Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-27-RC2, 2019

Atmospheric Measurement Techniques Discussions
Open Access

Interactive comment on "Flow-induced errors in airborne in-situ measurements of aerosols and clouds" *by* A. Spanu et al.

Anonymous Referee #2

Received and published: 6 June 2019

General comments

The paper takes simulations of the airflow around the wing and canister mount of the DLR Falcon aircraft (the canister mount being the location of the aircraft's cloud microphysics instruments) and uses these to determine biases and corrections upon cloud microphysics measurements. It also examines how this airflow can induce droplet deformation and breakup. A particular aspect of this work is its use of compressible flow in the simulations which allow application to airspeeds relevant to the faster speeds flown by the Falcon.

In general the work is entirely relevant to AMT and very worthwhile. Significant biases can exist in aircraft cloud microphysics measurements and this addresses one of those sources. In particular the work regarding biases in concentration due to airflow are

Printer-friendly version

Discussion paper

incredibly useful for the community. There are two overarching limitations though, that I feel the authors should address, that I will mention here in the general comments. I will add more detail in the specific comments section.

The first of these limitations is that the simulations do not include the aircraft fuselage. There are three basic elements that could distort the flow along a particle trajectory before measurement. These are the instrument itself (represented in this work by a general instrument canister), the aircraft wing and the aircraft fuselage. The authors have chosen to include two of these three items in the simulations. They need to justify why the instrument and the wing are important, but the fuselage is not. I am not claiming that the fuselage is important, as I cannot say for certain that it is or it is not, but the authors must justify its exclusion.

The second of the limitations is the discussion around image deformation and drop deformation and breakup. Figure 16, showing the breakup diameter for various airspeeds is a very useful plot as it shows the limits of our measurements. However the discussion of droplet deformation seems to be less rigorous than the work regarding flow induced biases and given the other mechanisms which may induce IMAGE distortion, it is difficult to be certain that the distorted images are definitely the result of distorted droplets in the manner described here. Some of the theory, observations and discussions do not seem consistent. I would suggest one of two options. Either this section needs rewriting, being much more careful about consistency and ensuring it is very clear what is discussion and what is conclusion, or alternatively this section could be significantly reduced in size or removed – the section discussing efficiencies is interesting enough to stand alone. As a final general recommendation, although the English in the paper is mostly very good (and much better than my foreign language skill would permit me to write in my non-native tongue), there are times when it is obvious that the paper is written by a non-native English speaker and the wording is difficult to understand. I would therefore recommend that the authors have the final version proof read by a native English speaker.

Overall, the paper covers important aspects of the cloud microphysics measurement system and I absolutely recommend publication with the changes outlined in this review.

Specific Comments

P2L14 Explain why the temperature and pressure further affect the aerosol and cloud measurements – what mechanism are you referring to.

P2L18. Are droplets appearing deformed because they are deformed due to aerodynamic forces or are they appearing deformed because of a bias with the instrument system (off axis flow, incorrectly measured particle speed).

P2L32 compressible flow is not a hypothesis

P3L2 and throughout aircraft is probably a better word than plane or airplane in technical writing.

P4L24 SID-2 is an open path OPC, not an OAP. Note also that SID-3, despite taking images, is not and OAP either as it uses a 2D CCD, not a line of photodiodes.

P5L15 Insert the word measured before "dynamic pressure"

P5L23 Is the overestimation of airspeed an overestimation of PAS or TAS. Be specific.

P6L4 Have you checked the conservation of total pressure at the two pitot tubes? This would give a good check that their calibrations are not introducing bias.

P6L19 The authors have felt it necessary to make the edge length 10 times the canister length to avoid biases from the boundary. Although I appreciate that this was no-doubt chosen because it was a round number, that was at least as large (but probably larger) than necessary, I would guess that within this domain one may find either the aircraft fuselage itself or air which had been modified by flow around the fuselage. It may well be that this is not the case, or that the flow distortion caused by the fuselage does not impact the particle paths, but the authors should show that this is the case or they should provide some limits on the potential effects of the fuselage. They should also describe the effect of the wing itself, so that it is clear to the reader that both the wing and the instrument canister have an impact upon the flow. This will certainly feed into decisions made by future studies into flow effects on microphysics measurements.

P8L14 It would be nice to highlight the test case on all plots, either by circling the data point or putting the line in bold or some other method.

P8L21 replace probe and free stream with local and free stream

P8L28 it would be good to see the incompressible solution on the charts for comparison.

P9L1 At first reading I thought the 1% error in ps was causing a 23% error in U. It may be worth rewriting this sentence to ensure other readers don't make the same mistake.

P9L2 How much do we really care about temperature? Later in the paper the authors refer to the fact that there is a compression of air and a bias proportional to density which is in turn proportional to temperature. However, the temperature increase is a maximum of 3%. The authors also mention that the temperature is measured "round the back" of the probe so may not be that relevant to the sample volume anyway. The authors should simply consider if the bias caused by temperature increase is worth considering, given the other uncertainties, and if it is they should explain at this point why it is of interest.

P9L11 "Well represent" is an ambiguous term. The points from the simulations on figure 6c do not show the upside down U shape that the measurements show. The authors should state some measure of the actual discrepancy and why they feel that this is sufficient.

P9L14 What does "installed at the back" mean? Give some better description of the location of the temperature sensor and if it is not close to the sample volume then describe the expected error due to the position and whether this is sufficient – see above comment re temperature in general.

P9L31 In what way is the stokes number modified and why?

P9l33 Equation(4) which velocity is used here for U? See later comments regarding stokes number.

P10 It is very impressive how well the data fit on the sigmoid curves on fig 8. This clearly forms an excellent correction factor. However a few items may aid the reader in understanding the analysis that is occurring here. Firstly it would be good to indicate that the fit lines used are from equation (6). Secondly it should be noted that α is of the order 1 and contributes negligibly to the parameters x0 and k0, so should probably be removed. Thirdly the authors should note that eˆ(-k0(log(stk)-x0) can be rearranged to b*stkˆa which is a much simpler form. Also if they remove the dependence of k0 and x0 upon α, then a and b simply become fitting constants. In fact b=10ˆ(x0*log(eˆk0)) and a=-log(eˆk0). Which makes b approximately -1. Fourthly it may be useful for the authors to rearrange the form slightly as this may help the points all converge onto 1 line. They should note the classic inlet efficiency equation from Belyaev & Levin doi:10.1016/0021-8502(74)90130-X, equation (5) Efficiency = 1+(u0/u-1)*sigmoid_function_of_stk_only Where u0 is the far field velocity and u is the velocity at the inlet. This basically states that the deviation from unity efficiency is proportional to u0/u. This form may be useful to the authors, perhaps with u0/u replaced by α. Fifthly, as described previously, the authors should consider whether it is really worth including temperature in this analysis as it is a relatively small effect and temperature may not be well measured by the probe.

P11L14 Do the authors actually mean mobility in the sense that it is used in aerosol science, e.g. for a scanning mobility particle sizer instrument? If not then change this word.

P11L26 The word positional error seems like an odd choice. Perhaps the authors mean distortion errors or something similar.

P11L27 did the authors mean 100 um? 10 um particles seem significantly affected by the flow.

P11L31 rephrase. I think you want to say something like we adjust the calibration constants in the data logging software so that the pitot tube reports an air speed close to TAS rather than PAS. When this is reported to the instrument it causes the (insert info about lines imaged at the correct rate), but the PAS may still be recovered later by using the recorded pressures and the correct calibration constants.

P12L14 The images shown in figure 12 are clearly distorted. However, it is not at all clear to me that this is a distortion of the droplets or just the images. In particular the images show a skew. This skew could be due to misalignment of the particle trajectory with the instrument axis, or perhaps it could be due to shear flow distorting the particles. This skew in itself causes an apparent lengthening of the particles in the x direction – you will note that the farthest shadowed pixels on a droplet do not fall on the same line. It is not clear if any corrections for this have been made. Until a model can account for the skew distortion and the flattening I think it is difficult to claim which mechanism is responsible. I think it is appropriate here to discuss how flattened the droplets are expected to be and to suggest it is a possible mechanism in the context of other possible mechanisms. But I don't feel it has been proven here that droplet flattening is the definite cause. Indeed the authors have the flow data to answer the question – are the particles travelling parallel to the probe axis?

P13L5 It appears that airspeed errors of 10-20% would be perfectly sufficient to centre the distribution of dy/dx over 1.0 for Atom-1. The a-life and Atom-2 data appear to show the opposite effect to that which would be expected from the droplet distortions described here. Please explain.

P13L21 Be specific about what a small deviation is. Give the amount.

P14L23 There is no evidence for Taylor instabilities in Fig 13. Visually the scatter in the data looks to be entirely consistent with the error bars that the authors have put on the data. The authors would need to do some further analysis of the data scatter and if they find that the scatter is too large to be explained by the uncertainties, then and only then, should they invoke a mechanism to explain the extra scatter.

P14L28 Something here is not really matching up. The authors state that for a 200 um droplet they have We of 2.5 and it cannot be considered spherical, however close to that diameter on figure 13 the particles seem to generally have dy/dx within one error bar of unity. The author appears to be suggesting that observed broken up particles are caused by the mechanism here, or at least that some aircraft can go fast enough that droplets will be broken up so cannot be observed by the probes above a certain diameter. But the points in Figure 15 show droplets going up to aspect ratios of 1.5 to 8. The only times I have seen such distorted particles has been when the refresh rate of the diode array has been clearly wrong. It may be that the authors have extrapolated the data for figure 15 past the break up point or perhaps there is something I am not able to piece together. But I can only suggest that section 3.2 needs a really thorough rewrite in order to ensure the arguments being made are consistent with the data and the model and to be clear what is a discussion and what is a conclusion.

P14L29 I feel that the items discussed in section 3.2.2 are probably an overanalysis of the data. Again the authors should be clear about what is a conclusion and what is a discussion and before they begin suggesting corrections for an effect they first need to be clear that they have shown the effect is real.

P15L25 should be clear that size is diameter rather than radius

P15L30 see comment above re corrections

P16L5 This set of steps would be much simplified to the point of being obvious if the advice above is taken regarding making the equations a bit clearer and specifying which air speed is being used for Stk.

P17L3 Again it is not clear to me that droplet deformation as a cause of the image distortion has been proven.

P17L13 Stating this as a potential limit for the drop size that can be seen as a function of aircraft speed is I think very interesting, but my gut feeling is that observed droplet breakups are caused by impacts or near impacts with the probe.

P26Fig2 It would be nice to highlight the test case.

P27Fig3 The lines of best fit are linear – the word polynomial should be saved for higher order equations. Replace with Linear. All plots should at least have x and y axes that start at the same value. I would ideally like to see the same max value on the x and y axes too, but I appreciate that for Fig2d, this may not be appropriate.

P27Fig4 ensure that 0 is marked on the colour bar.

P28Fig5 It would be nice to see the test case highlighted

P29Fig6 It would be nice to see the test case highlighted

P36Fig13 The lines have no proper description of what they mean – are they averages over some size range? I think they are not useful anyway and should probably be removed unless the authors can argue a good reason for them. Also 2 different shades of grey should not be used. It is hard to tell them apart.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-27, 2019.