We wish to thank the reviewer for carefully reading our manuscript and for providing suggestions which helped us to improve the manuscript. In the following, the questions and comments raised by the reviewer are marked in blue and our answers are written in black.

This manuscript is a useful addition to the growing body of work investigating the influence of airborne particle instruments on the measurements that they are making. The most important aspect for me is the influence of Stokes number on particle velocity through the probe sample volume, this is nice. That said, I think that there are a couple of general and more specific issues that I would prefer to see addressed before publication.

The flow model includes the airframe, or possibly a section of wing and the canister plus pylon, and a hemispherical dome approximation of the probe. Some more details about how much of the aircraft is included in the model would be useful and if the flows at each of the four hard points are the same or different. No specifics are given for modelling the DC-8, it is unclear whether the flows at the probe location on the DC-8 shall be similar or the same as those on the Falcon. This is important as one of the conclusions relates to minimizing the difference between TAS and PAS by judicious positioning of the probe on an aircraft.

Related to this question, we give more detail below. Generally, the flow under the wing is slightly varying along the wing since the wing cross section varies between the aircraft fuselage and the wing tip. Because of this, small differences between the hard points are possible. We haven’t modelled the DC-8 since our results show that the concentration depends only on the ratio between the local air flow and the free stream air flow.

The angle of attack (AOA) of the Falcon is briefly mentioned in section 2.1.2. It appears that the AOA is held constant for all flight conditions used in the simulations such that the orientation of the probe is always aligned with the local flow, or at least in so far as the error associated with the misalignment is negligible. Given that the simulations are done over a very large range of pressure and TAS and that an aircraft’s AOA maybe expected to change by degrees over this range, it would be useful to see some justification for this statement.

We have performed numerical sensitivity studies related to the dependency of the results on AOA. Figure 1 (in the reply) shows the deviation between PAS and TAS as a function of ambient pressure color-coded with the angle of attack (AOA) for the arbitrarily chosen TAS range from 152.5 m/s to 157.5 m/s from measurements during the SALTRACE campaign. A TAS range had to be chosen for this figure because the deviation between PAS and TAS depends also on TAS. However, the picture is similar for other TAS ranges and shows that the AOA has only a minor impact on the measurements ((PAS-TAS)/TAS changes less than 2% when the AOA is changed). Therefore, we chose a median AOA value (4°) for our analysis.

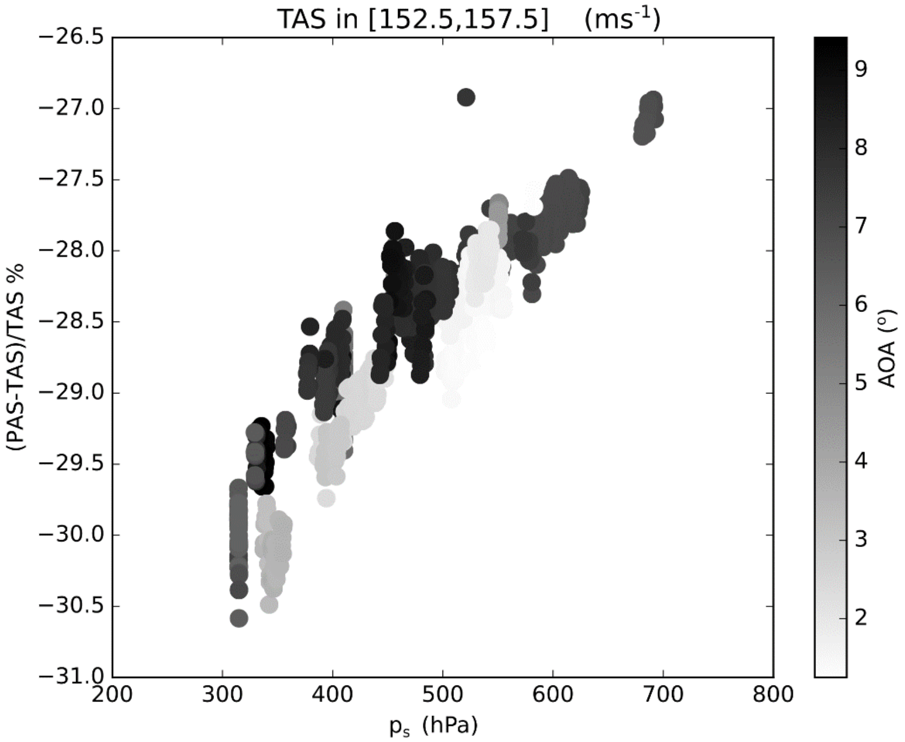

Figure 1. Deviation between PAS and TAS as a function of ambient pressure color-coded with the angle of attack (AOA). Markers represent measurements at 1Hz collected during the SALTRACE campaign.

No mention is made about these matters with regards to the DC-8 either.

On the DC-8, CAPS is mounted near the wing tip, i.e. it is less affected by the AOA, since the wing chord and the wing thickness is smaller at this part of the wing.

This work appears to approximate the probes with a semi-hemispherical dome. It would be a significant addition (and an unreasonable request) to include the actual probe geometries. However, a comment comparing this work to that of Weigel et al. (2016) and Korolev, Emery, and Creelman (2013), both already cited and which use accurate probe models, would be very interesting given the influence of the probe design on the local flow distortions and the apparent similarities in PAS/TAS ratios seen.

Figure 6b and 6c in the manuscript show the measured and simulated differences between p and PAS at the probe location and the corresponding values at free stream condition. The simulated values are well within the measured range. Therefore, we think that the simplified instrument geometry is a valid assumption.

Specific Comments

More specific comments are included here. I have included the page.line number/s to assist although some of these comments may apply to multiple places in the text.

3.31 The manuscript uses ‘airspeed’ extensively throughout. It would be useful in the context of airborne measurements to clarify that true air speed is used as opposed to any of the other definitions used with regards to aircraft. TAS and PAS are defined later at 5.20. ‘Speed’ is mentioned in section 2.1 but this clarification may be introduced earlier if more convenient/coherent. See comment 7.22 below for more.

We changed this in the manuscript as suggested.

4.3 Air speed uncertainties are more relevant here than those in static pressure, can these be related to air speed errors for the range of conditions relevant to this work?

Yes, if we assume the errors are only affecting the static pressure. In this case, a 1% overestimation of the static pressure will result in a 0.5% overestimation of the air speed according to the Bernoulli equation.

4.5 Are the uncertainties of the DC-8 MMS known? Are they comparable with those of the CMET?

Yes, the two uncertainties are similar and represent errors smaller than 2% on the total pressure. Figure 2 shows the deviation of the total pressure between CAS and the Falcon CMET system during SALTRACE and between CAS and the DC-8 MMS system during ATom-1.

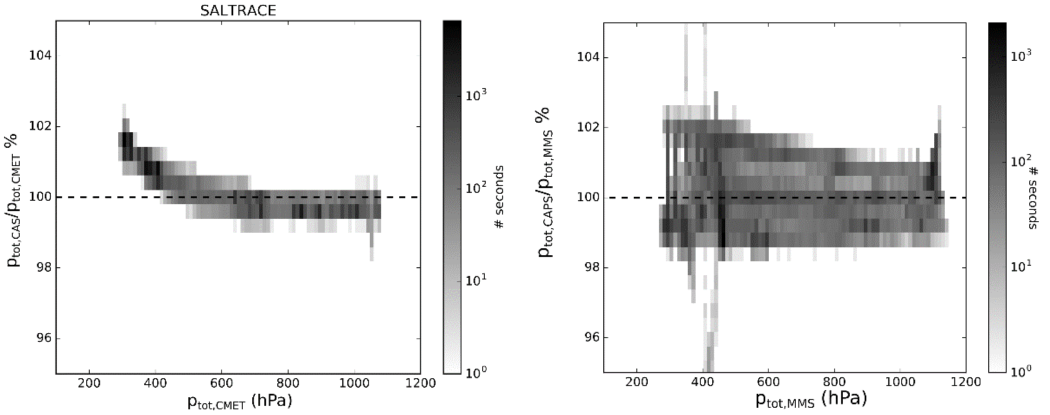

Figure 2. Deviations of total pressure measured by CAS and by the Falcon CMET system during SALTRACE (left), and the DC-8 MMS system during ATom-1 (right), respectively.

4.30 In my opinion it is inaccurate to say that most probes have pitot tubes close to the sample volume. The DMT PIP- or CIP-based instruments do, the AIMMS does (although is less relevant as it does not measure all that close to particle instrument sample volumes), but (most/all?) other instruments do not. So actually, the reverse of what is stated is the case. I’d suggest adding the importance of local PAS measurements to the conclusion of the manuscript with discussion about when it is most/least necessary based on what is being measured.

We changed the text in the paper as suggested. It reads now:

“As we described, OPC and OAP measurements depend on the flow, therefore wing-mounted instruments are sometimes equipped with flow sensors to constrain local conditions.”

In addition, we added text to the conclusion of the manuscript explaining the importance of local PAS measurements.

5.26 Positional errors are discussed throughout the manuscript and I found the usage confusing. Positional error, when defined in section 2.1.2, relates (as I read it) to the difference between TAS and PAS where TAS is the free stream air speed and PAS is the airspeed in the sample volume in the probe. However, in 9.9 it seems that positional error possibly also refers to the difference between the airspeed measured at the pitot on the probe and that at the sample volume of the probe (due to the difference in PAS measured by the long and short pitot tubes). However, there is no specific mention/quantification of the difference in air speed between the pitot and sample volume. Please clarify the usage if possible.

With the term “position error”, we refer to the difference between TAS and PAS. For this reason, a longer pitot tube will reduce the position error because the difference of the pressure at the probe and at free stream is exponentially decreasing as a function of the distance from the probe head. We assume the pitot tube measurements to be representative for the sampling area (see Fig. 5 and text of the revised manuscript). Thus, using a longer pitot tube, implies the sampling area also to be at a larger distance from the main instrument body. This is the case for CAPS.

6.16 Does “simplified three-dimensional model” mean a hemispherical dome as shown in Fig 4? If so this is quite the simplification for a CAPS for example. So mention of expected uncertainties due to this simplification would be useful.

As mentioned previously, Figure 6 in the manuscript shows the measured and simulated differences in pressure and air speed at the probe in comparison with free stream conditions. The simulated values are well within the measured range. Therefore, we think that the simplified instrument geometry is a valid assumption.

7.22 The use of ‘airspeed’ throughout is not as clear as it should be. In this sentence it is used to describe the velocity of a particle relative to the volume of air that contains it (slip velocity). Previously it has been used to describe the velocity of the aircraft and/or probe relative to the air. The difference is very important in this work so I suggest defining unique terms for consistent use throughout. TAS and PAS are traditionally used for the latter case so I’d suggest the use of slip velocity or some other term for the former.

We modified the text and the figures, according to your comment and now use “slip velocity”.

8.23 Mention should be made to confirm if aircraft angle of attack changes with TAS in this model (here and elsewhere).

The simulations presented in the manuscript were made for a fixed AOA (4°). This AOA was selected to represent the median conditions as visible in Figure 3. Also, it can be seen from Figure 1 that changes of the AOA have only a minor impact on the relative deviation of PAS from TAS.

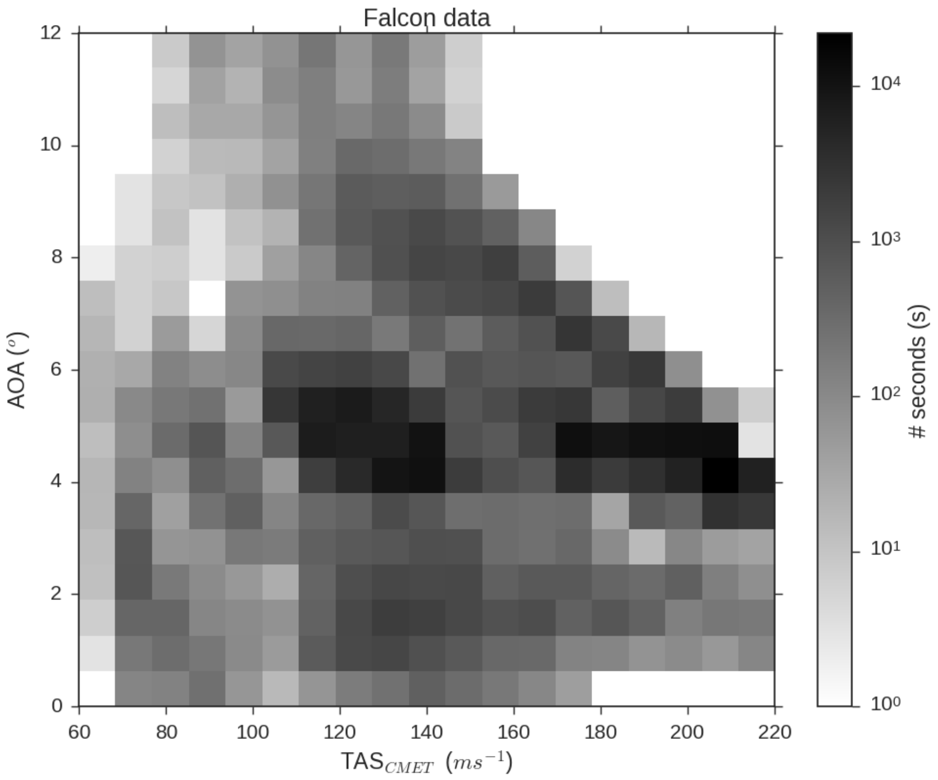

Figure 3. Frequency of AOA vs. true air speed (TAS) during the SALTRACE campaign in 2013.

13.11 There is something missing in the description of the Vargas experimental set up which made interpretation difficult. It turned out that this is the fact that the droplet is ’dropped’ perpendicular to the plane of the rotating airfoil and therefore has a velocity in that plane of zero when far from the airfoil. An extra sentence to more fully describe the experiment that you are simulating would be useful. Also suggest using slip velocity instead of Urel as this is a commonly known term.

We extended the description of the Vargas experiment to clarify it. We also substituted Urel with Uslip in the entire manuscript.

How is ‘breakup’ that occurs around 60 ms−1 defined/identified given that the images in figure 14 contain only a single particle?

According to Vargas (2012), droplet breakup is occurring at the edge of the droplet by a stripping mechanism. In the volume of fluid (VOF) simulations the breakup is identified by considering the number of regions in the simulation which contain a liquid phase. If more than one such region is identified, the droplet is counted as “broken”.

14.10 It is not obvious to me why the varying slip velocity case is different from the invariant one. An explanation would be appreciated.

According to Vargas (2012), the droplet in case of a varying slip velocity has more time to adjust to the flow. We extended the description and added a reference to Vargas (2012).

17.4 VOF has not been defined previously.

Corrected.

17.5 Has there been discussion about “wiggling” behavior previously? More specific language is required here.

According to the comment, we removed the term “wiggling” and reformulated the sentence.

17.6 Have Taylor instabilities been discussed previously?

Yes, they were mentioned on 14.22. We reformulated the sentence and removed this term from the conclusions.

3 Technical Corrections

There are quite a few basic notation inconsistencies, spelling mistakes, and technical errors that make following the text difficult and detract from the quality of the manuscript. Below are listed the technical corrections that I have caught, given as page. line numbers.

3.32 “per default” is quite an odd expression. “By default” would be more typical although in this instance I think the sentence makes sense without either, ie “The DLR Falcon is equipped with a...”

Done.

4.3 Does “pilot-induced manoeuvres” mean turns but not fluctuations due to auto-pilot corrections or turbulence? Clarification of this phrase would be helpful.

Yes, according to the cited article it refers only to pilot-induced maneuvers. Auto-pilot corrections or ‘normal’ turbulence probably result in even smaller errors.

4.19 Is “composed instrument” supposed to read “compound instrument”?

We modified the text and now leave out “composed”.

4.21 A OAP does not measure diameter directly, the user/software determines the size from the image recorded by the array. Different size metrics are defined in section 3.2.2 and so could be referred to here.

We modified the sentence including a reference to 3.2.2.

4.23 I’m not sure what is meant by “named shutter-speed of the camera”.

We modified the sentence and now leave out “named shutter-speed of the camera”.

4.24 The SID is a scattering instrument not an OAP so should be removed from this list.

SID has been removed from the list of OAP.

5.19 It seems as if “attack angle” here is used in reference to the alignment of the probe to the local air flow. This is confusing as “angle of attack” is traditionally used in reference to the alignment of the aircraft to the air flow (and indeed appears to be used in this way in 5.16). This should be clarified.

Your comment is correct. To avoid misunderstanding, we modified the sentence.

5.21 I’d prefer that “measured” is removed as the PAS is the airspeed at the probe location whether it is measured or not.

Done.

6.16 Change PMS and add canister to sentence to read “...Particle Measurement Systems canister...”

Canister in mentioned later in the same sentence so that we think it is not necessary to add it here.

6.21 Do you have a reference for snappyhexmesh?

We added a reference to snappyhexmesh:

Montorfano, A (2017) Mesh generation for HPC problems: the potential of SnappyHexMesh, doi:10.13140/RG.2.2.25007.53923

6.24 There is some confusing notation regarding *U*, ***U***, and U. The usage should be consistent, particularly between the italic and upright versions. One can reasonably assume that U0 is defined along the free stream direction given|U0|=TAS but an explicit definition of the axes along with consistent nomenclature would make this is clearer.

Done. For clarity, we included a new table in the revised manuscript giving an overview of the different velocities and speeds.

7.13 Change “as” to “an”.

“As” was correct, but “an” was missing. Now it reads: “Droplet breakup, as an effect of the instability … ”

7.16 Change “model including...” to “model to include...”.

Done.

7.17 Change “not fully agrees...” to “does not fully agree...”.

Done.

7.27 I’m not sure what is meant by “...both in the spatial and temporal discretization”. Should “in” be replaced by “with”?

We think that “in” is correct, but modified the sentence slightly to make it more clear. It now reads as: “… both in the spatial and in the temporal discretization …”

8.26 This sentence uses upright U, should they be italic?

Corrected.

9.3 The sentence starting “The simulation results refer to...” is unclear. Is this “The figure refers to...” or “The simulation results are relative to...” or something else?

Improved. The sentence reads now “The simulation results in Fig. 6 are valid …”

9.9 See discussion on positional errors in the previous section but in this sentence it would be useful to know why and how the longer pitot tube effects to positional error. This shall assist in understanding the uncertainties when applying the methods discussed to instruments with different pitot tubes or even using pitot tubes mounted near a probe but not as part of the instrument.

See also our reply to comment 5.26 (p. 4). The text in the manuscript has be revised.

9.17 Typo in “However”.

Although there is the term “howsoever”, we now use “nevertheless”.

10.9 The test case u100_p900 is not actually included in Figure 8.

The case u100_p900 has been added to Fig. 8.

11.16 Specify which air speed you are referring to here.

As recommended, we use "probe air speed” to make the message more clear.

12.18 It may be pedantic but Figure 12 shows circular images (which admittedly one assumes are of spherical drops).

We changed the text as follows: “Most of the droplet images are circular…”

12.23 The x and y dimensions should be defined in terms the array and with time (in terms of which the image is described in 12.16) so that the reader understands the aspect ratio values. This is clarified in 13.1 but should be brought forward to this point.

We modified the manuscript as suggested.

12.24 The markers in Figure 13 are not black.

We modified the Figure 13 which now uses three different colors.

14.3 The symbol for Weber number should be italics so that it is more obvious in text.

We modified the Weber symbol in the entire text.

15.1 Add what threshold value you have used in your greyscale images when finding image area.

We have used a threshold value of 0.8. The value is now specified in Sect. 3.2.

18.22 This is cited (incorrectly) in the body as Osborne and Cotton. The full author list is required here.

The SID reference has been removed according to your comment and consequently the reference too.

45.6 The data DOI needs updating.

We are in the process of updating the data DOI. Since we included new data, the update takes more time.

Comments on figures:

Fig 2 Many of the figures are shaded by number of seconds, for clarity it would be useful to add “...number of seconds of data at 1 Hz".

We added the “...number of seconds of data at 1 Hz" in the figure captions where necessary.

Fig 4 The free stream flow should be marked on this plot. There is some confusion regarding the orientation of the flows in the model between figures 4, 5, and 7. In figure 4, if the free stream is oriented with the figure then it appears as if the angle of attack (AOA) of the aircraft is not accounted for and the probe is not aligned with the flow. In figure 5, the free stream is aligned with the probe and so the AOA has been accounted for, however for the given flight conditions I assume that the AOA is changing, and this is not mentioned (see previous comments regarding AOA). In figure 7, the free stream seems to be coming up to the probe from below (which is opposite to the case shown in figure 4) or perhaps aligned with the probe, it is difficult to tell.

To make the message more clear, we changed the color-scale and added an arrow indicating the direction of the free stream flow. We also modified the scale label now using “(U-U0)/U0” to be consistent with Figure 5.

Fig 5 The pitot tube seems to be located diagonally off centre from the dome. Does the schematic represent where the pressure/velocity was measured or was it measured in front of the stagnation point of the dome (I assume that this is where the sample volume is)?

Correct, the pitot tube is located diagonally off center in the instrument and in the simulations. The schematic represents where the pressure/velocity was measured/simulated. The simulation results refer to the pitot tube static port location.

Fig 7 It would be useful to use the same colour scale parameters in this figure as in figure 4. When I converted from ratio to difference the values appear inconsistent between these two figures, is this an issue with my maths/understanding of what is being presented or an actual inconsistency between the two plots?

We modified the Figure. Now it is showing the density of the air ($(\rho-\rho_0)/\rho_0$). We modified the text accordingly.

Fig 13 The main text and caption refer to black markers, maybe it's my display/print but there doesn't appear to be any black markers.

The A-LIFE and ATom-2 data have similar shades of grey, completely different colours would be significantly clearer.

We modified the figure using now three different colors.

Is there a reason that the red trace, the mean for the ATom-1 data does not approach unity for small particles?

Yes, the bias is introduced by the wrong droplet speed (PAS instead of TAS) for the image reconstruction.

Fig 16 There appears to be multiple data points for the same TAS values (most evident for 125 ms^{-1}), is this a plotting error or does it illustrate the different test cases? If the latter, it should be mentioned that P and T don't make a lot of difference.

For some velocities (TAS) there are more than one test case (see Table 2 in the manuscript for details). We added a clarifying comment in the manuscript.