly state that we do this measurement without a sonic anemometer or other external wind sensors.

3. *L11: I suggest simply stating the wind speed range. Furthermore, I consider 8 m/s as rather moderate wind speed and given that the minimum wind speed listed in Table 2 is 2.9 m/s, the term "low wind speed" doesn't describe the range of conditions very well.*

   On the full scale of possible wind speeds at the site and generally possible in the ABL, we believe that the statement is not completely wrong, but we change it to "low to moderate wind speeds" and give the rounded wind speed range as $3 \ldots 8$ m s^{-1} in the abstract.

4. *L30: The new statement, "Another alternative is to put sonic anemometers on UAS? with the drawback of requiring much larger systems." should probably refer to Ghirardelli et al. (2023) and Thielicke et al. (2021). The cited articles are not visible and don't show up in the bibliography. The study Ghirardelli, et al. (2024), presenting a xUAS with a sonic anemometer in a sling-load configuration, is in this case more relevant than Ghirardelli, et al. (2023), which illustrates the feasibility of using sonic anemometers on xUAS based on CFD simulations. Furthermore, "larger systems" also have their advantages, e.g., longer flight times and the capability to carry more sophisticated sensors, to name a few highly relevant advantages of this approach.*

   We apologize for the confusion with the citations and the error in the representation of the citation in the revised manuscript. This should have been Ghirardelli et al. (2023) and Thielicke et al. (2021), but there was a mistake in the bibtex file. Larger systems certainly have their advantages, but not in this context. Flight times do actually not necessarily scale with size. In the meantime, we operate a drone below 500 g which flies 70 minutes and thus longer than larger drones that carry heavy instruments, even the one mentioned in Ghirardelli et al. (2025). Carrying more and heavy instruments is the big advantage, but maybe not necessary for heat flux measurements which are addressed in this manuscript.

5. *L38: Start a new paragraph*
   ok.

6. *L31-L32: The statement is still kept too general and thus misleading. When considering a dedicated flow sensor, the separation distance between the sensor and the rotors, as well as the mechanical implementation, are important parameters. Furthermore, the sensor specifications e.g. size and sampling frequency, determine the smallest resolvable scales. For this reason, it would make sense to limit the statement to the specific sampling approach.*
   On the other hand, those parameters that you mention are very specific for putting a sonic anemometer on a drone. Those considerations are well described in manuscripts dealing with this approach. We change our sentence to: "Using the drone itself as a wind sensor, the smallest resolvable turbulence scales for multicopter UAS depend ..."

7. *Figure 2: Please correct the caption of Figure 2 (red circles).*
   We changed the caption with the correct description of symbols.

8. *Table 1: The Table has been improved a lot, and together with Table A1, it gives a clearer overview of which UAS have been used. However, I still lack a more detailed caption, allowing the reader to understand more easily what the different columns indicate. Some basic details on the source of the listed background conditions should also be provided here.*
   We expand the table caption accordingly:
   "Measurement flights from 22-25 July 2024. t is the take-off time, z gives the flight heights of the drones, the average wind direction Psi, wind speed U, turbulence intensity TI, temperature T and relative humidity φ are based on inflow mast observations and cloud cover is determined by the crew in the field."

9. *L156-L157: This is the definition of the sonic temperature so "...which is often referred to the 'sonic temperature'..." should better be changed to "..., the sonic temperature, ... "*
   Thanks, this is certainly a better way to put it.

10. *Eq. 2: Please remove the +273.15K in the equation. Conversion from sonic to air temperature works fine when using either units (K or degC) consistently. The equation in its current form only works for T in degC and Ts in K.*
    Actually, since we did define T in degC before, we need to add the $273.15°C$, because the equation requires the temperature to be in Kelvin. We should however subtract the value after the conversion in order to obtain T_s in degC as well

and be consistent, so we change the equation to:

$$T_s = (T + 273.15^oC)(1.0 + 0.51q) - 273.15^oC \quad , \tag{1}$$

11. *L161: Use "sonic temperature" instead of "equivalent sonic temperature".*
Equivalent is not necessary here and will be removed, yes.

12. *Fig 3: According to the authors' response, this figure should have been removed.*
That is correct, we wanted to remove this figure and will definitely do it in the next revision. We had only removed the text, but not the figure by accident.

13. *L169-L170: This new statement is still slightly misleading. The following should be corrected: - The bouyancy flux is proportional to cov(Tv'w') - cov(Ts'w') is a good approximation for the kinematic vertical flux of virtual temperature cov(Tv'w'), but requires scaling with density and specific heat capacity to yield the buoyancy flux - Eq. 6 should be kept as is, but should indicate that this is the approximation applied in this study to estimate the buoyancy flux from the sonic anemometer data. Furthermore, it should be made clear that you compare the sensible heat flux from your UAS to the buoyancy flux from the sonic anemometers and provide some estimate on the difference between these two under the prevailing conditions. When would the contribution of the latent heat to the buoyancy flux become problematic?*
Thanks for catching these important details that we will correct in the revised manuscript. For the last point, we think that there is a misunderstanding, because with the last revision, we converted the temperature measurements from the drones to sonic temperature, using the humidity measurements by the HYT-sensor. Thus, we do compare buoyancy fluxes and with that minimize the errors in the comparison. Of course this introduces the uncertainties from the humidity sensors, but is better than comparing sensible heat flux with buoyancy flux, which becomes particularly problematic in conditions with a very small Bowen ratio, i.e. large contributions of latent heat flux.

14. *L205-L210: This section would benefit from some cross-references to the relevant equations. E.g., I assume that a 0D calibration would result in c1 in Eq 7. being a certain constant.*
We added more references to Eq.7 and the coefficients in the text. A 0D calibration just applies an offset to the already calculated temperature based on Eq.7. You could also say it is a modification of c_0 only.

15. *Table C1: This table shows only 13 out of 25 sensors. Are the other 12 sensors 0D calibrated or not used at all? Does n.a. indicate that standard coefficients c0 and c1 are being used? I would assume that they can't be completely unknown since they are needed for the conversion from R to T. Delta T should not be given with varying precision. In the current form, a Delta T = 0 suggests that the bias of most sensors is below 0.00K, which is a bit hard to believe. Please also indicate your findings on the long-term stability (sensors 1 and 4) in section 4.4. The unit of c0 should be indicated (degC).*
The table shows those sensors that were used within this study, the others are not used. n.a. means that the coefficients are unknown, documentation of the values is missing. The conversion from R to T is done on the microcontroller of the

temperature sensor. $DeltaT$ is the offset that was applied to those sensors where a significant bias was found. For those sensors with ΔT=0, no bias correction was performed in post-processing. The laboratory calibration was found to be within the desired uncertainty. We add a statement in 4.4 about the two sensors that needed a bias correction and indicate the unit of c_0 in the table.

16. *Sect 4.5.: It is very helpful to have this section; however, in its current form, it is not clear how the different parameters are determined: the gravitation and acceleration term in Eq 8 can be easily determined from INS data, but it is not clear how T and FL are determined. In Eq9 and Eq10 it is not clear what c is and how it is determined, and why two different cases are treated depending on the sign of Fz. Are the coefficients c identical for the body frame and geodetic coordinate system? From a purely physical consideration, calculating a velocity from a balance of forces requires some integration in time. I assume that this is somehow accounted for by the coefficient c. However, this leaves me wondering about the initial conditions required for the integration and the sensitivity to sensor drift. I assume that small errors in the INS data would add up over time.*
Calculating a velocity from forces does not require an integration in time. The basic concept, as in horizontal wind estimation is a drag equation. Expanding on the wind algorithm goes well beyond of the scope of this paper and is extensively described in Wetz et al. (2021); Wetz and Wildmann (2022); Wildmann and Wetz (2022). The latter reference describes how T and F_L are determined, why two different cases are treated and how the coefficient c is determined.

17. *Sect. 5.2: Fig 7a shows four Fig 7b shows five UAS. Please adjust the text and caption accordingly, also indicating that you now show two calibration flights. I think the correction is described in Sect 4.2 and according to this and Fig 7 you convert the UAV temperature to sonic temperature (not virtual temperature). The deviations between the sonic and UAV data, and also between individual UAV data, could also originate from errors in the HYT humidity readings.*
We change the caption accordingly. It is sonic temperature and was not correctly changed in the last revision. When we calculate sonic temperature, it is true that humidity readings can lead to errors. A 5% uncertainty of the HYT humidity readings is the specified value, which at the given temperature, humidity and pressure yields an uncertainty of approximately 0.1 K

18. *L249: "mean absolute deviations" instead of "average relative deviations"*
Done.

19. *L264-L266: Can you provide an equation or a reference for the time-lag correction? I recommend to state that 20s is much longer than the stated time constant (HYT-271: <5s, HYT-R4211: <2s)*
We add Eq. 2 and a statement that it is noteworthy that the observed time constant is much larger than the specified one.
$$T_{\mathrm{corr}} = T + \tau \frac{dT}{dt} \tag{2}$$

20. *Table 3: Please use the same precision (number of digits) for all numeric results of the same parameter to allow for a proper comparison (not only in this table).*
Done.

21. *L306: should be "slightly higher"*

    Done.

22. *L317: put citation in parentheses*

    Done.

23. *L325: Figure 11 has been improved by also filtering out values not fitting the UAS criteria in the sonic-sonic comparison. However, it still looks like there are some differences. It is hard to count the unmasked data points, but it still looks like there are quite a few more in 11 a than in 11 b although n is almost equal. I also still have difficulties finding some distinctive data points in 11a e.g. for H > 400 W m-2, there are two data points in Fig. 11b but no such point in Fig 11a. If 11a should serve as a benchmark for the experiment uncertainty, I expect this to be based on the same data points as in 11b. This means the number of data points should be identical as well as the sonic heat flux values in 11b should be found either on the south or north mast in 11 a. Since the conclusion from this analysis is that the UAS-based flux measurements fall within the experimental uncertainty, it is important that this uncertainty is benchmarked as correct as possible. The argumentation in the author's reply to my previous comments (reply 50.) that strong temporal variability is often observed, resulting in very different fluxes when shifting the start and end of the averaging intervals, leaves me wondering whether different averaging intervals have been chosen. If it is the case that the averaging intervals are different, this should be mentioned to put the results in the right context, although I would prefer to see this corrected. Given the mentioned temporal variability (as shown in Figure 12), I would still advise correcting the time lag expected for the separation distance, and prevailing wind speed and direction or at least provide a proper discussion that some of the discrepancies between the two masts can be attributed to the roughly twice as long separation distance.*

    As we described within the last round of reviews, we believe that it is valuable to have as many data points as possible in the benchmark between the masts and thus use the whole period within which the UAS were flying. If we only use the exactly same data points as in the comparison between drone and mast, we do not get a very good statistic because only 32 10-minute periods remain, compared to 232 which we used for the whole period. Remember that we obtain the good statistics for the UAS comparison because we are operating multiple drones at the same time. Thus, a one-to-one comparison of the difference between mast and UAS and the two masts against each other is not reasonable in our opinion. Figure 2 shows the desired comparison matching the drone flight periods exactly. It shows that the RMSE is quite a bit larger and R^2 much smaller than for the bigger dataset. For that smaller dataset you will find the datapoints with approximately 400 W m^{-2} in heat flux. Those points correspond to the short burst in heat flux at 12:35 UTC on 22 July, which can also be seen in Fig. 12 of the manuscript. Small shifts in the averaging period make a big difference in that case due to the high temporal gradient.

    With regard to the time lag correction, we did apply it already, as described in the last review. We are sorry that we did not make this clear enough. We added a sentence in Section 5.4.2.

24. *L339-L343: mention that QAV15 and QAV25 are next to the southern mast. It is not clear that the right panels in Fig 12 correspond to the 99m level UAS and sonics. Please also indicate this in the text and the caption. Please also explain*

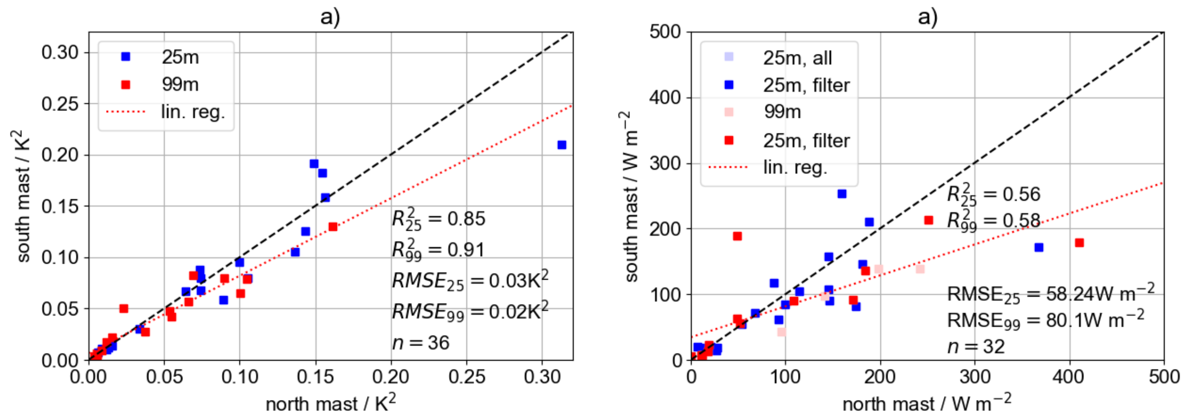

Figure 2. Comparison of temperature variance between sonic anemometers of north and south mast (a) and heat flux between the two masts (b). Shown are the comparisons at 25 m level (blue) and 99 m level (red). The linear regression is shown as red dotted line.

what the shaded areas and error bars are based on (also in the caption).

We add an explicit statement that QAV15 and QAV25 are next to the southern mast, although it can be seen from Fig.2 and Table A1. The information about the height for each panel got lost in some revision and is added again in the next version. Error bars and shaded areas are based on the RMSE that was determined in the Section 5. We add the information to the caption.

25. *L354: How do you get to the value of 1 m for the turbulent scales that can be resolved? Would it be more accurate to state the temporal resolution of 2 Hz, since this is supported by your spectral analyses?*

It is mostly an order of magnitude that we can give here. At the lower end wind speed of 2.5 m/s and the current sampling rate of 5 Hz, 1 m is the resolution in space. Describing the temporal resolution is more straightforward and we change the text accordingly.

26. *Flights 69 and 70 (Table 3) only partially support the claim that turbulence can be measured accurately.*

We described in detail why especially flight # 69 is showing some stronger deviations. Flight #70 actually shows remarkably low errors. We are transparent in stating that low turbulence, stable boundary layer measurements are particularly challenging and not very well feasible with our method. We provide uncertainties with an unprecedentedly large dataset of comparison flights in this study and thus believe that our claim is well justified.

References

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