Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-233-RC1, 2017
© Author(s) 2017. This work is distributed under
the Creative Commons Attribution 4.0 License.

Atmospheric Measurement Techniques Discussions

Open Access

Interactive comment on "3D Wind Vector Measurements using a 5-hole Probe with Remotely Piloted Aircraft" *by* Radiance Calmer et al.

Anonymous Referee #1

Received and published: 15 September 2017

Review of manuscript '3D Wind Vector Measurements using a 5-hole Probe with Remotely Piloted Aircraft' by Radiance Calmer et al.

A) general comments

The manuscript is not well focused. Some statements are questionable. The presented wind-measuring and calibration method is not accurate and in parts wrong. Therefore plenty of confusing corrections were applied to the measurements and lots of discussion / arguing was used to justify missing agreement with other measurements and theory, especially in sections 4 and 5. The results are overall not convincing.

The authors should have a decent look at publications and textbooks that describe the use of flow probes (like a 5-hole probe) aboard small aircraft in detail and then repeat

Printer-friendly version

Discussion paper

their raw-data processing.

Sorry for being harsh.

B) specific comments

I am using the following method to name page and line: page/line. E.g. 5/17f means: page 5, line 17 and following lines

* 2/6: 'INS measures six axes': No! It measures 3 linear and 3 circular accelerations or motions.

* 2/14: Having a look in Reuder 2008 shows: SUMO was not able to measure the wind vector. Actually it had no flow sensors aboard at that time. Wind speed and direction were estimated by the drift of the aircraft, instead. An early example of a small and light RPA that actually measured the wind vector was published by Spiess et al in 2007 (already in the list of references).

* 2/19: very unusual and confusing way to cite articles: M^2AV-Spiess and MASC-Wildmann! So, the authors names were Spiess (2007) and Wildmann (2014). Aircraft described in these articles were named M^2AV and MASC. Please correct!

* 2/22: Having a look at the sophisticated error analysis of van den Kroonenberg et al (2008), I doubt that Thomas et al (2012) and Baserud (2014) measured the vertical wind 5 times more accurately, the latter with a much simplier measurement system. Please explain how this was possible!

* 2/26 Now measurement of the vertical wind seems to become 20 times more accurate. Explain how this was possible!

* Are these accuracies (from 2/16 to 2/26) all the same by mathematical definition? Do you mean absolute accuracies of the mean vertical wind or resolving turbulent fluctuations? How are the various precisions for the wind velocities in the referenced literature calculated? Are they comparable with each other?

CC BY

* 2/30 What do you mean with '(4) improved algorithms for wind field estimation from dynamic soaring'?

* 4/19 Indeed, it is possible to use a 5-hole probe with only three differential pressure sensors. But considering publications e.g. by the American Institute of Aeronautics and Astronautics (look for Weiss et al. 1999 to 2002), using 5 differential pressure sensors increases accuracy (measuring the pressure difference of the 5 holes relative to a combined reference), significantly. Since a pressure sensor is light and cheap, can you explain why you decided to use only three?

* 4/10ff (section 2.2) Using a 5-hole probe also requires a proper tubing strategy, see Wildmann et al (2014), in order to avoid signal damping and acoustic tube resonance. Can you please explain how did you take care of tubing issues?

Summing up section 2.2: Why don't you apply well known, state-of-the-art and published (e.g. articles that are already in your list, by van den Kroonenberg and Wildmann et al.) methods for tubing your 5-hole probe? Or in other words: What are the advantages of your unusual method? And can you show that the results are still good enough?

* 4/20 'hole 1 measures the total pressure' - No! And this is a substantial mistake. Since the stagnation point (angle of attack and side slip, \alpha and \beta being not zero) is *somewhere* on the spherical surface of the 5-hole probe, hole 1 does not represent the total pressure! Doing this mistake, the following discussion of wind measurements and accuracies is pointless!

* 4/24f No. An IMU measures accelerations (3 linear and 3 circular). An INS combines an IMU with GPS, usually using a Kalman filter.

* 4/29 'plane'. You mean aircraft or aeroplane.

* 5/1: No! The Eulerian angles are NOT given in the aircraft coordinate system, but in the Earth's coordinate system. This is why people invented INS. And this is important,

CC BY

if you want to measure the atmospheric wind using an aircraft!

* Entire section 2.3 (page 5): So, you have an INS that delivers the Eulerian angles (roll, pitch, heading). And you have a 5-hole probe that (when properly tubed and calibrated) delivers angle of attack and side slip (\alpha and \beta) as well as the total pressure (although you will not have them with the methods described in the manuscript ...). Now you can apply the exact equations, published since the 1970ies, also by Don Lenschow and others, without having to apply any simplification or estimation that only causes worse data. To have a nice and short overview on how to do this, please read (and apply) van den Kroonenberg et al (2008).

* 5/4 Even straight and level flights have varying Eulerian angles. Roll and especially pitch are not zero! The simplified equation (1) for the wind vector possibly holds 1) for heavy and large manned aircraft (talking about airliners) that are less dynamic during flight, and 2) probably only for the estimation of the mean wind vector. But a tiny RPA as used in the presented study is heavily agile. Reading the manuscript I do not see any reason not to use the precise equations (again, see e.g. van den Kroonenberg et al.), or is there any?

* 5/11 V_e etc is confusing. Why don't you use (V_x,V_y,V_z) or (U,V,W) for the ground-speed vector, since you defined your Earth coordinate system in the same paragraph using (x,y,z)?

* 6/1 Eq. (2) is not correct. The pressure differences in the denominators have to be divided by the dynamic pressure enhancement, which is the difference between the total and the static pressure. Hole 1 does not deliver the total pressure! See above.

* 6/6 Where does Eq. (3) come from? Please cite literature!

* 6/21: Why do you need a linear relation between C_alpha/ C_beta and alpha/beta?

This is leading to the question how alpha and beta are calculated from the calibration to obtain the wind vector components. Using the system e.g. described in Wildmann

Printer-friendly version

Discussion paper

CC BY

(2014), alpha/beta of up to +-20 degree can be used and a polynomial fit accounts for potential asymmetry of the probe's tip structure.

* Entire section 3.1: There is no need to invent the wheel once more. How to calculate the wind vector and how to calibrate a 5-hole probe is well published and explained. For example in articles in your list of references, see again van den Kroonenberg (2008), there: page 1972f, Eq. (1) to (13).

* 7/7ff: Again, the pressures measured on the dynamic and static pressure holes of the 5-hole probe (in your manuscript delta(P1-P6) and Ps=P6) change with alpha and beta. Moreover other work shows, that the static port of five hole probes fluctuate for inclined inflow.

* 7/9 Why did you calibrate with wind speeds between 12 and 34 m/s? Of course, a single calibration of the 5-hole probe (the entire grid, see Fig. 3 and 4) holds only for one airspeed, i.e. the entire procedure has to be repeated in e.g. 1 m/s steps. This leads to the question, how your autopilot system is controlling the air speed of the RPA?

* 7/13f: What is "triangular motion applied to the pitch axis of the platform"? Why is that performed?

* 7/15: Actually Fig. 5 shows plenty of noise, causing a systematic uncertainty of the vertical-wind measurement of about 0.1 m/s. This means turbulent fluctuations in this order of magnitude cannot be resolved by the presented system. Please make this clear!

* Entire section 3.2: Eventually averaging leads to a mean vertical wind about zero. Having in mind the mean vertical wind should be about zero in the ABL if you average long enough, this is ok if you are only interested in the mean wind. But why is there so much noise? Possibly caused by the electronic pressure transducers?

* 7/28 It is the Gaussian propagation of errors, not the maximum error propagation -

CC BY

this should be mentioned!

* 8/4, Eq. (6a): please define a_\alpha !

* 8/5, Eq. (6b): please explain the entire equation!

* What is missing in section 3 or 4 is the most simple test to see if the calibration of the system works at least for the mean wind vector: Flying identical legs in opposite direction in a calm atmosphere (e.g. the residual layer, or in an almost neutral stratification under a overcast sky). See also Fig. 6 and Eq. (17) in van den Kroonenberg et al. (2008). Can you show that the heading does not influence the wind measurements?

* Section 4.1: It is very important to check the power spectra of the resulting wind (of course only if the mean-wind check was ok, i.e. identical legs in opposite direction in a calm atmosphere). Spectra show systematic errors as visible in Fig. 7, above 1 Hz (can be the noise level). These are not mentioned in the manuscript - please do so!

However, the mean spectra in Fig. 7 show two critical issues:

1) the Kolmogorov slope is NOT achieved neither with the sonic nor with the RPA data. I do not agree with your text in 9/7. Your spectra have significant different slope. But this can be caused by not having ideal conditions for a locally isotropic turbulent sub-range. More critical (not acceptable) is the following:

2) the spectral power of the RPA data is by a factor of about 5 larger than the spectral power of the sonic. As you mention in 9/8, this was caused by 'the motion of the RPAS'. But if the measured data is governed by the aircraft motion and not by the atmospheric turbulence, any further analysis of turbulence is useless and futile!

* Fig. 8: There is a huge difference between sonic and RPA data around zero vertical wind, please explain!

* Fig. 9: There is a huge difference between mast and RPA data around zero vertical wind. Is there any easy explanation? Section 4.2 is not helpful but confusing.

CC BY

* 9/13 'This step is needed ...' I do not understand this - the attitude of the aircraft and the 5-hole probe (assuming there is no mounting error) is known from the Eulerian angle delivered by the INS. And the mounting error of the probe to the aircraft is constant.

* Section 4.2: What is the intention of this analysis of the TKE? What shall be learned? Why is it filled with corrections? I am quite sure that these considerations become unnecessary after doing a correct wind-vector calculation (see above).

* 9/30 TKE: From the section 3.3 we know that the uncertainty for the vertical wind is 0.1 m/s. Thus the measurement system causes (possibly by electrical noise) already a standard deviation in this order of magnitude in the data. How large is the uncertainty for the horizontal wind? This is important to know in order to estimate the significance of the presented TKE data.

* 10/2 I doubt that reported TKE deviations between sonic data and other small RPA is increasing faith into the presented method or is explaining any physics. What is your message here?

* Section 5: The differences and shifts in the distributions shown in Fig. 12 to 15 are mainly in the order of magnitude of 0.1 m/s. This is the systematical uncertainty caused by the measurement system and explained before. And Fig 8 shows that the RPA was not able to measure small vertical wind speeds adequately. Considering this, what insights are left?

* 11/28 The abstract says 'are now able to accurately measure ... even in clouds'. But now it is written that water is accumulated within the probe, making it useless. It seems (what could be expected) that 5-hole probes cannot be used in clouds, can they?

* 14/17 'Motions induced ...' Well, this can be expected in case the wind was properly calculated using the correct formulas!

* 14/19 My suspicion: the simplified wind equations (that possibly hold for large and

CC BY

heavy aircraft) and the faulty calibration caused all the insufficient agreements between the RPA data and other data and theory.

* 14/24 ’following Kolmogorov’ - actually, not really. See above.

* 14/25 Considering that the Kolmogorov distribution was not measured, I doubt that any isotropy of the turbulent flow can be assumed. Can you prove that the variances of the two horizontal wind components are equal, as written in the text?

* 14/27 This is not surprising. Without a proper heading you cannot measure the wind vector. How accurate was the heading so far?

C) technical corrections

Here are some minor comments:

* Proper use of hyphen! E.g. line 3 on page 2: ’boundary-layer turbulence’ would be correct. Also correct would be (in contrast), again in line 3 on page 2: ’aircraft based wind measurements’.

* Language has to be improved, e.g. in line 2 on page 2: ’vectors are an essential parameter’ - this doesn’t make sense.

* Using the cross ’x’ in equations is usually reserved for the vector product, not for normal scalar multiplication.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-233, 2017.

Printer-friendly version

Discussion paper