Answers to comments of referee 2: “amtd-8-1731-2015.pdf”
“Calibration of 3-D wind measurements on a single engine research aircraft”
By C. Mallaun, A. Giez, and R. Baumann

General comment: *This is a useful addition to the body of knowledge of wind measurement on research aircraft, and as such deserves publication after some revision. It covers the bases fairly well with carefully developed procedures, but the readability of the narrative would benefit from use of references to existing material (especially to the extensive discussion in the recently published Airborne Measurements for Environmental Research (Wendisch, M. and Brenguier, eds., 2013)—for example, Eqs. 2 and 3 are really not necessary for the purposes of this paper.*

We want to thank the referee for his / her thorough review of our paper, the positive feedback and the important comments which in parts were definitely not easy to answer. The first reference within our paper (Bange et al., 2013) on line P1733/8 is referring exactly to the appropriate chapter within the above mentioned book. We try to keep the introduction to the wind calculation procedure in Sec. 2 (including Eqs. 2+3) as short as possible in order to help the (unexperienced) reader to understand the following sections. The importance of the laborious calibration procedure presented in the paper has its justification only in the dependencies included in Eqs. 2 and 3 and therefore, we think it is important to have them directly visualized. This defines the common and accepted base for the following discussion in the paper.
In the next section we will answer to special issues commented by the referee and hope that we were able add the missing information and to explain our approach in clear manner where required. The comments of the referee will be in *italic* type followed by our answers.

Issues: References are to page/line numbers.

1. P1736/15 and Fig. 1b: Drift angle is customarily angle from aircraft forward x-coordinate to ground speed vector, not as shown in figure, but as correctly stated later (p1736/20): “The drift angle (δ) is the difference of the true heading (Ψ) – the direction the nose of the aircraft points to relative to the North – and the actual track angle (ATA) – the direction the aircraft moves relative to the Earth fixed CS.” The next sentence is not quite right either (“δ determines the strength of the cross wind component while the wind component along the aircraft results from the difference of the respective ground speed component and the TAS.”) since aircraft xcomponent is not always along the TAS vector.

Thank you for this comment and we will correct the direction of δ in Fig. 1. The second comment is also correct. The idea is to give a qualitative estimation of the major contributions in order to help the reader to understand the main processes of the wind calculation. We will change the phrase to: “… results primarily from the difference of the respective ground speed component and the TAS.”

2. P1735/14-17: Sentence “The sequence…” replace with “Bange et al. (2013) describe the methodology for rotating aircraft to earth coordinate axes”. Then replace “The authors derive…” to “ Lenschow (1986) simplified the wind equations in level flight to be:…” Lenschow, D.H., 1986: Probing the atmospheric boundary layer. American Meteorological Society, 270 pp.

We will follow the suggestion of the referee, but would prefer to use the Lenschow (1989) instead of Bange et al. (2013), because we think that the procedure is better explained in the former. For the second part of the comment compare issue line 131 of referee 1.

3. P1738/25ff: In Eq. (7), the assumption is that the sensitivities (partial derivatives in equation) are independent. I.e. in the turns, beta is the issue and in the straight legs, alpha is the issue. Actually, in the turns, both alpha and beta are involved. So I don't think this procedure makes sense. Also, I don't see how the upwash (or sidewash) effects are accounted for in this procedure. In the turn, the attack angle will increase to hold altitude, increasing the upwash effect. At the very least, the authors should rewrite this section to be far more explicit.

We see that both of the referees had difficulties in following our argumentation and therefore, we will revise this section to be much clearer. It is correct that both, alpha and beta are involved in the wind calculation and that especially during turns alpha and beta are of comparable importance for the calculation of the vertical wind. However, the important point in the presented method is that it is capable to separate the effects of the two (mechanical) offset angles by application of two very robust assumptions.
For the calibration of the alpha offset angle ε_b we assume that the mean vertical wind vanishes when averaged along of a long enough flight track, e.g. a whole flight. This is exactly the same requirement as suggested by Khelif et al. (1999) in section 5 for the offset calculation of alpha, but we offer an analytical solution for the offset calculation. The principle is: as long as there is an uncorrected (constant!) bias in the angle of attack, the mean vertical wind will significantly differ from zero, and by calculating the derivative of the mean vertical wind with respect to the offset angle, we can calculate the required correction to force a zero mean. Please note: we are not speaking here about correcting dynamical effects like the upwash effect, which is handled by calibration of the alpha sensitivity factor by means of specialized flight maneuvers (see section 4.2), but about correcting a pure geometric (mechanical) misalignment between the two coordinate systems of the 5HP and the IRS which can be assumed to be independent on flight state.

The beta offset (η_b) calibration is based on the final goal to eliminate any dependency of any measured parameter on the state of the aircraft after correct calibration. To determine η_b we exploit that no correlation between roll angle Φ and the vertical wind during turns may exist (i.e. $Cov(w, \sin\Phi) = 0$). The idea is: *if* there is a constant offset in the sideslip angle beta, i.e. the boom is tilted either to the left or right, *then* any *rotation* around the x-axis, e.g. during turns, would effectively tilt *up or down* the boom. This vertical tilt would produce an artificial signal in the vertical wind, which is the product of the sine of the roll angle times the beta-offset η_b times the true airspeed. Because the sign of the "wrong" vertical wind changes according to whether it is a left turn or a right turn, the effect of η_b can be discriminated from changes of the upwash, because this does not depend on the direction of the turn. Similar to the alpha correction also here an analytical solution to calculate η_b is offered.

We cannot exclude that a corrupt alpha influences the result of the beta offset, therefore, we repeat the calculation iteratively for at least three times in order to optimize the results. Nevertheless, we do not find significant changes in the results between the different iteration steps. We agree that during turns both attack angle and the upwash are increased, however this will not change the alpha offset when the calibration of the sensitivity factor of alpha is correct beforehand. Probably we should emphasize, that the two methods presented in section 2.2. are to be applied only after the correct calibration of the sensitivity factors of alpha and beta which include the effects of upwash and sidewash as discussed in section 4.2 later in the text. We discuss the disadvantages of calibrating beta during non-steady flight conditions starting with line P1740/5.

4. P1740/1: Why show data from the Falcon20 in Fig. 2? Later (Fig. 9), the Caravan results for εb and ηb are shown. I didn't catch that Fig. 2 is about Falcon20 data right away which lead to some confusion as I tried to reconcile your statements later.

We think that this is an important point and we must avoid any confusion about the aircraft. We show the data from the Falcon20, because the results (which were not published so far) are very helpful to prove the validity and robustness of the method. More than 800 flights have been evaluated with this method and show very consistent results to help the reader to have confidence in the method. We will modify the passage where the Falcon20 is first mentioned in order to avoid any confusion. (e.g. “Note that for statistical reasons we show the results from a different aircraft here, for the new measurement system on Caravan much less flights are available.”)

5. P1740/16ff: The important measurements for this paper are Rosemount 858 air velocity, the IGI/AEROcontrol GPS/IMU system, and the static pressure. The extensive discussion, for example, of humidity (P1743/5ff and Fig. 4) is distracting to the narrative, and consideration might be given to shortening the overall discussion of instrumentation and sensor calibration and accuracy.

We were also aware of this difficulty and already tried to keep it complete as necessary and brief as possible. We will follow the suggestion and revise the section on the basis of this aspect.

6. P1742/3 and Tables 2 and 3: Are the IGI/AEROcontrol accuracies real-time, or after postprocessing? Model IId is indicated but not shown in http://www.igi.eu/aerocontrol.html?file=tl_files/IGI/Brochures/AEROcontrol/AEROcontrol_specs.pdf . I think some discussion should be added about the extraordinary heading accuracy (.01 deg). Were differential GPS antennas on the aircraft used to obtain this level of accuracy? Some mention of airframe and noseboom flexing should be added to indicated how this could degrade the accuracies.

The IGI/AEROcontrol model IId is the one available on the Caravan, but just newer models (e.g. model IIf) are available now. The cited documentation for the quality of the system (Cramer, 2001) is referring to the model IId. We will add more explanation of the system in the revised document. The GPS correction is based on a real-time DPGS correction signal received via a satellite link during flight (OMNISTAR). Therefore, no additional differential GPS antenna is needed.
The airframe of the Caravan is very rigid with the wing struts reinforcing the construction. Therefore, very small contribution of this effect is expected. The influence of the noseboom flexing on the measurement data is discussed in Sec. 5.3 beginning with P1757/10. In the power spectrum Fig. 12 the contribution of this flexing is almost not visible; the contribution of the friction within the pressure tubes is dominating in this example. Both effects must be considered when the high frequency data are used for turbulence evaluation. For the measurement uncertainty of the mean values these artificial fluctuations have minor impact.

7. *P1747/11ff and Eqs. 13-14: K=0.0789 from the Rosemount report is also the potential flow prediction. Rodi and Leon (2012), working with a Rosemount 858 on a somewhat heavier twinengine aircraft, report K to be smaller (about 0.0583), as suggested by their wind tunnel study and theoretical analysis (Traub and Rediniotis, 2003). Would this modify any of this paper’s results if verified on the Caravan? At least some mention of this should be added. Traub, L. and Rediniotis, O.: Analytic prediction of surface pressures over a hemispherecylinder at incidence, J. Aircraft, 40, 645–652, doi:10.2514/2.3168, 2003.*

Assuming a different linear coefficient to estimate the indicated values of the flow angles would lead to a different result in the linear correction coefficient of the aero-dynamical calibration, c.f. P1748/2. In the end, just the combination of K and this empirically derived correction coefficient must be correct, no matter which value of K to use as a starting point. Thus, it would not modify any of the results in a substantial manner.

8. P1748/22ff and Fig. 6: : If K is actually smaller than 0.0789, the magnitude of α_{ind} values would increase, and the slope of the line through the data would be smaller, indicating a larger upwash effect. Could this possibly be the case?

We use the calibration of the manufacturer to calculate a local α_{ind} and correct afterwards for any kind of errors including upwash effects. Therefore, we cannot distinguish between the different contributions. However, our goal – a correct alpha for the complete flight envelope – is fulfilled.

9. P1749/19ff: "orientation of the gust probe is more stable compared to the IRS and gives better reference." I don't understand this. Please explain.

We are sorry for being unclear in this point. We will add the following information to the revised text: "This is due to the fact that the mounting points of the METPOD lead to a fixed position of the gust probe, while the orientation of the reference platform might slightly change from one system integration to the other within the tolerance of the mounting studs on the seat rails in the cabin." Once the system is integrated on the aircraft the orientation of both devices is constant.

10. P1750/21: Asymmetry in pressure deviation with sideslipping: A brief explanation (speculation) about why having the 5HP with static ports on a boom under the left wing causes asymmetry would be useful.

During the sideslips the modification of the airflow due to the aircraft is modulated and with a 5HP mounted under the left wing the probe is possibly shaded by the aircraft fuselage when turning to the left side while it is exposed more directly to the flow when turning to the right side. We find this effect significant for the pressure field as shown in Fig. 7 b). The influence on the beta calibration itself is small referring to the results discussed in Sec. 4.2.3.

11. P1752/23 and Fig. 8a: Again, if K is actually smaller than 0.0789, the magnitude of β_i values would increase, and the slope of the line through the data would be smaller, indicating a larger sidewash effect. Could this possibly be the case?

We calculate: $\beta_{NB} = 1.04 \cdot \beta_i$. Thus, beta indicated is slightly smaller than the real value. This is true, when a streamline of the air along the aircraft is (slightly) bended towards the aircraft x-axis which seems to be plausible. We find a small sidewash effect for this aircraft and think that this is realistic. If the indicated values of beta would be significantly underestimated by the results of the manufacturer, β_i would be bigger than β_{NB} and we do not see an aerodynamic effect capable of this.

12. P1757/16: Three bladed propeller? Would there be propeller frequency components at 180 Hz aliased back into the spectrum (say at 20Hz?) – what frequencies were filtered (Table 3 cites "appropriate filtering").

The filtering of the data is performed by the analog/digital device. The sampling rate of the system is 7812,5 Hz with an antialiasing filter of 2 kHz. The data were compressed using internal filtering to the desired acquisition frequency of 100 Hz (i.e. a filter of 50 Hz must be applied). A rotation of the three bladed propeller with 1800rpm leads to 30 rounds per second and 90 passes of a blade. We recently conducted some high frequency acceleration measurements with a sensor fixed on the seat rail in the rear part of the cabin shown in Fig. R4.

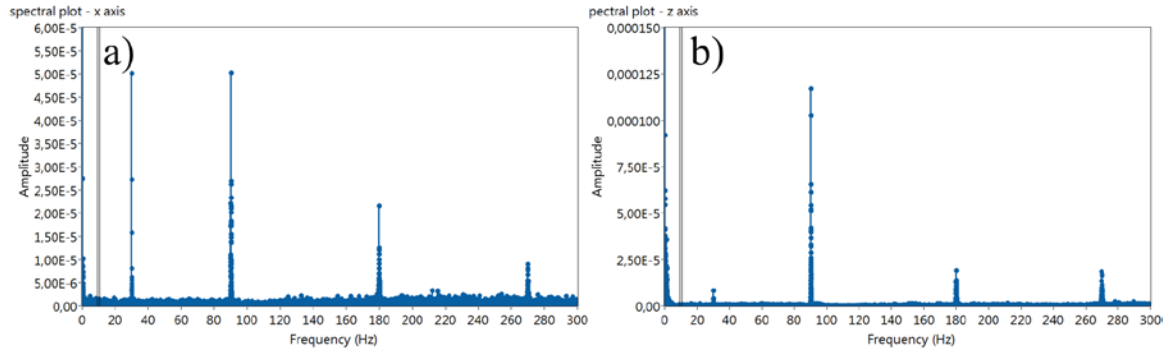

Fig. R4: Results from acceleration measurements within the rear part of the cabin in the Caravan. Plot a) shows the vibrations in the aircraft x-direction while plot b) shows the z-direction.

Within the cabin vibrations with 90 Hz and their harmonics can be observed in all directions and especially in the aircraft x-direction also 30 Hz vibrations are visible. A transmission of these vibrations to the METPOD is unavoidable and can explain the observed peaks in the turbulence spectra. We will include these results in a revised version.

13. P1758/28: No white noise in the raw data. This has to do with the filtering, and also the resolution of the digitization. P1759/3: …white noise added to the "raw" data time series – i.e. differential pressures, total temperature, etc.

We know that there is always a certain level of white noise present and state that no white noise is visible in the data. It signifies that in our case the white noise level of the system is lower than the resolution of the system (16 bit) as listed in Table 3. This is a quality proof of the presented measurement system.

14. P1757/22ff: Error analysis: Injecting white noise into a non-linear set of equations is a known method to determine sensitivity. However, I think interpreting the sigmas as measures of uncertainty (Table 5) is misleading and seriously underestimates the actual errors involved since it does not take into account the distinction between biases and random error, other factors causing bias and noise, nor cross-correlations in the measurements. I recommend this section be deleted, along with P1764/21-P1765/3, and the last sentence in the abstract. Perhaps this analysis can be re-worked and enhanced as the basis for a separate paper.

The "measurement uncertainties" listed in Table 5 represent the increase of white noise which is found in the processed data after injecting artificial white noise into the raw data being used in the calculation. The listed values are calculated as the difference relative to the original (i.e. non-spoiled) data set. They are determined by means of the autocovariance function in order to distinguish between white (noise) and correlated (atmospheric) variance. Therefore, the listed errors represent the variance which is caused exclusively by the propagation and superposition of the injected white noise signals.

For a single data point of a time series the addition of a noise value (offset) will result in a deviation of the processed data when compared to the original value. The deviation being found represents the exact impact of the applied offset (noise amplitude) and does not underestimate the error propagation. For a successive calculation based on a Gaussian distribution of noise input values the processed data will show the exact "translation" of this statistics into a respective error distribution of the calculated units and there is no underestimation of the resulting total error.

For this discussion it is completely irrelevant whether the applied offset represents a systematic bias or a random error. The method simply shows the propagation of any kind of error bandwidth into the processed data. An unknown bias in the sensor data is nothing else than an additional source of uncertainty with a given bandwidth (“error”) around the mean value and can therefore be treated identically to the random noise. Therefore, random error and bias effects can be treated in one single step by using a combined error amplitude. It is important to note that the method does allow for the addition of two independent noise signals to a single time series in order to treat different error contributions separately. However, we don’t see any advantage in doing so.

We also use the method to identify the contributions from different input data sources to the overall error of a processed unit by selectively activating the white noise injection for the different input parameters.

In order to fully understand the error calculation one needs to know the meaning of the mentioned “seed value” in a pseudorandom number algorithm. This number is used to initialize the pseudorandom number generator. Normally, it is recommended to use different seed values in order to generate independent random number series. However, it is possible to reproduce an identical random number series by using the same seed value again. We use this effect in order to treat cross-correlated errors. One example is the static source error, which applies with different signs but same amplitude to the static and dynamic pressure measurements. Therefore, the demonstrated error calculation can and does handle cross correlations.

We will change the following minor issues as suggested.

Minor issues:

15. P1748/16: “…is perfectly…” perhaps should be “…is in stabilized straight and level flight…”.
16. P1748/28: “demonstrated”
17. P1753/2: Perhaps “…too pessimistic…” should be “…too large…”.
18. P1763/22: “4 different…” should be “Four different…”.
19. P1763/23: “…mixing ratio…”

Bibliography:

Bange, J., Esposito, M., Lenschow, D. H., Brown, P. R. A., Dreiling, V., Giez, A., Mahrt, L., Malinowki, S. P., Rodi, A. R., Shaw, R. A., Siebert, H., Smit, H., and Zöger, M.: Measurement of Aircraft State and Thermodynamic and Dynamic Variables, in: Airborne Measurements for Environmental Research, edited by Wendisch, M. and Brenguier, J. L., pp. 7–75, Wiley - VCH, 2013.

Cramer, M.: Performance of IGI AEROcontrol-IId GPS/inertial system., Final report, University of Stuttgart, Institute for Photogrammetry, D-70174 Stuttgart, Geschwister-Scholl-Str. 24 D, http://www.ifp.uni-stuttgart.de/publications/2001/Cramer_Igi01.pdf, 2001.

Khelif, D., Burns, S. P., and Friehe, C. A.: Improved Wind Measurements on Research Aircraft, J. Atmos. Oceanic Technol., 16, 860–875, http://dx.doi.org/10.1175/1520-0426(1999)016<0860: IWMORA>2.0.CO;2, 1999.

Lenschow, D. H.: Probing the atmospheric boundary layer, American Meteorological Society, 1986.

Lenschow, D. H. and Spyers-Duran, P.: Measurement Techniques: air motion sensing, RAF Bulletin 23, NCAR - RAF, NCAR Research Aviation Facility, www.eol.ucar.edu/raf/Bulletins/bulletin23.html, description of air measurement processes at NCAR, 1989.