1. Does the paper address relevant scientific questions within the scope of AMT? **Yes**
2. Does the paper present novel concepts, ideas, tools, or data? **Novel tools**
3. Are substantial conclusions reached? **Yes, but needs some more work**
4. Are the scientific methods and assumptions valid and clearly outlined? **Yes**
5. Are the results sufficient to support the interpretations and conclusions? **Yes in part**
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? **Yes**
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? **Yes**
8. Does the title clearly reflect the contents of the paper? **Yes**
9. Does the abstract provide a concise and complete summary? **Yes (minor corrections below)**
10. Is the overall presentation well structured and clear? **Yes**
11.
12. Is the language fluent and precise? **Needs some grammar and narrative corrections.**
13. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? **Yes**
14. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? **Format of the tables and some citations need change.**
15. Are the number and quality of references appropriate? **Yes**
16. Is the amount and quality of supplementary material appropriate? **Yes**

General comments:

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.

Specific comments:

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.

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?

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.

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

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.

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.

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?

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.

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.

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.

Technical corrections:

Brosy et al. 2017 – wrong DOI

Line 26: consider replacing “steady flight” with “vertical profile” or similar, since steady flight implies hover too.

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

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

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

Line 81: Consider replacing with “Wind measurements are taken by hovering the UAS in one place …”

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 …”

Line 101: “discernible systematic” reads wrong. Please correct.

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 …”

Line 160: replace coordinate directions with axes.

Line 168 and 174: citations not in the right format

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

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

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

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.