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.

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 micro-physics instruments) and uses these to determine biases and corrections upon cloud microphysics measurements. It also examines how this airflow can induce droplet de-formation 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 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.

The flow around the aircraft modifies pressure and temperature of the air and as a consequence of the gas law also the air density at the instrument's sampling area. Consequently also the particle concentration, in particular of small particles, is affected. Furthermore, the flow distortion also modifies the sampled flow velocity. For example, higher pressure is related to a lower flow velocity (Bernoulli equation, energy conservation).

We have updated the manuscript.

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).

They can appear deformed because of both mechanisms:

1.) Droplets may appear as deformed on the images, but they are not deformed on reality. This is the case if the camera does not use the correct velocity for taking the images.

2.) Large droplets are deformed due to aerodynamic forces. Both citations refer indeed to shape distortion mechanism at the droplet surface.

We rephrased the paragraph to make it more clear.

P2L32 compressible flow is not a hypothesis

We reworded the text and skipped the word 'hypothesis'.

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

Done. We now only use the word aircraft.

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.

We removed the SID from the list of instruments.

P5L15 Insert the word measured before "dynamic pressure"

Done.

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

This statement is generally valid, i.e. to any velocity derived from pitot tube measurements using the Bernoulli equation under the assumption of an incompressible fluid. For example, using the Bernoulli formula for incompressible gas together with the nose boom pressure values will lead to an overestimation of TAS. In contrast, the same formula applied to the pitot tube of the wing-probe instrument will lead to an overestimation of the PAS.

We adapted the text.

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.

Figure 1 below shows comparisons of the total pressure measured by the pitot tube of the CAPS with measurements of the CMET pitot tube on the DLR Falcon and the MMS pitot tube on the NASA DC-8, respectively. The deviation between the different pressure sensors is less than 2%.

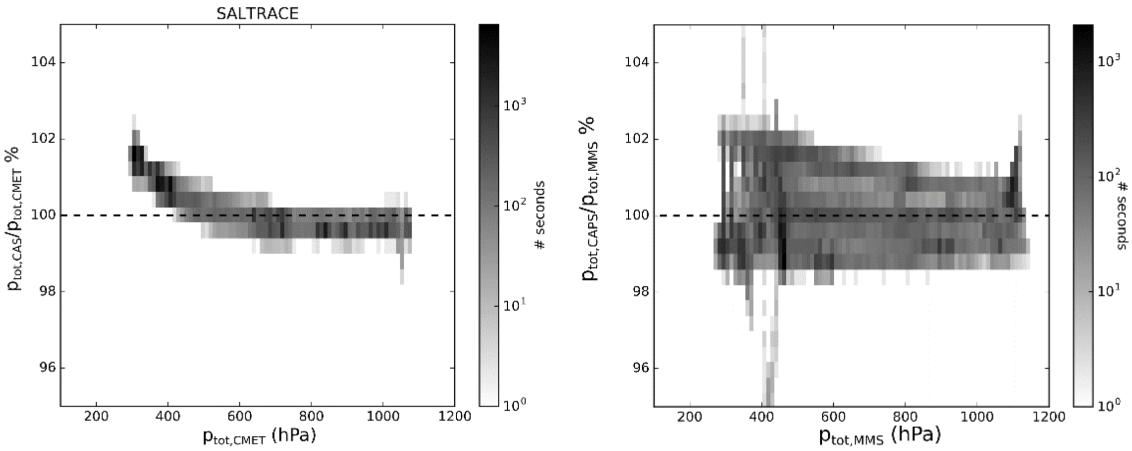

Figure 1. *Comparison between different total pressure measurements as a function of ambient pressure. Left: Ratio between CAPS pressure measurement and the DLR Falcon CMET pressure measurement. Right: Ratio between the CAPS pressure measurement and the NASA DC-8 MMS pressure measurement.*

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.

It was computationally too expensive and therefore not feasible to include the entire aircraft fuselage in the simulations. However, the pylon where the probe is mounted is about 1.5 m away from the fuselage therefore the wing and the pylon/canister/instrument are the main contributors to flow distortions at the measurement location. Furthermore, the measured and simulated differences in pressure and air speed at the probe in comparison to free stream conditions show that the simulated values are well within the measured range (see also Figure 6 in the manuscript). Therefore, we think the exclusion of the fuselage does not introduce a significant bias.

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 in to decisions made by future studies into flow effects on microphysics measurements.

We performed new numerical simulations only considering the effect of the wing. The result is shown in Figure 2. The wing will contribute only up to 9% whereas the total effect is up to 24%.

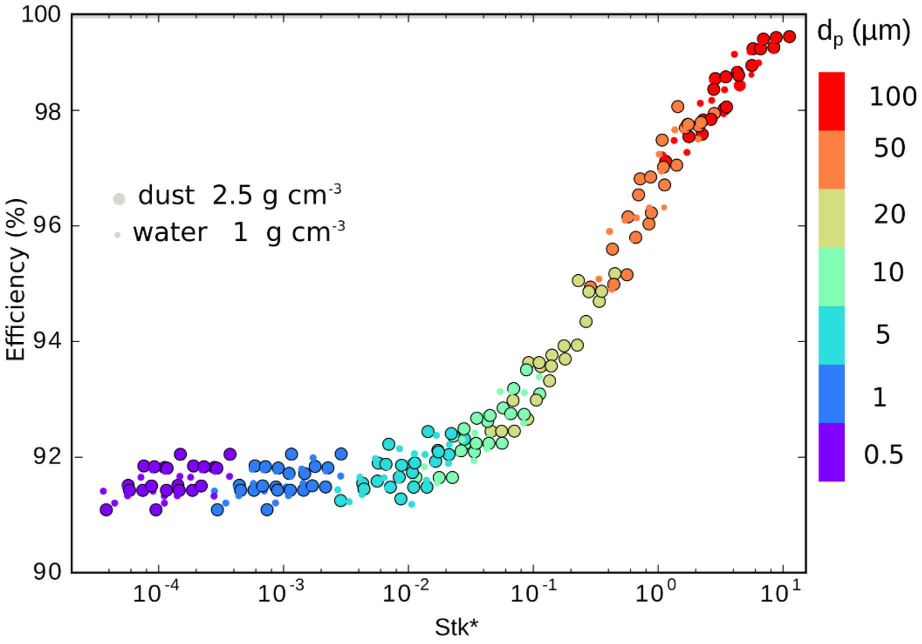

Figure 2. *Same as Figure 8 in the manuscript, but only simulating the effect of the wing. The sampling efficiency is calculated as a function of modified Stokes number (see Eq. 4) for the selected numerical test cases of Tab. 3 in the manuscript. Each marker represents a run where we released 2 x 10^5 particles of a specific diameter (colors) and density (small marker size: water; large marker size: dust) calculated at the upstream of the probe in the computed flow field. Sampling efficiency is defined as the ratio between particles released and particles passing through the sampling area, renormalized by the corresponding areas.*

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.

The test case (u100_p900) is always plotted with blue color.

P8L21 replace probe and free stream with local and free stream

Done.

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

The incompressible solution will look like the u75_p1000 simulation where the effect of is still small. We added a statement in the text.

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

Done.

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.

The temperature bias has only a minor effect. We kept mentioning the temperature bias in the text but also made clear that for our case it only has a minor effect.

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.

For small TAS (below ~90 m/s), the configuration of the aircraft may be different (typically these speeds are observed during take-off and landing where the flaps are used). Also, the effect from the ground might be not negligible at low TAS. This is why we are not surprised by the small deviation between simulations and observations at these TAS values.

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.

Figure 3 (see below) shows the position of the CAPS temperature sensor. The sensor is located at the same position in case of the CAS instrument.

Figure 6 (left panel) in the manuscript shows a statistical analysis of differences in the ratios between temperature values measured with the Falcon CMET system and with the CAS instrument during SALTRACE.

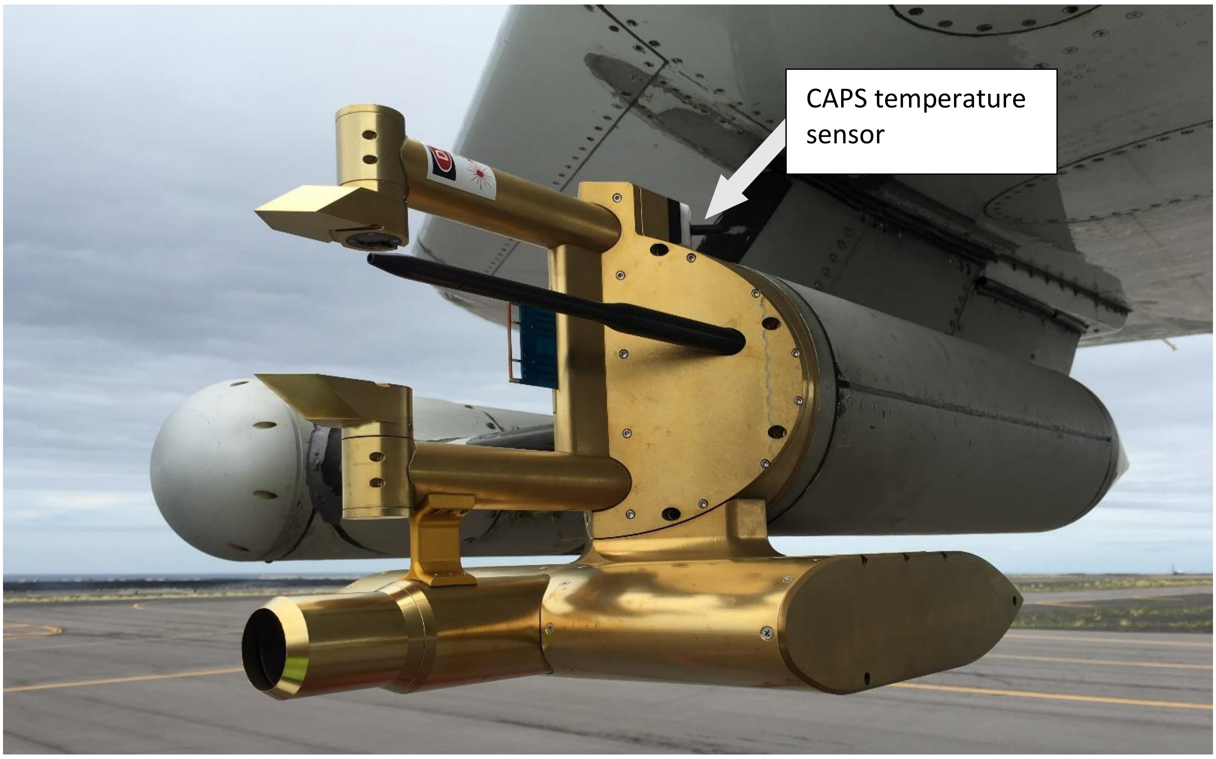

Figure 3. *Photograph of the CAPS mounted at the wing of the NASA research aircraft DC-8 during ATom-4. The position of the temperature sensor is marked by the white arrow. (Photograph: B. Weinzierl).*

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

According to Israel and Rosner (1982) the Stokes number is modified by introducing the correction factor psi which is a function of the Reynolds number distinguishing between the laminar and the fully turbulent case.

The text was modified accordingly.

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

To calculate equation 4 we use TAS. We added a new sentence to clarify the procedure.

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).

The fit lines were obtained using a generic sigmoid function of the Stokes number using x0, k0 and k1 as free parameters. In the second step, we correlate these numbers (x0, k0, and k1) with flight conditions expressed as the alpha parameter.

We would also like to point out that there was typo in equation (6) where a plus was missing. We replaced it with the corrected equation.

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.

The formulas given for x0, k0 and k1 were not correct. With the correct equations, the effect of alpha is stronger. Alpha is close to 1 in our case, but can have a larger effect in other cases. Therefore, we decided to keep the more general formula.

Thirdly, the authors should note that e^(-k0(log(stk)-x0) can be rearranged to b*stk^a 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=10^(x0*log(e^k0)) and a=-log(e^k0). Which makes b approximately -1.

We rearranged the formula.

Fourthly, it may be useful for the authors to rearrange the form slightly as this may help the points all converge onto 1 line.

We are not sure whether we understood this comment correctly. If the comment refers to Figure 8, we would like to clarify that the points in Figure 8 are not sitting on a 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.

We considered the recommended explanation and the references in the text.

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.

See above.

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.

Yes, the term mobility is used in the same sense that it is typically used in aerosol science.

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

We improved the subsection title.

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

We corrected the sentence now referring to particles with 30 µm diameter.

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.

We rephrased the sentence as recommended.

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 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?

The observation is correct. The particle trajectories especially for droplets with d>50µm can be considered parallel to the AOA. The difference between the AOA and the probe axis is in the considered case small enough to be negligible. Indeed even considering a 1 degree angle between the AOA and the instrument axis this will result in a orthogonal component of TAS*sin(1°) which is less than 2% of TAS.

We would like to point out that only the larger droplets in Figure 12 are distorted, the smaller droplets (300nm and smaller) are not. This lead us to the conclusion that it is not the image what is distorted, but the distortion is caused by an aerodynamic force which preferentially acts on the larger droplets.

To make this point more clear, we included additional measurements from ATom-4 in Figure 12.

P13L5 It appears that airspeed errors of 10-20% would be perfectly sufficient to center 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.

We included additional data from ATom-4 also in Figure 13.

We would like to point out that for ATom-1 the CAPS pitot tube was calibrated to measure the PAS, which for large particles leads to errors in the particle's/droplet's velocities of about 15% (depending on particle/droplet size). See Figure 11a in the manuscript for ATom-1. For A-LIFE, ATom-2 and ATom-4, we modified the calibration of the CAPS pitot tube to measure the TAS. The reason was to force the instrument to take the CIP images with the correct camera speed (large particle move with a velocity close to TAS).

If droplets start to deform by elongating in the y-dimension, they have a higher chance not to be fully recorded (see red contours in the new Figure 12 in the manuscript) and consequently they are removed from the analysis (Figure 13). This means that the deforming effect becomes less visible on larger particles.

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

The results of our simulations are within two times the error bar of the experiments by Vargas (2012). We extended the text.

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.

In the range between 200-500 µm, the dx/dy=1 line is outside the range of the error bar for most droplets. We think the data is sufficient to say that there is some scatter. We believe that the mentioned instabilities can be a possible reason for this 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.

Figure 12 shows deformed and broken particles/droplets (see red contours). According to Vargas (2012) this effect can be explained by aerodynamic forces.

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.

We would like to point out that Figure 15 shows the results of simulations and not the analysis of droplets measured with the CIP instrument. In the grey-shaded area of Figure 15 there are also broken particles.

P14L29 I feel that the items discussed in section 3.2.2 are probably an over analysis 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.

We would like to refer to Vargas (2012), who has shown the effect in a laboratory setting. Therefore, the shown effect is probably real. Furthermore, the simulations reproduce the same effect. Therefore, we think that a model-based testing of different formulas for the droplet volume calculation is useful for the reduction of errors of the droplet volume calculated from droplet images detected by OAP.

P15L25 should be clear that size is diameter rather than radius

Done. Everywhere, diameters are used.

P15L30 see comment above re corrections

See comment above.

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.

We modified the manuscript and would like to point out that the velocity used for calculating Stk number in our case is the TAS.

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

The images in Figure 12 show that smaller droplets appear circular while larger particles appear deformed. We extended Figure 12 to include droplet images from another campaign. They confirm the same finding. We extended the sentence.

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.

In Vargas (2012), the effect has been observed in lab experiments. This lab experiment has been verified in our simulations.

P26Fig2 It would be nice to highlight the test case.

In all figures, the test case u100_p900 is marked in blue.

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.

Probably this comment refers to Figure 2? We updated Figure 2. Axes ranges for x and y axes start and end at the same values and “polynomial” was replaced with “linear”.

P27Fig4 ensure that 0 is marked on the colour bar.

0 is now marked in 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

In all figures, the test case u100_p900 is marked in blue.

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.

We modified the caption adding a description of the lines. The different campaigns are now plotted with 3 different colors and markers.