Response to Anonymous Reviewer #1:

This was a very helpful review and we appreciate the care, attention, and perspective of the reviewer. Several shortcomings identified in this review need to be addressed, and we are grateful that they have been called to our attention. If we are permitted to revise this manuscript, we will accept almost every recommendation in this review. The following is our response to the specific comments:

1. The reviewer suggests that we should provide a little more background information describing the instrument, possibly with a figure, even though the instrument description has been published in detail elsewhere. We think this is a good suggestion and propose a small expansion of Section 2 to help make this paper more self-contained. The paper is already fairly long and reviewer suggestions will require other expansion, so we will only make a relatively small addition, but an additional small figure should be helpful.

2. We agree fully with this suggestion and are embarrassed that it was necessary, because we had intended to be consistent with standard terminology. We understand what is required and will make the recommended changes, with thanks to the reviewer.

3. As for point #2, the reviewer's advice is correct. Page 2590 line 28 should read, "The peak Doppler frequency can be measured *with a standard uncertainty that,* ~~to an accuracy that,~~ converted to airspeed, is ~~better~~ less than $0.1\mathrm{m\,s^{-1}}$. We also propose to change "accuracy estimate" at the top of the next page to "estimated standard uncertainty". Page 2590 line 7 will be changed to read "attitude angles and aircraft velocity with respective standard uncertainties of about 1 mrad and about 0.1 $\mathrm{m\,s^{-1}}$, after incorporation of measurements from Global Positioning System (GPS) receivers." Yes, the IRS uncertainty is comparable to the LAMS uncertainty; both enter the calculation of the wind relative to the Earth.

4. "Section 2, p2590, line 27, 'negligible error' - can this be quantified explicitly?" The error attributed to variation in the laser wavelength is equivalent to ± 0.01 $\mathrm{mm\,s^{-1}}$ for wind measurements that are typically about 200 $\mathrm{m\,s^{-1}}$. In comparison to the 50 $\mathrm{mm\,s^{-1}}$ precision of the measurement this error makes a negligible contribution to precision or uncertainty.

5. "Section 2, p2591, line 3, 'algorithm that defines the peak' - are there details or a reference?" The details are included in the reference cited for the instrument (Spuler et al., 2011, Applied Optics), in the last paragraph of section 3. We will add a specific reference to this section.

6. Yes, temperature is required to determine the calibration of the pressure system, but the point intended here is that, once calibrated, the measurements from the LAMS can be used to determine temperature without further reference to temperature sensors or even to their presence on the aircraft. Perhaps wording similar to the following final sentence in the abstract can make this clearer: "Finally, it is shown that, although the initial calibration of the measured static and dynamic pressure requires a measured temperature, once calibrated those measured pressures and the measurement of airspeed from LAMS provide a new measurement

of temperature that does not depend on any other temperature sensor." The sensitivity to pressure and temperature is implicit in equation (2): The fractional error in p is the same as the fractional error in q, but q is smaller so the error is less, and because the error in p is the negative of the error in q (by the assumption in this paper), the calculations using (3) and (4) account for this covariance of p and q. For temperature, differentiation of (2) leads to fractional uncertainty in q equal to about 0.5 times the fractional uncertainty in absolute temperature T, for typical flight conditions, so if T=213 K then an uncertainty in temperature of 1 K leads to a fractional uncertainty in q of about 0.5% or, for $q = 60\,\mathrm{hPa}$, about 0.3 hPa. Because the calibration of the temperature sensors is checked in this paper to a standard uncertainty of 0.3 °C, this uncertainty in temperature leads to an uncertainty in calibrated dynamic pressure of about 0.1 hPa, or an uncertainty smaller than the overall standard uncertainty of 0.3 hPa claimed for the calibration of pressures. In response to the reviewer's comment, it appears appropriate to include a summary of this in Section 3.1. The "tentative assumption" reference on page 2594 line 7 is verified later in the paper via the calibration of the temperature measurements with the required uncertainty. In answer to the final question: Yes, one could use the approach developed here to infer temperature from any measurements of dynamic and static pressure and the airspeed provided by the LAMS, provided that those measurements are calibrated well, but the sensitivity of the inferred temperature to errors in the pressure measurements would make that of limited utility unless the pressure-sensing instruments were calibrated with uncertainty comparable to that obtained in this paper. If, for example, a trailing-cone system were used for the calibration, the resulting calibrated pressures could be used to deduce temperature from the LAMS-measured airspeed.

7. The flow angle for the yaw maneuvers was that determined from the radome-based gust sensing system of the aircraft, which deduces the sideslip angle from horizontally separated pressure ports on the radome, located near the pitot tube. Flow distortion may cause the flow angle at the pitot tube to differ from that measured; we should add a comment to that effect and a reference to how this angle is measured.

8. The inertial navigation units used in the cabin and in the pod with the LAMS independently undergo an alignment that determines their attitude angles in Earth-referenced coordinates before the start of flight. These measurements are used to find the angle between the LAMS laser and the fuselage longitudinal axis. The conventional wind-sensing system on the aircraft measures the angle of the relative wind relative to the longitudinal axis of the aircraft, so we can correct the LAMS-measured line-of-sight airspeed to deduce the magnitude of the relative wind. Drift angles of these inertial systems are typically $<1^\circ$ and in the case of the LAMS IRU are corrected by reference to GPS measurements via incorporated Kalman filters. Because the typical flow angles between the LAMS beam and the relative wind were around 1° (as a result of installing the LAMS with an appropriate offset from the longitudinal axis of the aircraft), these corrections were typically $0.03\,\mathrm{m\,s^{-1}}$ and so negligible in normal flight conditions. We propose to add a comment of explanation to the manuscript.

9. The model used for the fit does not need to be optimal or correct to support this claim, because it arises only from the standard deviation between the values deduced from LAMS and

those provided by the fit. If it is assumed that the values provided by LAMS are accurate, this is a direct measure of how well the assumed formula reproduces those measurements. That is what we intended by the phrase "(from this source alone)" at the end of that paragraph. We will attempt to reword this to help clarify that point. Perhaps a better test of the validity of the fit is that, when it is applied to measurements other than those used to determine the fit but where LAMS is operating and able to provide a reference, the variance remains about the same. We argue that the fit is not a significant source of uncertainty in comparison to the variability that arises from using different sample volumes to determine the two values of dynamic pressure, one deduced from the LAMS airspeed at a location ahead of the aircraft and displaced laterally from the nose, the other measured at a location on the fuselage near the nose.

10. In this case, "error" (p. 2602 line 2) appears to us to be the right term. We use measurements of a quantity that should be the same in reverse-heading maneuvers (subject to stationarity of the atmosphere) and find this offset, which would then characterize the error in the measurement. (Other terminology in this paragraph needs revision as noted in point #1.)

11. The complete answer to this point is complicated, and we intended this only as a simple check that the values we have obtained are reasonable. The measure used for RVSM certification is much more restrictive than a standard error, and there are detailed procedures specifying required testing and maintence of the system, but we are reluctant to add to this section because it does not seem important to the arguments in the paper and we worry about possibly misrepresenting the complex regulation.

12. Thanks; the figure caption will be corrected.

13. The sample frequency for the static and dynamic pressures is 25 Hz after reduction and filtering from higher-rate samples. However, the point being raised is that the difference between samples applies to instantaneous measurements, not to 1-s averages, and 1-s averages are being used here. We will revise the argument appropriately.

Response to Anonymous Reviewer #2:

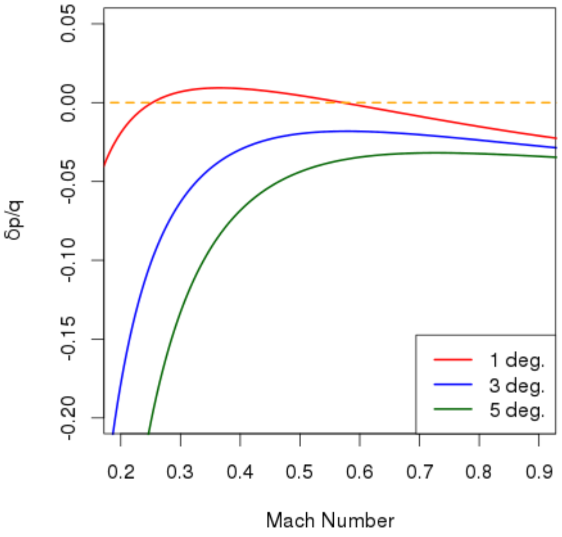
This review also is very helpful and contains important comments that we would want to address in a revision. We thank the reviewer for considering the paper in such detail and for thoughtful suggestions that will improve the paper. Here are responses to the reviewer's specific numbered comments:

1. The reviewer suggest including more discussion of the issues related to pressure measurement and especially the dependence of these results on the location of the pressure ports. We agree with this suggestion and with the comments offered, and we propose to address this comment by including information on the pressure-port locations (which are non-standard for this aircraft type and therefore important to document). The suggested reference is a

useful one to add to the manuscript in support of the argument for accurate measurement of total pressure. This point could be addressed by adding a paragraph at the end of section 3.2, perhaps like the following:

"The results obtained in this way are dependent on the specific locations of the pressure ports providing the static source. On these research aircraft, to avoid interference with the standard ports used by the avionics systems, separate ports have been installed to provide this static source. The locations on the GV are at fuselage station 247.0 and water line 80.2, symmetrically on the starboard and port sides. The primary pitot tube on the GV is located at fuselage station 54.0 and buttline -19.0 (on the port side). The correction procedure developed in section 3.5 depends on measurements of flow angle as determined by pressure measurements from the radome gust system (with pressure ports on the nose of the aircraft), so the pitot tube needs to be relatively close to the nose in order for those measurements to provide accurate characterization of flow conditions in turbulent conditions. Other locations for the static sources will have different errors and different dependence on flow characteristics. This approach to calibration, however, should work with any pitot source that is insensitive to flow angles and is installed outside the boundary layer that is present next to the fuselage."

2. The suggested plot would be a good addition to the paper, and we propose to add such a plot. The normalization to p in (8) and (10) was chosen because it provided a better fit than normalization to q. The phrase "better" on p. 2599 line 17, however, refers to a different problem, the danger of extrapolation of results based on high-order fits to values outside the range where those fits were determined; "better" is just a qualitative reference to the advantage of avoiding such extrapolation. The suggested plot is shown here, for Equation (9) in the manuscript:



Adding this to the paper will help show the variations and magnitude of the correction that has been determined. We are also grateful for the reference to NASA Reference Publication 1046; this is a good reference that we can use at several points in the manuscript for equations, insensitivity of total pressure to flow angles, and typical magnitudes of correction factors.

3. Page 2595, section 3.3 (iii), discusses the corrections made for flow angles. The airspeed as used in the referenced points in the manuscript is that given by the last line in that paragraph. It could be helpful to add to that section that the IRU mounted with the LAMS aligns during preflight, as does the IRU in the fuselage, so (with GPS updating via a Kalman filter) they provide a reference for the relative orientation of the two axes, and this correction is made when using the measurements provided in this manuscript.

4. This is correct; the correction referred to he is that made to the LAMS measurement, as discussed on p. 2595 section 3.3 (iii). The point is that, even when there is flow at an angle relative to the pitot tube, for small angles it still delivers the correct total pressure, but the LAMS in that case may measure a reduced airspeed dependent on the flow angle, and that correction needs to be made. We will reword this sentence to clarify this.

5. A comment along the lines of the following, added to line 21 p. 2596, should help address this point:
   "The recovery factor used for the GV in this study (specified later in section 5.2) was determined by fitting to data in Goodrich Technical Report 5755, and the measurements in that report span a range in a_T that corresponds to a standard uncertainty of about 0.007 in α_T. For a representative airspeed of 220 $\mathrm{m\,s^{-1}}$ this corresponds to an uncertainty in temperature of about 0.2°C. Calibration of the temperature measurement, which includes dependence on the recovery factor, is presented in section 5.2, where it is argued that the temperature is constrained by that calibration within about 0.3°C of that measured, so using v as measured by LAMS keeps the standard uncertainty introduced by T within this limit."

6. This is a valid point, also raised by reviewer #1. The argument would apply to instantaneous measurements, not to measurements averaged over 1 s. We will revise the argument to apply to measurements averaged over 1 s. When high-rate measurements are used, the correlation between airspeeds is a maximum if the LAMS measurement is shifted by about 0.1 s.

7. The scatter of the points about the fit line is not a measure of how constrained the fit is, just as the standard deviation in a mean value can be less than that of the individual measurements. The fit is determined with low uncertainty, so the predictions from the fit have small uncertainty even though individual measurements entering the fit have larger scatter about the fit. A fit better than the scatter in the measurements can occur if there is an additional source of variability between the two signals used in the calibration, if that variability arises not from the measurement being calibrated but from the one used as a reference, provided that the unrepresented variability is not biased. We are not sure how to quantify this beyond pointing out that the fit result is highly constrained and itself introduces little uncertainty when it is used.

8. [Some characterization of the dependence of the correction on Mach number is now included in the figure provided in response to point #2. It is true that there are effects from accelerated flow, but the calibration developed here is qualified as applying to straight-and-level flight. Note, however, that the variation in d-value, except for the large excursions when accelerated, is about 5 m, corresponding to a pressure difference of about 0.3 hPa, which is the tolerance expected for the calibration. It is correct that the red line represents a horizontal pressure gradient as discussed in section 5.2.

9. To span the range of flight conditions needed for the calibration, it was necessary to use flights at varying altitude and airspeeds, including some in the boundary layer. That was the only way to obtain the needed range in Mach number and angle-of-attack. As a consequence, the full set of measurements includes many from turbulent regions, and they contribute the most to the variance in the fit. We acknowledge that this discussion needs revision, as noted with point #6 above.

10. The climbs and descents were at fixed and limited rates for the experiment used, to permit adequate response of the chemistry instrumentation onboard, so effects of lags in temperature sensors should cancel. We don't see how atmospheric variability would be expected to introduce a bias, unless there is systematic structure, and we have attempted to correct for that using the approach of Section 5.2. This was less important in the case of the C-130 flights of Section 5.1 because the flights were all local and returned to the takeoff airport, unlike the flights of 5.2. Perhaps it should be mentioned that the correction in 5.2 based on d-value measurements was not made in section 5.1 so changing atmospheric structure between takeoff and landing could introduce a bias.

11. The climb restriction is introduced because, if the climb is too slow there is too little altitude change between measurements and the uncertainty in the pressure measurement makes the measurement of little use; this is most important for cases of level flight, when the measurements just introduce noise, and that was the intent of the restriction. The measurements with high climb rate were excluded because they introduced problems in the fits, causing large changes in the best-fit coefficients. While the normal climbs and descents in the project used for this analysis were limited in rate to fall below this limit, occasionally air traffic restrictions or approaches to airports dictated more rapid descents, outside the normal descent rate used for research, so it was desirable to exclude these. Here also, a short explanation can be added to the manuscript to clarify.

12. The recovery factor would be clarified if we add the comment suggested in our response to the reviewer's point #5.

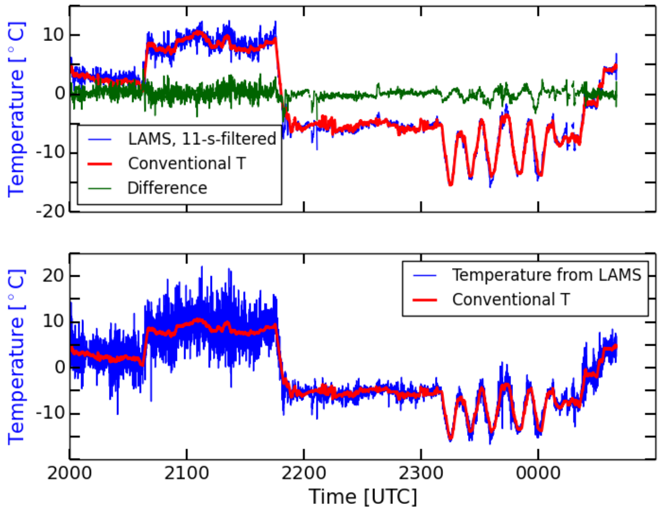
13. The main problem that introduced erroneous measurements into the data set was that sometimes flight in ice or supercooled water cloud would cause the ports on the radome to become unresponsive and the angle-of-attack measurement, used in the representation of the pressure-correction factor, was not available. These events were evident upon review of the data, and it was important to remove them. There were some other rare periods where the

data-recording system malfunctioned; these were also easy to identify upon review of plots of the measured quantities. Excluding these was important to obtaining the results presented in this manuscript, but it doesn't seem to us necessary to provide a full description of the data-quality procedures employed (which are admittedly subjective).

14. This point also was made by Reviewer #1, and needs clarification. Please see our response to point #6 of Reviewer #1. We want to argue that, although temperature measurement is needed to determine the calibration, once done the pressure measurements and the airspeed from LAMS can be used to determine the temperature without further need of that temperature sensor. This could be valuable, for example, for identifying malfunctions of the temperature measurement, or it could even be used without the temperature sensor being present or operational.

15. The variance in the direct measurement of temperature, the red lines in the figure, should indeed provide a good representation of the true variability in temperature. The deduction of temperature from LAMS, however, requires use of measurements separated in space: p from the static buttons, p_t from the pitot tube, and v from LAMS. Any failure of these to be corresponding measurements representing the same air motion will lead to additional variability in the temperature deduced from LAMS. Spectral analyis indeed shows a loss of coherence between the LAMS-measured velocity and the velocity measured by the pitot-tube with static buttons at high rate. In regions where there is turbulence the spectra both look reasonable without white noise, but in non-turbulent regions the spectrum is indeed noisy at the maximum rate simply because the resolution limit of the conventional measuring system is reached. Error analysis based on (23) for typical conditions (e.g., $v = 200\,\mathrm{m/s}$, $p = 400\,\mathrm{hPa}$, and $q = 140\,\mathrm{hPa}$) shows that the temperature deduced from the measurements via (23) varies by 2.6, 1.9, and 2.6°C for respective changes in $\{v$, q, and $\delta q = -\delta p\}$ of 1 m/s, 1 hPa, and simultaneous ±1 hPa (as would occur for an error in the applied correction term to the pressures). The measured temperature is therefore quite sensitive to the input values, and small fluctuations in the measured quantities result in large changes in the deduced temperature. In the boundary-layer segment plotted in Fig. 5 for times around 21:00, fluctuations in measured airspeed have a standard deviation of about 1.9 m/s even over short measurement periods, so the turbulence here is fairly intense. Above the boundary layer, starting at about 21:45, the turbulence is much reduced and so is the variability in the temperature deduced from LAMS. The variability is again significantly reduced for repeated climbs and descents beginning at about 23:10. This pattern indicates that the variability in the measurement from LAMS is associated with the turbulence level and, because similar variability does not appear in the conventional measurement of temperature, this variability must arise from a mis-match among the sensors contributing to the temperature as in (23). Similar patterns could result from mis-match of the signals caused by timing errors, but the time sequences have been shifted in exploratory studies and those confirm that the lags being used here are appropriate. The explanation offered in the manuscript indeed needs some modification as admitted above in our response to points #6 and #9, but the association with turbulent fluctuations seems clear.

16. The revised figure is shown below, with the difference shown as (conventional minus LAMS). The main feature it seems to reveal is a bias in climbs and descents, as would result from a lag in the conventional temperature measurement. This probe is not fast response but an anti-iced version that indeed has response time more than 1 s, so this is likely the source of this feature in the plot. There is also an offset between measurements between 0–5°C and 5–10°C; this is consistent with the calibration adjustment discussed in Section 5.1, so is best attributed to a small error in the conventional measurement.



17. The measurement on both aircraft is made using a pitot tube (delivering total pressure) and static buttons (delivering ambient pressure), but the transducers providing the measurement are connected so that an absolute pressure transducer is connected to the static button source and a differential pressure sensor is connected between the static button source and the pitot tube. The measured quantities therefore correspond to ambient and dynamic pressure. This was explained at the beginning of section 3.2; the information to be added in response to the reviewer's point #1 should clarify the configuration.

Correction 1): Thanks for catching this error, which we acknowledge. The calculations were performed correctly.

Response to Reviewer #3:

We also thank Reviewer #3 for a very careful and helpful review. We are particularly grateful for the recommendation to eliminate the Appendix and use standard references. This has caught an embarrassing error. Where we simply say "Agree" in the following response, we agree with the suggested change. The suggested wording changes are consistently improvements, and we thank the reviewer for those suggestions.

- Page 2587, line 16: Agree

- Page 2587, line 19: Agree; the tube is trailed, not the transducer.

- Page 2589, line 12: Agree

- Page 2590, line 2: The answer is, perhaps not. An aircraft like the GV is pressurized and certified, and passing anything through the pressure vessel is a major certification issue. During development of the aircraft, we attempted a trailing-cone calibration, but concluded after the flight that the measurements were influenced by the cone not being trailed far enough behind the aircraft. The trailing-cone measurements did not agree with either the certified pressure system that is part of the avionics for the flight management system of the aircraft or with the calibration obtained here. Once developed, provided appropriate fibres can be passed through the wings, the LAMS can be deployed on any aircraft that has an appropriate pod for mounting, which is a standard PMS-style canister available on many research aircraft.

- Page 2592, Discussion of Fig 2: We are reluctant to make this addition because we don't think that the previously used calibration had a good justification. We just wanted to make the point that previous measurements were not as much in error as indicated by this figure. We are in the process of reprocessing old projects to use this new calibration.

- Page 2592, line 13: We acknowledge this; changing "most" to "many" would be appropriate.

- Page 2593, line 5: Agree; the redundancy should be eliminated.

- Page 2595, line 10: Agree.

- Page 2595, line 25: The calculations in this paper are not done iteratively, but the fractional change in q is small and the correction for the angle itself is also very small. For example, flow angles departing by 1° from the LAMS centerline, the common range because the LAMS is tilted to match the normal angle-of-attack, affect the measured v by only 0.015%, and changing q by 3 out of 100 hPa (a typical correction) changes this value by 3% (because the flow angles are linear functions of $1/q$), so the effect of the correction to q on the calculated v is only 0.001 m/s. We have neglected this in comparison to the assumed standard uncertainty in v_l of 0.1 m/s.

- Page 2596, line 12: Perhaps a still better rewording is "typically contributes a larger ..."

- Page 2596, line 18, Eq 7: The alignment of the temperature probe shouldn't matter very much because the housing is designed to avoid sensitivity to small flow angles, and our studies of the effect of the recovery factor indicate it is not a function of angle of attack. However, the equation should use $v = v_l/\cos(\theta)$ as defined in section 3.3 (iii), to use the best estimate of true airspeed from LAMS. This will not have a significant effect on any results because the change is so small, but the equation should be corrected. The difference between the conventional measurement of true airspeed prior to this calibration and the LAMS-measured true airspeed is much larger than the difference introduced by flow angles. We thank the reviewer for pointing to this and suggesting that there is something wrong with (7).

- Page 2597, "Fits to the corrections" section: No, this shift wasn't made. We did explore with parts of the data if it mattered to shift the LAMS measurements slightly, but found almost no difference and none that could affect the fit procedure. To a large extent, any small fluctuations between 1-s values determined with and without shifting should not have a bias because turbulent fluctuations or other symmetrical fluctuations would contribute a randomly varying error rather than a bias.

- Page 2600, Eq 10: We don't have an explanation other than speculation that the GV, highly streamlined and flown at high speed, seldom encounters significant sideslip angles. This difference was very evident in the fits for the two aircraft, with significant statistical significance for the C-130.

- Page 2600, line 5: The correction is much larger than 0.3 hPa; that is an estimate of the uncertainty in the correction. Fig. 1 shows that the correction is several hPa. The 2004 calibration with a trailing cone for GOTEX no longer applies because significant changes have been made in components including pressure transducers and even the configuration of the radome, so it is not meaningful to compare to that calibration.

- Page 2601, line 15, Eq. 11: Yes, but these corrections are insignificant for the purposes of this section. Technically v_t needs to be the relative-wind component along the longitudinal axis of the aircraft but projected to a horizontal plane, to be comparable to the ground-speed component. The angles involved are therefore $(\phi - \alpha)$ (where ϕ is pitch and α angle-of-attack) and sideslip angle. During these straight-and-level flight segments in relatively non-turbulent conditions, both were only small fractions of a degree, so no correction was applied.

- Page 2601 and Table 1 on page 2621: If there is a bias in the wind measurement, it would always appear with the same sign in each pair of legs in Table 1. If for example the measured true airspeed is too low in the upwind leg, the measured longitudinal wind component will be too low, and it will still be too low in the downwind leg. The sum of these measurements of the resulting wind component should differ from zero by twice the bias in the true-airspeed measurement. Except for atmospheric fluctuations in the measurement conditions, the mean of the differences should thus be an estimate of twice the bias. With algebraic averaging, the estimate of the bias improves with sample size; it would not with averaging of absolute values. For example, if the variance in the pair-by-pair measurements is dominated by natural variability, it would just remain the same with averaging of absolute values.

- Page 2602, line 26: Agree.

- Page 2605, line 20: The climbs were repeated along the same track for half of the flight leg and then the course was reversed for the remainder.

- Page 2606, line 15: The nature of the error was an erroneous bath calibration of the temperature sensor, caused we think by inadequate immersion of the sensor. Reprocessing is underway for past projects, with incorporate of the pressure calibration also for projects where it is appropriate.

- Page 2615, line 13: Agree
- Page 2615, line 14: Agree
- Page 2615, Eqs. A3 and A4, and other comments related to Appendix A: We have to admit that the reviewer is correct, and we propose to omit Appendix 2 entirely and substitute the Khelif et al. moisture expression in the analysis. This will likely result in some small changes in the results, but reprocessing to obtain the fits and other results in this manuscript will be necessary. We thank the reviewer for this correction.