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Comment on amt-2022-113

Anonymous Referee #2

Referee comment on "A stand-alone calibration approach for attitude-based multi-copter wind measurement systems" by Matteo Bramati et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-113-RC2, 2022

Bramati et al. present a study about wind measurements with a multicopter UAS. The title suggests that a new "stand-alone" calibration approach is presented. Another novelty, according to the abstract, is the "isotropic" shape of the copter frame. Both of these topics are of interest for the community and could foster better measurements with multicopter UAS. However, these topics are not really well substantiated with systematic experiments and analyses. Only vague statements are given. The rest of the manuscript deals with algorithms that have been presented and validated with more comprehensive datasets before. For these reasons and the detailed explanation below, I can not suggest the manuscript for publication in AMT at this point. I am afraid that more tests and more data would be necessary to make a solid case about the two key topics of the manuscript (i.e. stand-alone calibration and independence of wind measurements on flow direction).

General comments:
- The title of the manuscript is a "stand-alone calibration approach". But why is an in-flight calibration actually necessary? What is the robustness, especially compared to dedicated calibration measurements? In general, the calibration flights that are presented are not well described. A time series of wind speeds during the calibration would be necessary to judge stationarity of the flow during calibration. What are the limits of the calibration approach in terms of wind speed and turbulence conditions? What are the expected uncertainties?
- How does this calibration approach differ from a calibration approach that is described for the PX4 autopilot: https://docs.px4.io/v1.9.0/en/advanced_config/tuning_the_ecl_ekf.html#mc_wind_estimation_using_drag ?
- Does the sphere really create rotationally symmetric flow around the multicopter? The rotors are a very important part of the aerodynamic features of the UAS and probably have more effect than the frame shape. I would at least have expected a graph showing the calibration error versus flow direction. There is probably not enough data there for this purpose, but I do not think that any conclusive statement can be made without such tests.

Specific comments:

p.1, l. 7: isotropy the right word? I do not think it should be used for the shape, but for the attributes of an object / material. Example: a perfect wooden shpere is anisotropic, because its meterial properties like shear stresses etc are different depending on the grain direction.
p.1, l.17: sonic anemometers do not need wind vanes, because they provide at least the two-dimensional wind vector.
p.2, l.47f: "linear behaviour" I am not sure what is meant by a linear behaviour of the parameter and I do not believe that Wetz et al. do assume a linear behaviour of the drag equation.
p.2, l.50: I would prefer "symmetry" over isotropy, see above.
p.2, l.56: multicopter
p.7, l.165: PosHold mode is not explained.
p.8, l.186: Were surface wind speeds measured? If so, where and how?
p.9, l.206f: "disturb the UAS with the same magnitude in an opposite way..." This assumes that wind is constant during the flight, but during an afternoon with low wind speeds, radiation input and probably turbulence, this assumption can be violated significantly.
Fig.4: There seems to be no scale or information about the position and extent of the map.
p.11, l.222: what is 8-1 ms^{-1}?
p.12, l.239f: I do not understand this statement "around 10 for 1 ms^{-1}". 10 what?
p.13, l.247f: "wind to be constant". It would be good if you showed this with independent measurements. Turbulence occurs on a wide range of scales and can significantly disturb scales within the flight time.
Table 3: I am much concerned about the comparison of ERA 5 data with a grid size of 9km and local measurements in quite complex, heterogeneous terrain as in Poltringen. Can it really be expected to match?
p.14, l.269: but is ERA5 a good comparison?
p.14, l.278 and p.15, l.291: What does this RMSE represent?
p.16, l.306: every sensor can only resolve signals up to half of the sampling frequency according to the Nyquist theory.
p.16, l.313: but the time lag depends on the wind speed. Has this been considered?
Sec.4.1: As the text says, turbulence is out of scope of the work. I that contxt I wonder what the section and Fig. 9 add to the goals of the manuscript. Maybe they can be removed. If it is only used to show that there is noise above 0.2 Hz and data needs to be rejected above that frequency, this could be mentioned early in the data processing.
p.20, l.356ff: I have doubts about these opinions and theories. I think they should not be presented without evidence, because it could confuse readers. A simple explanation for a plateau followed by a steep decay often is a noise level in the dynamics (e.g. by vibrations) and low-pass filters in the senors at higher frequencies (here the Kalman Filter of the angle solution).
p.20, l.370: It is unclear how MBE and RMSE are calculated. What are the considered averaging periods. What is the cause of the MBE?
Sect. 5.5: I do not understand why there is a subsection "vertical velocity and mass" and then subsections for each of those terms on the same level.