

Interactive comment on “Flying UltraSonic – A new way to measure the wind” by Martin Hofsäß et al.

Anonymous Referee #2

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The paper contains many grammar mistakes.

There is no discussion of calibration of the sonic anemometer with respect to induced flow from the rotor or from flow around the body of the aircraft in flight. The author states that the anemometer is mounted outside the "main influence" of the rotor wash, but it was not quantified how this was determined.

This type of measurement, especially using a rotary wing aircraft, is very power hungry. More discussion is needed to compare and contrast the benefits of long term measurements from a met tower with the spatial freedom and ease of deployment of a UAV.

Section 2.1, line 11 is unclear. perhaps provide a figure?

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The paper discusses benefits to measurements in complex terrain, but the experiment is in simple terrain. Is this a starting point to more advanced measurements?

Pg 5 lin 8. Mentions WEA1 but then not referenced elsewhere.

Pg 5 lin 9 "Previously defined position" unclear

***The term airspeed is used repeatedly in terms of corrections. This is wrong. The 3D airspeed is measured by the sonic and the windspeed in a fixed coordinate system is the airspeed corrected by the groundspeed and aircraft rotations.

Pg 7 line 17: Rump?

Pg 7 - Use something other than red to denote a new frame

Pg 9 line 25 - does the sonic have the same measurement range and sensitivity in all 3 axes in order to justify turning it horizontally?

*** Rather than upsampling data assuming it's periodic, it's probably best to compare like signals at their lowest sample rate, 10 Hz.

Pg 13 line 20: Was convective time between measurement locations accounted for? Additionally, saying "same general trend" is a very ROUGH statement and debatable

***Pg 14 line 3 "Very good agreement" is debatable. There seem to be large deviations in secondary quantities like v , w and wind direction.

Pg 16 line 6: Provide justification, discuss affect on results.

Pg 17 line 2: "hardly differ" is a debatable point, given the log log format. Additionally, given statement on pg 16 line 6, did that cause the PSD to align better?

Pg 17 line 7: The argument that the effect of rotor RPM can be excluded seems flawed. High RPM motion can still lead to lower frequency phenomenon.

Pg 17 line 9-10: What is the expected Kolmogorov scale and its relation to the dropoff in PSD?

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Pg 19 line 4-5: Yes, the concept works. There seems to be much room for improvement.

Conclusions: Prior to focusing on duration and sample rate, focus should be placed on better correlation between measurements and additional investigation into induced flow influence due to the aircraft and its rotor.

Interactive comment on Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2019-81>, 2019.

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