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Atmospheric Measurement Techniques Discussions

Interactive comment on "An Approach to Minimize Aircraft Motion Bias in Multi-Hole Probe Wind Measurements made by Small Unmanned Aerial Systems" *by* Loiy Al-Ghussain and Sean C. C. Bailey

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We would like to thank the reviewer for their time spent in review of this manuscript and providing their detailed comments and suggestions. We have prepared a revised manuscript which we believe addresses the concerns and comments raised in their review. In the revised manuscript, specific changes are indicated in blue text, with each of the reviewer's comments addressed below.

1. 1. The introduction in my opinion misses a clear line of argumentation towards

the main goals of the presented work.

The authors added the following statement to strengthen the connection to our main goals: "This contamination results in over- or under- estimation of the wind vector and, in particular, errors in estimation of turbulence statistics (e.g. momentum fluxes, dissipation rate, turbulence kinetic energy) measured by the sUAS. Hence, it is vital to minimize errors in the wind components measured by sUAS."

2. l2: "aircraft spatial orientation, translation and velocity"; I feel there is some inconsistency/inaccuracy that should be clarified: a. Aircraft spatial orientation: do you mean attitude with respect to the Eulerian angles for Pitch, roll and yaw or something else?

   Yes, we were referring to Euler angles (pitch, roll and yaw) in the present case. The statement as written was intended to be a more general statement as other approaches can also be used to describe the aircraft orientation in Earth-fixed coordinates. We have revised this sentence to explicitly mention Euler angles

   b. translation is already a velocity; do you want to distinguish between translational and angular velocities? be more clear and concise here!

   The authors have revised the sentence as: "Multi-hole probe mounted on an aircraft provide the air velocity vector relative to the aircraft, requiring knowledge of the aircraft spatial orientation (e.g. Eulerian angles), translational velocity, and angular velocity to translate this information to an Earth-based reference frame and determine the wind vector."

3. l12: insert "a" before "wide"

   The authors have added it.

4. l13: insert "," before "such as"

   The authors have added it.

5. l15: you are citing a lot here, which is in general not bad, but if you decide to go so broad out, then I feel that in particular for turbulence there are some central references missing, e.g.:

   - Mansour, M., Kocer, G., Lenherr, C., Chokani, N., & Abhari, R. S. (2011). Seven-Sensor Fast-Response Probe for Full-Scale Wind Turbine Flowfield Measurements. Journal of Engineering for Gas Turbines and Power, 133(8), 081601. https://doi.org/10.1115/1.4002781
   - Calmer, R., Roberts, G. C., Preissler, J., Sanchez, K. J., Derrien, S., & Oapos;Dowd, C. (2018). Vertical wind velocity measurements using a five-hole probe with remotely piloted aircraft to study aerosol–cloud interactions. Atmospheric Measurement Techniques, 11(5), 2583–2599. https://doi.org/10.5194/amt-11-2583-2018
   - Båserud, L., Reuder, J., Jonassen, M. O., Kral, S. T., Paskyabi, M. B., & Lothon, M. (2016). Proof of concept for turbulence measurements with the RPAS SUMO during the BLLAST campaign. Atmospheric Measurement Techniques, 9(10), 4901–4913. https://doi.org/10.5194/amt-9-4901-2016

   those references should also be used again when introducing multihole probes on sUAS, i.e. p2, l40/41

   The authors have added the references as suggested by the reviewer.

6. l22: what is the flexibility in a profile? do you mean the flexibility in choosing ascent/descent rates, or just the flexibility in location that is already stated before?

   What we meant by the profile flexibility is the ability of the sUAS to fly in different patterns easily compared with large manned aircraft. We have changed profile to flight path in the text.

7. l26: I think you forgot temperature as the most commonly sampled parameter!

   Definitely an oversight! The authors have added temperature to the list.

8. l37/38: “Usually, wind velocity measurements by fixed-wing sUAS require velocity probes with slightly better temporal response than Sonic anemometers (Witte et al., 2017; Mayer et al., 2012).”; My first response here was “Why?”; what exactly do you want to express here; there are sonics around with 100 Hz measurement capability matching the typical sampling frequency of multihole probes.

   We were trying to highlight the utility of five-hole-probes relative to other available technology. Although such high frequency sonic anemometers do exist, to our knowledge, these anemometers do not exist in a package size sufficiently lightweight to be used on a fixed-wing sUAS. Hot-wire anemometry is also an option, but is not yet in common usage for sUAS measurements and, to our knowledge, multi-sensor hot-wire anemometry capable of resolving the velocity vector has not yet been utilized on sUAS. The authors have removed this sentence as the justification for using multi-hole probes was provided in the sentence following the one described above, and no further justification for their use relative to sonic anemometers is required.

9. “large wind velocities”; that should here better read “large flow velocities”, as we are talking about the relative flow between the probe and the air

   The authors have replaced “large wind velocities” by “large flow velocities” as the reviewer suggested.

10. l49: “fly at velocities an order of magnitude greater than the wind velocity”; that can of course be the case, but in reality you easily can fly a fixed-wing in wind speeds up to 80% of the cruise speed of the aircraft, so you should not generalize this statement

    It’s hard to imagine our aircraft successfully flying in the ABL turbulence produced by 16 m/s winds, but the reviewer is correct and we have revised the sentence as follows: “These aircraft have the ability to fly at velocities up to an order of magnitude greater than the wind velocity.”

11. l60: replace “multi-hole probe” by “multi-hole probes”

    The authors have replaced “multi-hole probe” by “multi-hole probes”.

12. l61: remove “with” before “a central hole”

    The authors have removed “with”.

13. l61: I suggest to replace “parallel to” by “in line with”

    The authors have replaced “parallel to” by “in line with”.

14. l85: Equation 6 and corresponding text: that reminds me very strongly to Don Lenshows basic work; maybe a good idea to refer to!

    The authors have added the reference to Lenschow’s work.

15. l116: insert "the" before "determination"

    The authors have added “the”.

16. l126: “... and should include multiple changes of direction of the aircraft.”; How relates this statement to the calibration maneuvers suggested/required by Lenschow?

    Lenschow and several scholars suggested different calibration maneuvers to correct the sideslip angle and the angle of attack of multi-hole probes as reported by (Drue and Heinemann, 2013) to account for the flow distortion around the aircraft body. The maneuvers reported vary from straight flights to race tracks and other patterns depending on the parameters that needed to be corrected. These same maneuvers would be suitable for the correction described in this manuscript.

17. (a) l175 (and other occasions): “horizontal wind velocity magnitude”; I strongly suggest to use “horizontal wind speed” instead, this is the meteorologically correct term here The authors have replaced the term “horizontal wind velocity magnitude” by “horizontal wind speed” throughout the text.

    (b) figure1: has apparently the wrong y-axis label (should be wind direction in degrees); I also highly recommend not to use line plots for wind direction!; how is the downsampling done, just picking an individual value or applying some form of averaging? In addition I would just use horizontal wind speed, maybe abbreviated as vh as y-label for

    (c) figure2: same comments as for figure 1:

    We have replaced the labels on Figure 1 and Figure 2 with “Direction [°]” with “ζ [°]” to be consistent with the later figures and nomenclature used in the text. We deemed that line plots would be more precise for direction in this case since there were little to no instances where the direction changed across the $360°/0°$ transition and the line plots were more precise and more clearly showed measured periodicity. However, to avoid any misinterpretation on the readers’ part, we have also changed Figure 1 and Figure 2 to dot plots. Our use of the definition of magnitude for the horizontal wind speed was to minimize any confusion with the velocity components $[U, V, W]$ which were defined earlier in the text. Following the reviewer’s suggestion, we have defined V_h and used that for horizontal wind speed. Downsampling was conducted by plotting every 200th data point with no additional anti-aliasing filter applied. We have fixed the caption for Figure 3.

    (d) figure 3: again the velocity labels could be much easier and intuitive vh MURC and vh sUAS;

    We have changed Figure 3 labels to provide horizontal wind speed as V_h.

    (e) in addition there is something mixed up in the figure caption: Wind speed is a) and c), not a) and b) as stated in the caption.

    This has been corrected.

18. l219: "there is little-to-no evidence of this periodicity! is a rather brave statement; I still see some clear indications of such a periodicity and a formulation

in the direction" shows a distinctly reduced periodicity" sound to me much more appropriate! figure6 and discussion.

The text has been changed as suggested.

19. l221-227;

    (a) what exactly do you want to achieve with this potential temperature profiles? If it is just to give an overview on the state of the atmosphere, then you should distinctly simplify your presentation by e.g. only showing the 4 average profiles (e.g by bin averaging over 25 m vertical intervals;

    The authors intended to give an overview on the condition of the atmospheric boundary layer to put the fluctuations of the wind into context. (e.g. fluctuations caused by different turbulence intensity at different altitudes corresponding to the boundary layer stability rather than due to uncorrected bias). However, based on the comments of both reviewers, who saw little value in these profiles, we have removed the potential temperature profiles in the revised manuscript.

    (b) if you are also interested to present an inter-flight variability you can achieve that by using whiskers around your bin mean); as it is it is a rather hard to read/interpret figure figures 7 and 8: those are just messy in the present form; if you want to keep the shown information you could have this as a background with grey and reddish color, but on top you should again show some bin averaged values that would then give a clear picture how mean value and variability/standard deviation react on the proposed correction.

    The authors have revised these figures as the reviewer suggested.

20. l235: insert "," before "even"

    The authors have added ",".

21. l237: "suggesting a potential time response lag between the five-hole probe and inertial measurement unit"

    (a) insert "the" before "inertial measurement unit"

    The authors have inserted "the" before "inertial measurement unit".

    (b) this has been reported before: Båserud, L., Reuder, J., Jonassen, M. O., Kral, S. T., Paskyabi, M. B., & Lothon, M. (2016). Proof of concept for turbulence measurements with the RPAS SUMO during the BLLAST campaign. Atmospheric Measurement Techniques, 9(10), 4901–4913. https://doi.org/10.5194/amt-9-4901-2016

    The authors have added the following: "which agrees with what was reported in (Båserud et al.,2016)."

22. l244: insert "," after "For Z>200 m"

    The authors have added "," after "For Z>200 m".

23. l256: remove "measured" before "profiles"

    The authors have removed "measured".

24. l264/265: how will a systematic time shift, e.g. introduced by a time delay of the data output of the IMU (as hypothesized in line 237), affect the correction procedure and your results? It might be worth to test (e.g. by a correlation analysis) if there is such a systematic time delay in your data set.

    This particular error is compensated for with the Δt term in the correction procedure and is currently one of the larger sources of error requiring correction in our data acquisition systems. We are currently logging IMU/GPS, pressure transducers, and PTU on three separate systems at different rates and time stamps (details provided in Witte et al. 2017). In post-processing we re-align them through cross-correlation of common parameters within each system, but

have found that the automated correlation process can be off (when sampling at ~ 200 Hz, even small-time misalignments can cause headaches) so the single optimized time shift was introduced to correct for this misalignment. Hypothetically, the approach could be expanded to account for multiple time shifts which might be introduced due to arrangements, sensor lag, etc. and is something we are currently exploring to correct for strong misalignments introduced during takeoff/landing maneuvers which we believe is due to delays introduced by the EKF calculations made by the IMU.

25. l267: insert “been” after “increasingly”

    The authors have added “been” after “increasingly”.