

Response to Reviewers

Anonymous Referee #2

Ryder et al. discuss airborne measurements of Saharan dust performed between Cabo Verde and the Canary Islands in August 2015 with focus on several aspects of the dust, for example the coarse mode size distribution. I think the paper is interesting and fits well into the scope of ACP. Overall, the paper is in a useful shape but there are many, mainly minor, things the authors should improve before the paper is ready for final publication.

We thank reviewer 2 for their comments and are pleased that they find the paper interesting. We have responded to the minor comments below and corrected or altered them in the manuscript. In particular the question about why we did not process the CIP15 data with the XY metric has led to additional reprocessing of some of this data, which has been informative to the manuscript. Further details are given below. We apologize for incorrect cross-referencing of tables and figures, which have all now been corrected. We have added clarification to our flight patterns section to better explain the data.

General comments:

Abstract: Maybe one sentence with results for the MBL could be added.

We have added, "Within the MBL, mean effective diameter (d_{eff}) and volume median diameter (VMD) were 4.6 μm and 6.0 μm respectively, giant particles with a mode at 20-30 μm were observed, and composition was dominated by quartz and aluminosilicates at $d > 1 \mu\text{m}$."

On p316 you define coarse and giant mode dust. However you do not follow this definition, e.g. at p7115, p9132, p12127, p25125. Please make sure that the paper is self consistent.

We have changed the introduction to reflect the references throughout the rest of the text. It now reads, "Coarse and giant mode dust (defined here as $d > 2.5 \mu\text{m}$ and $d > 20 \mu\text{m}$, respectively)..."

Often the citation type, e.g. '(Ryder et al., 2013)' vs. 'Ryder et al. (2013)', is not correct.

These have been corrected throughout the manuscript.

You did not define the flight legs. At p1115 you mention 'R2' for the first time, but you did not introduce this properly. In Fig. 4 an undefined 'P2' etc appears, probably also referring to flight legs. I suggest to add a proper introduction of the nomenclature and to add a table (maybe in the supplement) with more details about the flight legs considered in the paper (e.g. name of leg, start time, end time, duration, height, etc.).

We refer the reviewer to Section 2.1, 'Flight Patterns,' where in the final paragraph profiles and straight-and-level flight legs are explained and defined. We have added an explanation of R and P which refer to straight and level runs (SLRs) and profiles respectively, as suggested by the reviewer. We have also changed the terminology throughout the paper to use SLR rather than 'flight leg,' since SLR relates more closely to the 'R' abbreviation and is also the abbreviation used by the UK research aircraft community. Full time and altitude information for profiles and SLRs can be accessed online. We have added, "Full information about profile and SLR times and altitude are available from the Centre for Environmental Data Analysis (see Data Availability)."

You write SSA in the text but use ω_{0}^{550} in the figures. Please use only one of these.

We prefer to use ω_0 for the figures, so have added this to the first appearance of SSA in the introduction, "decreasing the single scattering albedo (SSA or ω_0)."

Sometimes for the imaginary part values a 'i' was added after the value, sometimes not. This should be made consistent, preferably removing 'i' everywhere because the 'imaginary part' is a real value; see also your definition at p8118.

The 'i's' have been removed from the text.

Often, the main text refers to the wrong table number.

We apologize for this and have changed the cross-references.

Specific comments:

p1l24: In my view 'during' should be replaced by 'at'.

Changed

p1l28: 'constituting up to 40% of dust mass': As you mention this number in the abstract, it should also be mentioned in Sect. 3.3.

We have added to Section 3.3, "In the extreme, up to 90% of dust mass can be found at sizes greater than 5 μm and up to 40% at sizes greater than 20 μm ."

p2l3f.: It is unclear what 'this complex evolution' refers to. Suggestion: '... to capture correctly both the dust composition and the size distribution including their changes during transport in order ...'

We have rephrased this sentence.

p3l27: 'preceeding the AER-D flights' comes a bit surprisingly. Please reformulate without referring to AER-D which is introduced only later.

We have removed the mention of AER-D and rephrased the sentence.

p3l33: I think 'however' could be removed here.

Done

p4l1: 'Mie theory conversion': it is not very clear what is meant. Please reformulate.

This has been changed to, "the scattering cross-section to particle size relationship is non-monotonic."

p5l7f.: 'The dust events sampled 550 nm AODs from 0.4 to 0.8' should be reformulated.

This now reads, "The dust events revealed AODs at 550 nm from..."

p5l19: 'aerosol structure' is a bit unclear. You probably mean the vertical distribution (structure) of the aerosol.

Correct, we have changed this

p5l20: 'nearer the ...' could be replaced by 'closer to the'.

Done

p5l26: 'Figure 1b' does not exist.

Changed to Figure 1

p5l28: 'flight' could be added before b920 and b924.

Done

p7l3: The wavelength list may be a bit confusing. Maybe you can just write the wavelengths in parentheses after the instruments?

This now reads, "Scattering measurements were made by a TSI 3563 integrating nephelometer (at wavelengths of 450, 550 and 700 nm). Absorption measurements were made by a Radiance Research Particle Soot Absorption Photometer (PSAP) at 567 nm."

p7125: 'aerosol' should be replaced by 'particle' to make the sentence more general. Particles with 6.2mm are usually not aerosol particles but much more likely some kind of hydrometeors.

Done

p8118: PCASP and CDP do not operate at 550nm. This should be mentioned here including your assumption that the refractive index does not change between 550 nm and the instrument's wavelengths.

We have altered this paragraph which now reads, "Bin sizes also depend on the choice of refractive index applied. In this work, a complex refractive index ($n^{550} = m^{550} - ik^{550}$) of 1.53-0.001i was used to determine the PCASP and CDP bin sizes, as determined from Section 2.5 for 550 nm. Since the PCASP and CDP operate at wavelengths of 633 and 658 nm, we assume a constant refractive index across these wavelengths. This is supported by the relatively flat spectral refractive index shape at these wavelengths indicated in Figure S2."

p8128: You could write ' ... in two different ways, resulting in different sizing metrics.' This would help the readers in the subsequent paragraph.

This has been changed to, "Thus to investigate some of these uncertainties, the 2DS data was processed in two different ways, using two different sizing metrics."

p913f.: Were the particles rotated such that one dimension is minimized and the other maximized? Or were x and y measured for each imaged particle without such rotation?

No, the particles are not rotated. We added the sentence, "The x and y dimensions are measured along the probe array, i.e. the particle is not rotated to minimize or maximize either dimension."

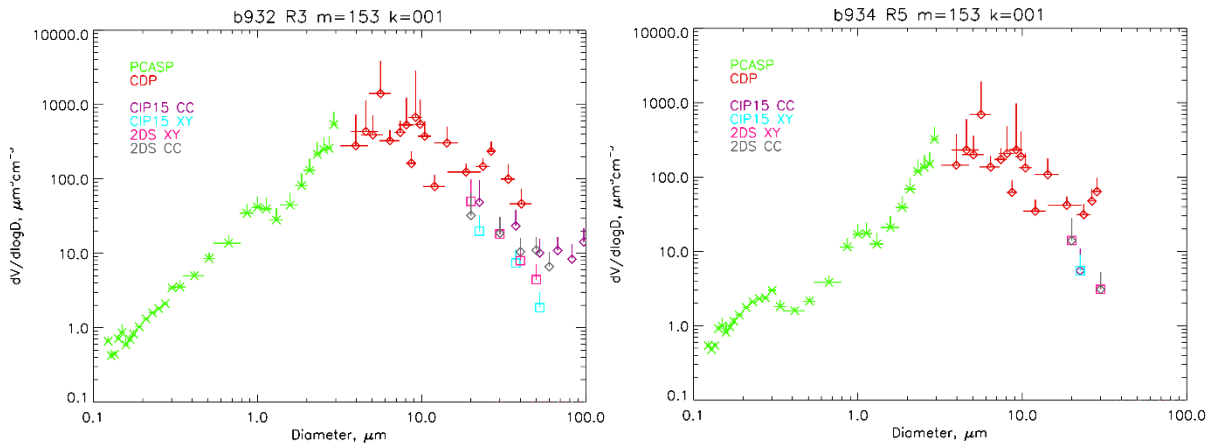
p916: '... though diameters will be lower than an area-equivalent diameter for example, if the particle is an ellipse.' looks wrong. For example, assume $x=1$ and $y=2$. Then $D_{XY}=(x+y)/2=1.5$. The area-equivalent diameter however is $D_{area}=(1*2)^{0.5}=1.414$ which is smaller than 1.5. This could be a reason why also the mean XY method somewhat overestimates the 'real' particle size.

This was a typo, 'lower' should have been 'larger.' We have added and changed this paragraph as follows: "The mean XY method is considered to give a more representative diameter for non-spherical particles than the CC metric. If the particle image is an ellipse, the mean XY diameter will be larger than an area-equivalent diameter, as used by the filter sample data. However, the OAP images capture 2-D image projections of the particles in their atmospheric orientation, while the filter samples are will be collected with their largest surface lying parallel to the filter sample, and therefore may be oversized in this context."

p918: I wonder if there is a reason why you don't use the mean XY method (instead of the CC method) for the CIP15 in this discussion paper?

This is a good point, and we would have done so initially in an ideal world. Unfortunately much of the OAP processing was initially done by different institutions for each OAP, and used institutional conventions, often selected for consistency between different fieldwork campaigns and optimized for ice/cloud particles, which allowed the whole of the ICE-D data to be processed with one assumption (over half the ICE-D flights were cloud flights).

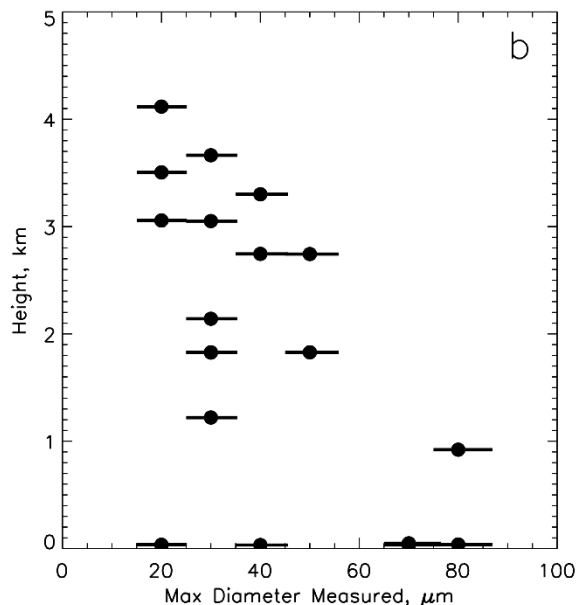
However, we have now run additional processing on the CIP15 data using a centre-in, mean XY metric, although unfortunately it has not been possible to do this for all the AER-D flights analysed in this article. Two contrasting examples are shown below.



It is clear that the biggest difference in the OAP size range stems from the choice of size metric (XY vs CC), rather than the instrument (2DS vs CIP15). The difference is not noticeable when the giant mode is smaller (right hand panel). The difference in mean d_{eff} from all SLRs analysed between the 2DS XY and the CIP15 XY is under 1%. Differences for the CIP15 CC are 4.5%, and for the 2DS XY under 1%. These uncertainties, and particularly that for the XY metric, are smaller than the 5% error already applied due to the uncertainty due to choice of refractive index for the OPCs.

However, the impact of OAP metric and instrument on d_{max} is larger. Differences from the 2DS XY d_{max} to CIP15 XY d_{max} are +6%, to the CIP15 CC +37%, and to the 2DS CC +21%. The upper error of 6% (based on the instrumental differences using the more realistic XY metric) is now incorporated in the upper error bars on d_{max} shown in Figure 8b. These are small relative to the existing error of 5 μm when d_{max} is around 20 μm , but comparable to the error when d_{max} is larger, and can be seen for example in the now asymmetric error bar for the points at $d_{\text{max}} = 80 \mu\text{m}$.

New Figure 8b:



The following text has been added to section 2.3.2, “Some CIP15 AER-D data were also processed using a centre-in, mean XY metric, but unfortunately it was not possible to process data for all the SLRs with this method. Therefore this data was used to inform on instrumental differences between the 2DS and CIP15 when processed with the same size metric (XY mean). It was found that the impact on the full PSD was very small

(d_{eff} differed by under 1%), but that d_{max} was up to 6% larger with the CIP15 XY compared to the 2DS XY. The upper uncertainty of 6% in d_{max} was therefore propagated in combination with the other uncertainties in d_{max} .”

p9l12: Which flight leg length do these 10^{-5} cm^{-3} correspond to, approximately?

This number concentration corresponds to a flight leg length of 132 km or approximately 20 mins of flight time on the FAAM aircraft. This information has been included.

p9l20: It is unclear what ‘... errors due to bin size from ...’ means. Please reformulate this sentence.

This sentence has been deleted.

p9l28f.: ‘as expected when the particles are non-spherical (section 3.2)’ could be replaced by ‘as expected for dust’.

This sentence has been re-worded.

p10l9f.: This looks quite similar to p9l10ff. Maybe you could bring both together or at least refer here to the previous text (e.g. ‘Therefore, we remove, as mentioned, cases where fewer ...’).

Done

p10l17: ‘of around 0.2 to 1 m’ should be replaced by ‘smaller than 2-3m’ when considering Fig S1.

Done

p11l5: ‘R2’, ‘R5’ are not defined.

These are now explained by the additions to the ‘Flight Patterns’ section.

p11l15: The areas of the ellipses were larger than the areas of the particle 2D projections because you used circumscribed ellipses. How large is this difference? Why didn’t you use the area of the projection itself to determine the area-equivalent diameter?

We did not use image contrast and brightness levels with automatic processing since this would have meant that not all particles would be detected, since not all particles had similar contrast and brightness. This technique would have allowed measurement of projected area. Instead images were analysed manually by circumscribing an ellipse to the particles. This method was selected in order to maximise the number of particles analysed, and as it is operator independent, systematic and reproducible. However, the particles were not notably jagged at their edges.

Secondly, p11 L17-19, (“Note that this technique may oversize the particle volume, particularly where the shape is a platy silicate with a tendency to fall with its largest surface parallel to the substrate; e.g. Chou et al. (2008) found the height of dust particles examined under SEM to be around one third of their major axis length.”) is an *additional* process which may lead to the filters PSD being oversized compared to the wing probes, which relates to the orientation in which plate-like particles fall flat on a filter substrate.

This paragraph now reads, “It was not possible to use automated image contrast to calculate projected particle area because of a high degree of variability in particle contrast. Our filters sizing this technique may oversize the particle size for two reasons. Firstly, the area of a fitted ellipse may be larger than a projected particle area, though the particles were not noticeably jagged around their edges. Secondly, our method may oversize particle volume where the shape is a platy silicate and has a tendency to fall with its largest surface parallel to the substrate. For example, Chou et al. (2008) found the height of dust particles examined under SEM to be around one third of their major axis length.”

p12l29: Shouldn’t ‘0.0001’ be replaced by ‘0.0005’? Otherwise I don’t understand this description.

This has been rewritten to better explain, and now reads, “a Mie scattering code is used to generate optical properties at 550 nm, using the ACC PSD, with refractive index of $m^{550}=1.53$ and k^{550} incrementing in steps of 0.0005 from 0.0005 to 0.006, but with an additional smallest value of 0.0001 which was required for the MBL SLRs.”

p13l1: ‘Figure 8b’ shows something else.

This sentence has been deleted. The correct figure is referred to in the next sentence (Figure 3).

p13l9: ‘between 0.0015i to 0.0025i’ doesn’t fit to the mode value of 0.001.

Thank you for pointing this out, there was a typo and it now reads ‘0.0005 to 0.0025.’

p13l11: Why do you use ‘volume fraction’ here and ‘number fraction’ for the external mixing case (p13l30)? How big is the difference between both cases?

Volume fraction is appropriate for the internal mixing assumption since the volume of each mineral determines its contribution to the total refractive index. For the external calculations, our scattering code requires input in the form of number concentration. Internally, the code calculates the surface area, since this determines the aerosol scattering properties. If the internal mixing assumption is used, but with number fraction, the results are very different because the smallest sized particles dominate the number concentrations, and have a greater contribution from different compositions such as sulfate and salt (as shown in Figure 11).

p15l16: I suggest to briefly discuss the difference from Marengo et al., who find maximum AODs of 2.0.

We have added, “Note that lidar-derived AODs and extinction shown by Marengo et al. (2018) are slightly lower than those shown here, which may be due to different extinction properties of the dust at the lidar wavelength of 355 nm, and the Rosemount inlet enhancement effects shown in Figure S2, or the differences between a lidar curtain and sloped aircraft in-situ profile.”

p15l28: ‘each flight leg’: As Fig. 5 shows only a single leg inside the SAL for each flight I assume that there was only one flight leg in the SAL for each flight? However, then Fig. 2 and 5 seem to not fit to each other as mentioned in a separate point below. In general, the legs should be described better.

This is not the case. Successive flights included 2, 1, 6, 2 and 3 legs in the SAL. Much of the data overlies itself for some parts of Figure 5b so that it might be inferred that fewer legs were performed. We do not intend for the reader to be able to distinguish each individual flight leg from panel a, and the points discussed in the text can be seen. We have added information to the caption of Figure 5 to state how many SLRs are shown. With the addition of information to the ‘Flight Patterns’ section, the legs should be more understandable.

p15l33: I do not really understand this sentence. Would the absence or presence of the coarse mode not always have an effect on the overall shape of the size distribution?

Here we intended to explain that during AER-D, when dust concentrations were higher, the coarse and giant mode particle concentrations increased together, as well as the concentration of particles in the accumulation mode, which meant that the overall shape of the PSD remained the same. This contrasts to Fennec, where coarse and giant particles often increased in concentration but without the smaller particle concentration increasing. In that case, the shape of the size distribution *did* change. A few changes and additions have been made to this paragraph to state this more clearly, and also in the conclusion.

p16l1: ‘peak volume concentration’ is unclear. I suggest to write ‘The peak of the volume distribution during ...’.

Done

p16l8f: ‘Figure 5b; green, orange and red’

Changed

p17l13: ' may be aligned horizontally in the atmosphere ... ': I suggest to add here a reference to Ulanowski et al. (2007), DOI:10.5194/acp-7-6161-2007, who made some simulations on this topic (see e.g. Fig. 9 of that paper).

Done

p18l10: How does 'Particles sized over 20 m diameter were detected in 100%' fit to Fig. 8b which shows that there are cases with $D_{\max}=20\text{m}$?

This has been changed to '20 um or larger.'

p18l14: 'Figure 8c' does not exist.

Changed to Figure 8b

p19l4: particles

Changed

p19l10: 4643 has probably too many significant digits. I suggest 'around 4600'.

Done

p19l12: 'decreases' is maybe the wrong word here. I suggest 'is lower'.

Now reads, "is around a factor of ten lower than the total mass"

p19l15: 'PM2.5' is not defined. As it is used nowhere else, I suggest to just write 'the accumulation mode'.

Done

p20l16: 'as they only include iron when detectable as single-iron particles': As far as I understand this sentence, 'they' should be replaced by 'we' and 'detectable as single iron particles' by something like 'iron was the dominant component of a particle'.

Done

p22l17: There is one 'is' too much.

Changed

p22l28: 'coarse mode present' could be replaced by 'coarse particles'.

Done

p22l29: 'so' could be removed.

Done

p23l1f: The sentence could be improved by removing 'same' and adding 'also' after 'mode'.

We thank the reviewer for this suggestion. After further consideration, we think that the sentence sounds better as it was and prefer to leave this unchanged.

p23l4: 'as dominate' is unclear.

'as' changed to 'which'

p23l7: 'RI' is not defined.

Now defined in Section 2.3.2, first occurrence of refractive index.

p23l10: Suggestion: 'The variability of the optical properties of dust in the SAL is probably mainly determined by ...'.

The wording here has been changed.

p23l14: 'the variability of the' should be inserted before 'optical'.

Done

p23l26: I suggest to write 'the variation of the SSA as function of composition, represented by k_{550} ', because this order is more logical and also better fits to Fig. 13 considering that the vertical axis usually shows the dependent variable ($y=f(x)$).

Done.

p24l1: 'optical property' could be replaced by 'SSA' to be more specific.

Done

p24l13ff: It is not clear how this fits to p13l29f where you write that you use the same size distribution (only number-weighted) for all components. In addition, you could consider a size dependency not only in case of external mixtures but also in case of internal mixtures.

Regarding number-weighting – see response to point above (p13l11).

Actually, we have calculated the size dependency of SSA for both the internal and external mixing cases, but perhaps this was not clearly described. These points can be seen as the large diamonds in Figure 13b. We have added a second panel to figure 14 to show the same values but for the full PSD, for the iterated RI, internal mixing RI and external mixing RI, and now discuss this in the text.

p24l21: 'In contrast to Fennec observations of the full PSD and associated optical properties over the Sahara,' could be removed. Maybe the information about the location could be added somewhere on line 22.

Done

p24l30: I suggest to delete 'during August 2015' and to add instead a new sentence like 'The flights were performed in August 2015 between Cape Verde and the Canary Islands.'.

Done

p25l7: 'to be' could be removed.

We prefer to leave the wording here.

p25l14: 'Deff for the SAL the mean (minimum, maximum) was' should be reformulated.

Done

p25l26: 'giant MBL mode particles' could be reformulated.

'MBL mode' has been deleted.

p25l30: calculate

Changed

p26l6: 'slightly lower' is an understatement because the 'base value' of the aspect ratio is 1.0. Then your value is only about half of the literature value.

We have deleted 'slightly.'

p26l6: 'and quartz' could be removed when considering Fig. 11.

We changed the wording to, “alumino-silicate particles dominated the composition at sizes above 0.5 μm , followed by quartz.”

p26l28: ‘was extremely scattering’ should be replaced by ‘was only very weakly absorbing’.

These two statements are essentially the same, therefore we leave the wording as it is.

p26l33: I suggest to start a new sentence after ‘dust’ and to write ‘Particles larger than expected from sedimentation processes alone are found.’.

Done

Table 1: The reference style is not consistent.

Changed

Table 1: During ‘SALTRACE’ also the ‘CAS-DPOL’ instrument was used, measuring up to 50m (Weinzierl et al., 2017).

This has been corrected.

Table 2: The ‘General Flight Aims and Conditions’ do not very well fit to Table 1 of Marengo et al. Furthermore, you write ‘b923’, and Marengo et al. ‘B923’. Maybe this could be more harmonized between both papers.

We have added ‘CATS underflight’ to Table 2 to b920, and changed ‘SAVEX’ to ‘SAVEX-D’ to align better with Marengo et al. Table 1. Other than this the details reflect the same information. We use ‘b’ lowercase as this is the official terminology used by FAAM for flight numbers, and will consider adjusting the text in Marengo et al.

Table 4: Negative latitude values don’t make sense here.

These were errors and have been corrected.

Table 4: Longitude and latitude values for b923/b924 do not fit to Fig. 1.

These have been corrected.

Table 6: Sometimes you write ‘D_{eff}’ and sometimes ‘d_{eff}’.

Corrected to d_{eff}.

Table 6: With ‘derived RI’ you mean the refractive index you iterated to fit the optical measurements? If yes, ‘iterated RI’ would be more specific.

Changed

Table 7: What means ‘assuming internal mixing’ here? In my understanding, the mixing state (internal/external mix) is only relevant for optical calculations but not for the derivation of the refractive index from filter samples.

In order to calculate the refractive index from the filter sample composition, only the internal mixing assumption can be used. In the external mixing assumption, the calculations step straight from the refractive index of the individual mineral components to their optical properties, and then to the optical properties of the total aerosol sample. They do not provide a refractive index for the total aerosol sample as an intermediate step. This is why the external mixing case cannot be shown on Figure 13b.

Figure 4: What is ‘SLRs’?

As stated above, we now explain SLR in Section 2.1, ‘Flight Patterns,’ and use SLR throughout the manuscript instead of ‘flight leg.’

Figure 5a: The $dV/d\log D$ value for b924 (green) at the largest three size bins is more than ten times higher than the corresponding average value shown in Fig. 2. However, there are only five flights and SAL flight lags. How do these figures relate? How did you calculate the average in Fig. 2? See also my comment on p15128.

As stated above, there are 14 flight legs shown in Figure 5a. Thus the weighting towards the b924 data is much less than 1 in 5. Please see response to reviewer 1 for the details of creating the campaign SAL average shown in Figure 2 and error processing, and associated text added to the manuscript.

Figure 6: You write '6(a)' while there is no '6(b)'.

a has been deleted

Figure 7: The dashed lines are not very well visualized and the description is missing in the legend (at a reference to p17112 should be added).

We have added this to the legend, and plotted the dashed line with open circles to be clearer.

Figure 9: 'Aspect ratios histograms as a function of number fraction of particles' is not clear. You mean 'number fraction of particles as function of aspect ratio'?

Yes, we have changed this as suggested.

Figure 10c: It looks like there is a height dependence of the fraction of $D > 5\text{m}$ particles within the SAL. I think this height dependence should be briefly discussed in Section 3.3.

Thank you for this suggestion. We have added the following to this section:

"Additionally there appears to be a trend with altitude shown in Figure 10c: the mean mass fraction at $d > 5\ \mu\text{m}$ decreases steadily from 0.75 at the surface to 0.23 at 5 km altitude. A decrease is also evident in panel d with the largest fractions being found towards the bottom of the SAL (excluding the MBL). These decreases with dust mass as a function of altitude are somewhat in contrast to the homogeneous distribution of dust size throughout the SAL shown in Figure 8. This may be due to the data shown in Figure 10c coming from profiles rather than SLRs, such that more data is available, and also that although d_{eff} represents the full size distribution, as such it is relatively insensitive to smaller changes in the coarse and giant particle concentration. Either way, there is clearly evidence of coarser dust particles being more prevalent towards the bottom of the SAL (and also in the MBL), indicating deposition processes occurring."

Figure 11: Relative 'n particles' for '10.0 to 40.0' shows no big difference between 'b920 R2' and 'b928 R2', so it is a bit unclear why you mention here 'B928 R2 (top right) contained giant mode MBL particles.' which should also be true for b920.

We refer to the wing probe measurements which show the substantial giant mode in the MBL for b928 R2 but not for b920 R2. This is now clarified in the caption.

Figure 11: The last sentence could be 'Errorbars are counting uncertainties.'

Changed.

Figure 13: 'size-specific RI is used': Is this explained in the main text?

The text has been changed to be clearer here.