Review of the manuscript AMTD-4-1303-2011

Title: "Measuring the 3-D wind vector with a weight-shift microlight aircraft"

by S. Metzger et al.

Manuscript evaluation

Scientific Significance: Good (2)

Scientific Quality: Good (2)

Presentation Quality: Fair (3)

General Comments

The subject of this manuscript is the calibration of a five-hole turbulence probe on WSMA aircraft, which is a relatively original subject, well within the scope of AMTD, with good research applications. As the authors conclude the main issue is wing upwash correction due to the specific conditions in WSMA: trike rotational freedom (mainly roll angle difference from wing) and aeroelastic wing. The paper is probably too long and difficult to follow due to many redundant technical details, which could probably omitted or simply mentioned in short. For the same reason the Appendices could be omitted because they reproduce other papers (Lenschow, 1986; Williams and Marcotte, 2000).

Among the minor points given below there is also a major point in data processing. More specifically Figure 8 shows decrease of upwash angle with measured lift coefficient, which is unrealistic and the possible cause is mentioned. This is a critical point because this negative slope is used to establish a real-time correction for upwash, which however is in error.

Specific Comments

Page 1306, third line from the end: The root mean square deviation should probably be renamed to root mean square error in order to discriminate it from the standard deviation which has to do sensor uncertainties.

Page 1310, Eq. (1): The accurate definition of lift is the force (i.e. acceleration) perpendicular to the free airstream instead of the vertical one. The difference may be significant when there is significant vertical velocity of the aircraft e.g. fast ascent and descent during forced oscillation maneuvers (calibration flight patterns).

Page 1310, Eq. (3): This equation is valid for solid wing with elliptical loading. The wing of WSMA is quite different not only due to aeroelasticity but also due to its shape (delta like wing with considerable sweep and small aspect ratio, i.e. significant downwash induced by wing tips), which results in different (lower, i.e. less lift) proportionality factor between upwash velocity and the product $V_{tas}CL$.

Page 1311, top and Page 1321, line 12: The "upwash attack angle ξ" is simply the angle of the longitudinal body axis with the line connecting the probe, which is below the wing, with the wing aerodynamic center (pressure center) and should not be called "upwash attack" angle in order to avoid confusion with upwash attack angle in Eq. (7) and Fig. 8.

Page 1311, second paragraph: Due to quite possible deviation of the flow around probe from the theoretical spherical model (which the authors have actually found during calibration) it is useful to measure dynamic pressure also with a Pitot probe which is quite insensitive to flow angles up to 20 degrees. In this way, they will be also able to diagnose the deviation of the flow around the probe from the spherical model. Such a test is described in section 3b of Kalogiros and Wang (2002a).

Page 1314, Eq. (4): There is an error. The β (sideslip angle) and α (attack angle) should replace each other in the equations. Also, the authors with the terms "mechanical" and "measured" flow angles probably mean spherical coordinates (like latitude and longitude) and "projection" angles (like the ones used by Lenschow, 1986 or Williams and Marcotte, 2000). The sideslip angle is the same between these angle systems, while the attack angle differs (in the latter system it is the "latitude" angle of the projection of the point of the sphere on the plane $\beta=0$).

Page 1314, Racetrack pattern: Mean wind direction is usually not known with sufficient accuracy at flight level to use it for in-flight calibration purposes. A better method for calibration using this flight pattern (also known as reverse heading maneuver) is to carry it out in a random direction and require that the estimated components of horizontal winds are the same in both directions, which differ by 180 degrees. This can be done by comparing the average wind components or minimizing the total difference of wind components estimated at the same positions of the flight directions.

Also, with the phrase "…adjusting dynamic pressure in Eq. (A8)" the authors mean estimating a calibration bias (offset) or slope? Their Table 4 implies probably the second. What could be the reason for this slope (higher than unit)? It is the deviation from the spherical model of the flow around the probe or the flow distortion by the aircraft? The first should be taken care by the wind tunnel calibration. The second is usually known for aircraft with fuselage and pressure ports on it as static pressure defect, which also affects (increases) the measured static pressure at the same magnitude but with opposite sign and this is not applied by the authors.

In addition, the turbulence probe is within the propeller flow "tube", which implies an increase of measured dynamic pressure and a decrease of the same magnitude of the measured static pressure relative to the free atmosphere. The engine is probably weak and the distance of the probe to propeller is probably large enough at 3.5 m, which may result in small effect of the propeller on the probe measurements. The level acceleration-deceleration flight pattern (constant altitude speed run maneuver) can show this effect of the propeller on measured static pressure as the difference between acceleration (close to full engine thrust, maximum propeller effect) and deceleration (low engine thrust, small propeller effect) as described in section 2 of Kalogiros and Wang (2002b).

Page 1317, Eq. (6): The uncertainties of airspeed and sideslip angle given are actually uncertainties of wind components. The uncertainty σ_β is not even dimensionally correct (m/s units, not degrees) to claim it a sideslip uncertainty.

Page 1318, VW3 (Forced oscillation): The aerodynamic response of wing to forced oscillations due to pilot actions is different from the response to turbulence (traveling air disturbances, wind oscillations). More details on this difference and the real-time estimation of turbulence effect on upwash are given in section 3 of Kalogiros and Wang (2002b). Thus, this maneuver does not give the correct information for evaluation of the effect of thermal turbulence in ABL on the flow around the aircraft.

Page 1324, line 11: Define "working" angle.

Page 1324, Step C – Tower fly-bys: As mentioned in a previous comment if an offset adjustment due to position error is applied to static pressure measurement a same magnitude, but opposite sign adjustment should applied to dynamic pressure (i.e. total pressure remains constant). Also, "Table 3" in line 21 should be "Table 4".

Page 1326, end of page, Fig. 7, and page 1327, top of page: As pointed out in a previous comment Eq. (3) is not expected to be exactly valid for the wing of WSMA. However, the main conclusion from Eq. (3), which is that upwash is proportional to airspeed and lift coefficient, should be valid but with smaller magnitude (i.e. less lift) of the proportionality factor.

During the forced oscillation maneuver airspeed also varies in addition to lift coefficient and in a different way. Thus, there could be a phase difference between lift coefficient and upwash. Also, there are significant altitude changes (i.e. significant vertical velocity of the aircraft), which give a small error in the estimation of lift by Crawford's model Eq. (1) as mentioned in a previous comment. Furthermore, there should be a propeller effect during accelerations as mentioned in a previous comment. I assume that the measured upwash was estimated as the remaining air vertical velocity assuming zero actual wind velocity above ABL and that the proper rotational transformation has been applied with a ξ angle of -41.9 degrees and roll angles difference between trike and wing, because the probe is below the wing and, thus, the upwash direction at the probe is not vertical. With the above details in mind I don't think that it can be concluded from Fig. 7 that the general upwash model (upwash, i.e. wing circulation, proportional to airspeed and lift coefficient) is not valid in the case of WSMA. This conclusion would be unrealistic and not in agreement with typical aerodynamics.

Page 1328, Eq. (7) and Fig. 8: Continuing the previous discussion for the upwash model, the proportionality of upwash with airspeed and lift coefficient translates to an upwash attack angle proportional to the lift coefficient. It's typical in aircraft aerodynamics that the lift coefficient of a wing increases with attack angle of free airstream ranging from zero lift angle to stall. For the aeroelastic wing of WSMA the lift coefficient may be simply considered to be dependent on airspeed due to changes in the shape of the wing as the authors point out at the end of page 1308 (section 2). If the negative slope seen in Fig. 8 was real this would imply that when lift increases (i.e. wing circulation increases) then upwash decreases. But, wing circulation is proportional to upwash. Also, a negative slope implies that at zero lift coefficient (i.e no lift, no wing circulation and no upwash) the upwash attack angle is maximum!!! Thus, the negative slope of upwash attack angle versus lift coefficient cannot be realistic. If the authors used Eqs. (8) and (9) from Garman et al. (2008) to compute the upwash attack angle I note that there are sign errors in tan(β) and especially the sin(θ) term in these equations. I think that this is the reason in that paper the authors have also "observed" a negative slope of upwash attack angle versus lift coefficient similar with the current paper (despite their aircraft had a rigid wing unlike WSMA). If in addition the authors of the current paper used this attack angle to estimate upwash after multiplication with airspeed then the measured upwash in Fig. (7) is also in error.

However, an equation similar to Eq. (7) (i.e. "including" the measured lift coefficient) using acceleration measurements is valid for the real-time upwash correction of measured attack angle regardless of the wing aeroelasticity (i.e. the possible dependence of lift coefficient on airspeed). A Fourier method to estimate the appropriate parameters (the actual response function of the wing) using real-time data in the ABL and compute correct time series of attack angle was presented in section 3, Eqs. (6) and (7) in Kalogiros and Wang (2002b). The processing in frequency space is needed because the response function of the wing is frequency dependent and not a constant over all frequencies. In the case of WSMA a rotational transformation for the ξ angle and the roll angles difference between trike and wing is needed because the upwash direction at the probe is not vertical, in order to separate the upwash in vertical and horizontal components.

Page 1334, line 20, "dynamic flight modes… require infinitely more in-flight data": As pointed in the previous comment the use of the measured lift coefficient (i.e. acceleration measurements) is the only measurement required for a simple real-time (dynamic) correction, which will also result automatically in correct energy of air vertical velocity in the inertial subrange of its spectrum. I note again that the aerodynamic response of wing (i.e. the changes of upwash) to turbulence (traveling air disturbances) is different than its response to forced pitching oscillation. The first is of interest in the case of aircraft turbulence measurements. For better quality measurements the interference of the pilot with control actions should be minimal (i.e. smooth straight flights are preferable).

Page 1337, Appendix A, Eqs. (A5) and (A6): These equations are the approximate equations of Williams and Marcotte (2000) for small attack and sideslip angles. Why not use their exact analytical equations which are valid for larger flow angles, too? This should be more appropriate for a slow moving aircraft like WSMA. I assume also that in Eq. (A7) $p_{q,B}$ is the dynamic pressure p_q used in the rest of the Appendix and in the main paper ($p_{q,B}$ is also used in the main paper). Probably $p_{q,B}$ should replaced by p_q to avoid confusion.

Table and figure legends: Many of them are too long and should be shortened. The details can be given in the text during the presentation of the corresponding tables or the figures.