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Atmospheric Measurement Techniques Discussions

Interactive comment on "Correction of static pressure on a research aircraft in accelerated flight using differential pressure measurements" *by* A. R. Rodi and D. C. Leon

Anonymous Referee #2

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I think that this paper presents a clever and important idea. However, it also appears to me that there are some problems in the approach and in the equations used, as I'll explain below.

The manuscript was initially hard for me to understand, so I'm going to start by explaining the essence of this idea as I understand it. The key result is that it is possible to determine the error in the measurement of static pressure by measuring the pressure at several points on a hemispherical surface carried on a boom ahead of the aircraft. The theoretical predictions for how that pressure should vary over the surface of that hemi-

sphere, involving an unknown parameter f, lead to a set of equations that overconstrains the unknowns (including angle of attack, angle of sideslip, dynamic pressure, f, and the error in static pressure) if enough points are measured. The static pressure enters because one of the pressures (P_1) is measured via a differential measurement $\Delta P_1 = P_1 - P_m$ relative to the static pressure. The static pressure ports on the aircraft supply a reference pressure P_m that may be in error by an amount $P_{err} = P_m - P_\infty$ where P_∞ is the true static pressure. In the case analyzed in this manuscript they have only four measurements and so cannot determine the five unknowns. In order to proceed, they use independent measurements of the error in static pressure, obtained with a trailing-code sensor, to obtain a fully constrained set of equations, and then they use those results to determine an empirical function for the parameter f. With that determination, they have a means of determining the error in static pressure from the four measured quantities without continuing to need independent measurement of that error.

The resulting expression for the error in the measurement of static pressure is then obtained from their Eq. (2) and their empirical formula (3) for f:

$$P_{err} = q\left[1 - \frac{f\,(\tan^2\alpha + \tan^2\beta}{1 + \tan^2\alpha + \tan^2\beta}\right] - (P_1 - P_m)$$

It would have helped me on first reading if they had said clearly that this is the correction developed in the paper, and referenced an equation like this when they discuss the correction, rather than leaving it implicit in their Eq.(2). It was also initially confusing that they say, in the Appendix, p. 3625, that $P_1 - P_\infty = \Delta P_1$ is measured when in fact $P_1 - P_m$ is the measured quantity and ΔP_1 is not a quantity that is measured directly. I think it would also be useful to discuss why this approach works. I think it is because the first equation in (A11) shows that ΔP_1 should be approximately equal to q, so a departure of ΔP_1 from q (beyond the small correction for the flow angles in that equation) can be attributed to an error in the pressure delivered by the static ports.

If q can be determined independently from the other equations, the error in pressure measurement can thus be determined.

Assessment and Recommendation:

The application to conditions where absolute measurement of geometric height was available demonstrates that this approach is effective in reducing errors in the measurement of static pressure, even in the important case of accelerated flight conditions. Because this improvement in the measurement of pressure can have significant benefits by supporting mapping of pressure fields, this is a significant and very useful result and merits publication. I do have some concerns, though, and suggest that the following "major points" be considered by the authors and addressed in a revised manuscript:

Major Concerns:

1. Unexpectedly small value of the parameter f that characterizes sensitivity of the 858 sensor

An aspect of these results that seems problematic to me is related to the sensitivity coefficient as measured by the parameter f. Potential flow predicts a value of f=2.25, while their results are around 1.67 even at the slowest flight speed. They show flow-model and wind-tunnel results that both suggest a substantially higher value of f should apply to their case, but they just present the results without discussion and don't offer any explanation for why these values are so different from what they determine from the flight data. With this variability and uncertainty in f, what is the reason for assuming that the same f applies to all the pressure ports? Indeed, if part of the effect causing f to differ from predicted values is related to airflow and especially to flow distortion, it is unlikely that such distortion is the same in the horizontal and vertical planes, so

f varying with position on the probe seems a distinct possibility. Further concern is raised by the cited study of Traub and Rediniotis (2003), indicating that the factor f will vary with incidence angle; this is in conflict with the equations representing the pressure distribution as presented in the Appendix to the present manuscript.

An analysis of the sensitivity of the equations to f raises further concern about the values determined here. As explained in the Appendix, one can determine the angles α and β without reference to f or P_{err}, so these quantities are not sensitive to f or P_{err}. Once α and β are determined, the first equation in the set (A11) (mostly) determines q and then the fourth equation determines f. My point is that f so determined is constrained primarily by an equation for the difference in pressures from ports displaced in the horizontal direction ($P_1 - P_2$), but the resulting value for f is used for both horizontal and vertical displacement of the ports. The parametric fit (3) then represents f in terms of a pressure difference measured in the vertical. This mixing of sensitivities in different directions seems to call for some justification for using the same value of f for both horizontally and vertically displaced ports.

It would seem straightforward to test if there is a difference in sensitivity for horizontal vs vertical displacement. This can be done easily by, e.g., varying pitch in level flight so as to produce known values for α, then use the second equation in (A11) to relate ΔP_α to $\tan\alpha$ and so (using q determined either from the first equation in A11 or from a separate pitot-tube measurement) to determine the value of f relevant to ports displaced vertically. An analogous test could be used for sideslip.

2. An apparent error in the fundamental equations

I think there is a problem with the solution for $\tan\beta$ given by Eq. A12 in the Appendix. The set of equations (A11), interpreted with P_∞ understood to have a potential error, is a valid starting point, as I was able to verify. However, an error then enters (I think) in use of those equations. If γ is defined as $\gamma = \Delta P_R/\Delta P_\beta$, the first equation in (A12) is equivalent to

$$\tan\beta = \sqrt{2}(1+\gamma) - 1 - 2\gamma \quad (1)$$

However, from the ratio of the 4rd and 3th equations in (A11)

$$\frac{\Delta P_R}{\Delta P_\beta} = \gamma = \frac{1 - 2\tan\beta - \tan^2\beta}{\tan\beta} \quad (2)$$

or

$$\tan^2\beta + (\gamma + 2)\tan\beta = 1 \quad (3)$$

Substitution shows that (1) does not satisfy (3), which can be verified quickly by picking a particular value of γ. For example, for $\gamma = 1/\sqrt{2}$ (1) gives $\tan\beta = 0$ and this does not satisfy (3). I suggest that the solution should be the quadratic-formula solution to (3), which is

$$\tan\beta = -1 - \frac{\gamma}{2} \pm \sqrt{(1+\frac{\gamma}{2})^2 + 1} \quad (4)$$

Because of this incorrect equation, I worry that the entire analysis may be compromised, because for example to find the correct value of f from the first equation in (A11) (using the correction from the trailing cone), one must first solve for β and α. It is a further illustration of the error than the derived equations (A13) and (A14) are also incorrect. (A11) shows that

$$\frac{\Delta P_\alpha}{\Delta P_R} = \frac{\tan\alpha}{1 - 2\tan\beta - \tan^2\beta}$$

so, because the limiting value of $\tan\beta$ as $\Delta P_\beta \to 0$ must also be 0, (A13) should be

$$\lim_{\Delta P_\beta \to 0}(\tan\alpha) = \frac{\Delta P_\alpha}{\Delta P_R}$$

Despite this problem, the manuscript presents convincing evidence that the correction scheme is effective in reducing errors in the measurement of static pressure, so there is something about this I don't understand. Perhaps there is a typographic error or (A12) is not used in the analysis, because the angles determined from the gust-sensing system would be substantially in error if (A12) were used. I suspect that the mathematical approach must be correct despite this apparently major error, and that the analysis has been performed correctly or the results would not be so good.

3. Organization and emphasis

While I feel less strongly about the need to address this third point than about the preceding points, I did find the presentation confusing and felt it took longer to understand the paper than was necessary. The first distracting aspect was the emphasis in the title and in the start of the abstract on the acceleration effects. I think the core of the results is a method for correcting measurements of static pressure, and application of this correction to cases with accelerations is important and a good test of the method, but highlighting this slowed my realization of what was really being done. In addition, equations (A11) and a discussion of what is measured are needed to understand what is being done, and it's not enough to trust that answers will emerge from the equations if you don't know, for example, that one of the basic measurements is the difference between the pressure at the forward port and the static ports and thus is affected by errors in the static ports. So putting the fact that $\Delta P_1$is measured in the Appendix, and mis-stating how it is measured there, just impeded my understanding of what was being done.

My suggestions (offered only for the author's consideration, and perhaps not essential because I think I eventually understood this work) are these:

- Consider introducing the topic in the abstract and in the title with less emphasis on the GNSS/IMU measurements and more on the development of a prediction of static-pressure error, and introduce the GNSS/IMU data later to show that the prediction is valid even in accelerated flight.
- Move (A11) and a discussion of what is measured into the text. The development leading to (A11) can stay in the Appendix, but I think a reader needs these equations along with a little more discussion of how they are solved to obtain the estimated error in static pressure. Otherwise, the sense is that results just appear from the equations, and that doesn't convey any sense of what is being done.
- Give the explicit equation for the pressure correction, perhaps in the form suggested at the top of this review. That helped me understand that the difference between q and $(P_1 - P_m)$ should be the correction and that, if q can be determined via solution of the equations, it will be possible to estimate the measurement error in static pressure.

Other minor comments:

1. The manuscript still needs editing to fix awkward sentences, incomplete sentences, and frequent dropped words. A few examples:
   (a) p. 3615 sentence beginning line 15
   (b) p. 3619 section title
   (c) p. 3620 sentence beginning on line 4
   (d) p. 3621 line 5, "excessively altitude excursions"?

   (e) p. 3621 line 11
   (f) (many more)
2. Figure 2: The caption for (b) and (c) says that the units are hPa, but I think they are Pa?
3. Figure 12: Units (obviously Pa) are missing.
4. Figure 13 and 14: The caption discusses three curves for pressure, but only two are shown; the curve for uncorrected static pressure is not shown.
5. $\Delta P_\alpha$is used on p. 3617, line 3, but only defined in the Appendix. Some reference to what this is or where to find the definition would be useful for someone trying to read this without reading the Appendix first.
6. p. 3621 lines 1-2: This says that "corrections were less than 5 Pa ... as shown in Fig. 8d" but the units for Fig. 8d are m and I don't see how it shows the claimed limits to the corrections.
7. p. 3621 lines 12-14: The claimed errors of 100 Pa and 275 Pa are not supported by the plot, which shows a *range* from minimum to maximum about this large but no individual residuals as large as claimed.

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