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Atmospheric Measurement Techniques Discussions

Interactive comment on "Field comparison of dry deposition samplers for collection of atmospheric mineral dust: results from single-particle characterization" *by* Andebo Waza et al.

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Dear Referee #1, We thank you for the critical comments and suggestions to improve the manuscript (MS). We have considered the comments and modified the MS accordingly. Our detailed responses to the comments are given below. General comments: Referee's comment: One of the main problems of the manuscript is that the shown data is not well explained. It is not obvious for the reader understand how the data in each plot has been calculated. Sometimes, this information can be inferred from reading carefully the caption and the references to the ïňĄgure in the text but this is not always the case and it makes it difïňĄcult to read the manuscript. See speciïňĄc

comments. In addition, the manuscript presents a large amount of data in different ïňĄgures and tables but in some occasions, the discussion of these data is too short. Authors' response: Major corrections in the revised MS are made. Moreover, answers given to specific comments can be seen in the specific comments section. Referee's comment: Lack of consistency. The magnitudes and concepts that appear through the text are mentioned in different ways, which makes the reading process very confusing. There are many other inconsistencies, such as the fact that some multi panels are not properly labelled using letters. See speciïňĄc comments. In addition, there are many formatting issues. Some of them are pointed in the speciïňĄc comments. Authors' response: Major corrections in the revised MS are made. Moreover, answers given to specific comments can be seen in the specific comments section. Referee's comment: The geometry and computational ïňĆuid dynamics analysis of 3 passive samplers is given in the section 3. However, there are no references of the BSNE sampler in this section, while in the other sections, the four passive samplers have been mentioned. The geometry and computational ïňĆuid analysis of this fourth sampler should be included or at least justify its absence. Authors' response: Because of resource limitation, we did the CFD analysis only for the three geometries (samplers) and therefore the BSNE was not included in the CFD analysis. Again, answer this specific referees' comment is given in the specific comment section. Referee's comment: Many comparisons are presented all over the manuscript, but it seems that a signiïňĄcant fraction of the data hasn't been plotted and they appear instead in tables in the SI. I suggest to plot all the data that appears in tables in the SI. Some of the given conclusions regarding to the agreement or disagreement of data need to be revised. See speciïňĄc comments. Authors' response: We have now plotted for the whole campaign data showing comparison among samplers. Answers are given in the specific comment section. Referee's comment: In this manuscript many comparisons in between different instruments are presented. Were the sampling times of each instrument over lapping in all the cases? This remains unexplained, and it seems very unlikely in some occasions, instruments were ran with very different times (24h data compared with 1h data). See speciïňĄc

comments. Authors' response: The reason why we set up different time interval is because one category of the sampler is operating passively while the other ones (the FWI and the Filter samplers) operate actively. The active ones have a much higher collection velocity. Therefore, we cannot set up the same time interval for both type of samplers, as this would result in either overloading of the active or underloading of one of the passive samplers. A further explanation is given under specific comments. However, based on the PM10 values recorded continuously, we have compared the active sampler interval with the passive samples one and found, that the average PM10 values of both intervals differ by 0.2 %. Therefore, we believe that the comparison is justified. Referee's comment: Regarding to the SEM analysis, were handling blanks taken during the campaign and then analysed under the SEM? In addition, did you test if the particles homogenously distributed over the sampling substrate? If not, this might signiïňĄcantly affect the measurements. Authors' response: Blank samples were analyzed. The contamination is small for the dust compounds (factor of 30-100 lower than the deposited particle numbers). A low density of pure iron particles is present, apparently already from the manufacturing process. These particles are identified by their chemical composition and removed from the dataset.

SpeciïňĄc comments: Referee's comment 1: Line 17. "This study focuses on the microphysical properties". This is too vague. Authors' response: A sentence is added in the revised MS to explain as clearly as possible. Referee's comment 2: Line 19-32. This paragraph of the abstract looks more like a collection of statements that are made through the paper rather than a paper abstract. Authors' response: Major corrections in the revised MS are made on abstract part. Referee's comment 3: Line 20. Acronyms in the abstract have not been deïňĄned before. Authors' response: Correction is made. Referee's comment 4: Line 26-28. Acronyms deïňĄned after they appear for the ïňĄrst time. Authors' response: Correction is made. Referee's comment 5: Line 97-98. What about the sampling time of the Flat plate sampler? Were the ïňĄlters ran for one hour or 24 with the passive samplers? Authors' response: The sampling time for all passive sampler including the Flat plate sampler was set to be 24 hours. The filters ran for one

hour. Referee's comment 6: Line 149. This section needs a bit more of detail. Authors' response: More detail on the samples construction and principle is added. Referee's comment 7: Line 159. The acronym SMPS hasn't been deïňĄned. In addition, there are no other references to the SMPS in the main text. Was the data used for this work? Authors' response: We used data only from OPC. So, corrections in the revised MS are made. Referee's comment 8: Line 164. How were the samples transported and stored? Authors' response: All samples were stored in standard SEM storage boxes (Ted Pella Inc, Redding, CA, USA) in dry conditions at room temperature. Referee's comment 9: Line 170. "Randomly selected areas". Were they randomly generated or were they selected manually by the user? Authors' response: First, the user orients the microscope the circular deposition area and then the microscope selects smaller sub-areas randomly. Referee's comment 10: Line 194 and 222. Was the temperature dependence considered in the density and dynamic viscosity choice? Authors' response: We have used constant values for density and dynamic viscosity. It is already mentioned in the MS. Referee's comment 11: Line 258. I think this section needs to be better explained and describe why and how different models were applied to different samplers Authors' response: A more detailed explanation is added in section 2.9 (line 253-255 and line 258-259). Referee's comment 12: Line 423. Which was the fraction of mineral dust in the samples? Was it dominating all the sizes? Were the non-mineral dust particles excluded from the calculations? Authors' response: We found that the fraction of mineral dust in all samples were dominating in all size ranges (96 %) and therefore in calculation, we assumed the fraction of non-dust particles to be negligible. Referee's comment 13: Line 430. In the mentioned tables, the size distribution for each collected sample is given in both mass ïňĆux and number ïňĆux. Why has it been described as "Minimum, Maximum and Median Mass Flux (mg/(m2d)) measured by..." in the captions of the table S1, S2, S3, S4, S5 and S6? Authors' response: In the captions of the table S1, S2, S3, S4, S5 and S6, the unit '(mg/(m2d))' was used for mass flux (mass deposition rate) while the unit '1/(m2d)' was used for number flux (number deposition rate). Corrections are made in the revised electronic supplement.

Referee's comment 14: Line 431. Has all the data in this section been calculated with the SEM? If so indicate. It would be useful to also indicate it in the ïňĄgure captions. Authors' response: All mass flux data in the section 4.1.1 is calculated with SEM. A sentence is added in the revised MS to make the information more clear. A sentence is added in the caption too. Referee's comment 15: Line 435. In this section, the terms "deposition ïňĆux" and "mass ïňĆux" seem to be used to refer to the same magnitude. If this is the case, use only one notation, and mention alternative notations when the magnitude is introduced ïňĄrst. Authors' response: Changed to mass deposition rate in the revised MS. Referee's comment 16: Line449. "we can clearly see that that there is high temporal variation in deposition ïňĆux between dust event days and non-dust event days". Fig. 9 doesn't clearly show this. There is a signiïňĄcant difference for the MWAC sampler, but for the other 3 instruments, the difference doesn't seem "high" for the ïňĄrst four bins (up to a factor 2-3?). This is difiňĄcult to see since there are not minor ticks in the y-axis. I suggest to add minor ticks and lines as for the x-axis, as well as softening the statement and explaining better the difference in between the deposition ïňĆux during a dust event and a non-dust event. Authors' response: The plot is modified in the revised MS. And the statement is modified according to the referee's comment. Referee's comment 17: Line450. Line 450. "Generally, the temporal variation is much higher than difference between samplers". This statement seems a bit weak for the reasons mentioned previously (Line 449). In addition, all the data in the tables S1, S2, S3 and S4 hasn't been plotted so, it is difiňĄcult to see if these argument is valid for all the data. I think this should be improved by adding more graphs (maybe in the SI) or doing some systematic statistical analysis. Improve this. Authors' response: a box plot showing temporal variation of size distribution is added to the revised Manuscript. Referee's comment 18: Line 451. As mentioned before, I suggest to add some y-axis minor ticks or plot it again in a way that allows the reader to understand the differences in the mass ïňĆuxes. This has been done for most of the other ïňĄgures of the manuscript. Reducing the range y-axis range to 10-1 to 104 mg/(m2d) (there is no data at all in the 10-4 to 10-1 mg/(m2d) range) could help to better appre-

ciate the differences between the different curves. Also, explain why some large size bins have been removed (is it due to a small number of particles in those bins?). Authors' response: The graph is corrected in revised MS. Regarding the last data point, there was not data actively removed. Although particles across all size ranges (up to approximately 100 μm) can be deposited on the passive samplers, in our analysis we did generally not find particles larger than 64 μm diameter. When the last data point is missing from the plots, no particle between 32 and 64 μm was detected. Referee's comment 19: Line 456-485. In this section, the ratios in between magnitudes obtained with four different instruments have been compared. Why has the Sigma-2 instrument been used as the reference instrument? This section doesn't compare the other instruments within themselves at all. Why? I suggest to add some information about how the other instruments compare to each other or justify why this comparison has been omitted. Authors' response: The sampler Sigma-2 has been widely used for deposition sampling and therefore, in this work it is used as reference. A comparison showing other samplers as reference is added (see electronic supplement). Referee's comment 20: Line 473. Having a legend in order to identify the different days could help to understand or discuss why the ratios change that much from one day to each other. Why is the ratio in between the Flat plate and the Sigma-2 of the cyan blue day that low when compared with other days? Authors' response: A legend is already added to different measurement days to identify the different days and can be seen in the revised MS. The flux ratio of Flat plate to Sigma-2 of the cyan blue day (July 29, 2017) is low when compared with other days. The low value of the deposition rate observed with flat plate for this particular day cannot be explained by other observations, so it has to be considered as an artifact. Therefore, we show the data, but we do not take it into account for further discussion. Referee's comment 21: Line 474-485. It is very difiňĄcult to follow what has been plotted in Fig. 12. Is the blue data the mean ratio between each sampler and the Sigma-2 (same ratios as in the previous section but using number instead of mass)? Has the BSNE deposition velocity ratio modelled data been obtained with the Piskunov model as stated in line 259? They-axis is labelled as deposition ve-

locity ratio, however, ratios of dry deposition ïňĆux has been plotted as well. Are these ratios equivalent as one would expect from the equation 7? In general, I think that this ïňĄgure and what has been plotted in it needs to be much better explained than it is now. Authors' response: Generally, Fig. 12 shows a comparison of velocity ratios of sampler A to Sampler B obtained from flux measurement to the velocity ratio obtained from different classical deposition velocity models. The ratio of flux measured by one sampler to flux measured by another sampler is equal to the velocity ratio of the two sampler. In the Fig. 12., the blue data shows the velocity ratio obtained from flux measurement while the red one shows the velocity ratios obtained from different deposition velocity models. A table showing different deposition velocity models used for different samplers is added in revised MS in section 2.9.3 (see Table 1). The flux ratios plotted in the Fig. 10 and 11 is meant to show the relative collection efficiencies of different sampler with respect to reference sampler (Sigma-2). The paragraph has been rewritten to clarify the type of display. Referee's comment 22: Line 475. "The deposition velocity ratio from models is often higher than the ratios derived from the mass and number". Is this something that happens in general and has been reported in other studies or does it only happen here? In the ïňĄrst case, add some references. Authors' response: It is true that the deposition velocity ratio from models is higher than the ratios derived from the mass and number flux. We are not aware of any other studies that have been done on the subject. However, as this is only a relative display, there cannot be any 'truth' (most accurate sampler) derived. The higher ration can mean an underestimation of the Sima-2 deposition velocity, or an overestimation of the others. This is stated now in the manuscript. Reviewer comment: Line 487. What has exactly been plotted in Fig. S2? It is not obvious from the description. Explain this properly. The main purpose of section 4.1.2 to investigate the driving force of atmospheric deposition rate. As already stated in section 4.1.2, Figure S 2 (now Figure S 10 in the revised electronic supplement) displays the correlation between deposition number fluxes (measured by flat plate sampler) and atmospheric number concentration by the OPC. An extended caption has been added to the figure. Referee's comment 23: Line 489-491. The anti-

correlation reported by the authors in the number ïňĆux-wind speed data cannot be seen in Fig. S2. Remove this or justify based in some quantitative statistical analysis. Authors' response: It is correct, there is not significant correlation for wind speed in Fig. S2 (S10). Justification based on quantitative statistical analysis is added in the revised MS to make clear. In addition to the Fig. S2 (now Figure S 10 in the revised electronic supplement), a quantitative statistical analysis was already shown by Table 2 (now table 3). Referee's comment 24: Line 496. Again, two different notations for the dust deposition ïňĆux have been used. How is this data related to Fig. S2? Authors' response: Correction was made on notations for dust deposition flux. Generally, Fig. S2 and table 2 (now Table 3) shows the dependence of small particle dust deposition flux on atmospheric PM10 concentration and wind speed. While Fig S2 (now Figure S 10 in the revised electronic supplement) shows the correlation between flux, dust concentration, and wind speed for samples measured by flat plate sampler, table 2 (now Table 3 in the revised MS) (Line 496) shows the same relation using quantitative statistical analysis for all samplers (Flat plate, MWAC, BSNE, Sigma-2). Referee's comment 25: Line 499. What has it been shown in the table 2? From the caption, the reader can understand than dust deposition ïňĆux (probably SEM measured) has been correlated to the external measurements of OPC particle number and wind speed. However, in the line 499 the authors suggest that the data in table 2 is a comparison in between the OPC measured concentration and the modelled concentration (using the models on the SEM ïňĆux data to obtain this concentrations?). This section is very confusing and unclear and it needs to be much better explained. Authors' response: Line 499-500 in section 4.1.2 should not have referred to Table 2 (now table 3 in the revised MS). The authors replaced this table with Table S7. Table S7 shows a quantitative statistical analysis for correlation between the OPC measured concentration and the modelled concentration (using deposition velocity models on the SEM ïňĆux). In addition, the paragraph has been rewritten to clarify the approach. Referee's comment 26: Line503. Do you mean from the correlations inTable2? If so indicate it. In the description of the table it says that the ïňĆux was correlated with OPC number concentration, but here

the authors mention here PM10. Do you mean number concentration below 10 μm? Authors' response: In line 503, the correlations refer to Table 2 (now table 3 in the revised MS). The table shows the correlation between ïňĆux OPC number concentration in PM10 size range. A description is corrected on the table 2 (now table 3 in the revised MS). And also the size range is explicitly stated now at the beginning of section 4.1.2. Referee's comment 27: Line 513. How have you plotted the wind speed? Did you divide each day in 30-minute interval averages and then calculated the mean and standard deviation from this data (I guess 48 points per day)? Explain it in the ïňĄgure caption. Authors' response: A 30-min averaged wind speed data was obtained by dividing each day data in 30-minute interval averages and then the mean and standard deviation was calculated from this data. An explanation was added into the ïňĄgure caption in the revised MS. Referee's comment 28: Line 514. What are the blue boxes showing? Is it the 25 and 75 percentiles? Are the black vertical lines showing only one standard deviation? Authors' response: Yes. On each blue box, the central mark is the median, the edges of the box are the 25th and 75th percentiles. The black vertical lines show one standard deviation. An explanation was added into the ïňĄgure caption in the revised MS. Referee's comment 29: Line 516. "Small particle". Is this a common notation in dust deposition studies to refer to the 1-10 μm size range? Authors' response: "Small particle" notation was used to refer to PM10 size range. Accordingly, the title of section 4.1.2.1 is changed to "Size-resolved apparent deposition velocity in the PM10 size range" in the revised MS. Referee's comment 30: Line 520-521. "The effect of wind speed on deposition velocity is negligible". Why? Authors' response: As already indicated by Table 2, there is not significant correlation between the wind speed and the observed deposition rate. While this could be still a second order effect of an anticorrelation between atmospheric concentration and wind speed, Fig. 14 shows clearly, that there is not wind speed effect for the smaller particles. While this is in contradiction to the models, one has to keep in mind that the (a) the observed wind speeds are comparatively low here, and (b) the considered size range is not the most affected. An effect of the wind speed might be much stronger at higher wind speed

and for larger particles. An according statement is added to the manuscript. Referee's comment 31: Line 522. In the text, the apparent deposition velocity concept has been introduced as the ratio of the number ïňĆux to number concentration. I suggest to use deposition velocity in the y-axis label. Authors' response: the y-axis label is changed to deposition velocity in the revised MS. Referee's comment 32: Line 528-529. "Mass concentrations calculated from different passive samplers agree generally well with respect to the statistical uncertainties". This agreement is not fully true for the July 28 and August 21 cases shown in Fig S1. Why? Authors' response: The agreement generally holds true with the respect to the mean value of the campaign. And yes, it is correct that agreement might not be true in single cases. Referee's comment 33: Line 532. Isn't the mas ïňĆux example given here the same as in Fig. 9a but with a different y-axis scale? If so, choose another example. Another idea would be removing the whole section and discussing the consistency between samples in a previous section. Authors' response: The authors are aware of the case that mas ïňĆux examples given in section 4.1.3.1 (Fig. 15) and the one in section 4.1.1 (Fig. 9a) are the same, but they do have different message. The message of the Fig. 9a is to show the mass flux measured during dust event day differs from the one measured during non-dust event days (Fig. 9b). The other message of Fig 9a is to show the variation in mass flux measured by different passive samplers (for the same measurement day). The purpose of Fig. 15 is to show the consistence in concentration obtained from flux measurement for different samplers and to show that different deposition velocity models selected for the samplers are generally suitable, despite the deviations in single cases. In addition, more one more day is added to Fig. 15 in the revised MS. Referee's comment 34: Line 532. Why does the max ïňĆux data measured by the MWAC differ so much from the others but when converting it to mass concentration it agrees with them? The deposition velocity has been calculated with the same model for the MWAC, BSNE and Flat plate. Authors' response: This seems to be a misunderstanding. MWAC is calculated with the different velocity model (shown in Table 2 now). Therefore, the model the observed differences in deposition rate to a similar range concentration comparatively

well. Deposition velocity used for different samplers is explicitly indicated in section 2.9.3 (see table 1 in the revised MS). Referee's comment 35: Line 532. What is "impaction curve & Piskunov" in the legend? The concept of "impaction curve" hasn't been mentioned before. Authors' response: The impaction curve was briefly introduced in the method section 2.9.3. To clarify, section 2.9.3 has been reworked and Table 1 added. Referee's comment 36: Line 540. How were the number size distributions calculated from the ïňĆux measurements? This should be better explained here or in the caption. Authors' response: To get the number concentration size distributions, first the number flux (#/(m2day)) measured by different samplers is obtained from SEM. Then, the SEM number flux is converted in to number concentration by using different deposition velocity models. An explanation on how number concentration size distribution is calculated is added in the caption in the revised MS. Referee's comment 37: Line 544. Why have these speciïňĄc samples (and these speciïňĄc instruments) were chosen as an example I assume there are lots of potential comparisons (you sampled during many days with four different instruments). How do other samples taken in other days and/or with other instruments compare the OPC measurements? It seems too arbitrary to show only 4 comparisons out of many and extract some generalist conclusions. Authors' response: More samples (representing dust event days and non-dust days) are added (see revised electronic supplement). These specific samples shown in the figure (in the MS) are exemplary and they represent a particular dust event day. However, more comparison involving this section can be obtained in the electronic supplement (randomly selected from dust event day and non-dust event day from all samplers. Referee's comment 38: Line 545. This caption needs to be rewritten in a more clear way. Were the SEM obtained mass ïňĆux distribution converted into mass size distributions using the different approaches and then transformed into number size distributions using a density value? Authors' response: An explanation was given in the previous referee's comment (Referee's comment 36: Line 544). Caption was changed accordingly. Referee's comment 39: Line 550. The Momentum ïňĆux approach data looks black not green Authors' response: 'The Momentum ïňĆux approach' data is

changed from green to black in the revised MS. Referee's comment 40: Line 555. "the above ïňĄgure (Figure 16)" should be referred as Figure 16 or Fig. 16. Authors' response: "the above ïňĄgure (Figure 16)" is changed to Figure 16 in the revised MS. Referee's comment 41: Line 555-560. "also show the comparison of the mass concentration size distribution measurement". Fig. 16 doesn't show any mass size distribution. Please correct or explain this. Authors' response: In "also show the comparison of the mass concentration size distribution measurement" sentence, 'mass concentration' is replaced by, 'number concentration' in the revised MS. Referee's comment 42: Line 563. In order to calculate the mass concentration measured by each sampler, don't you have to use the SEM obtained mass ïňĆux measurement and assume one of the mentioned models? You haven't mentioned yet a direct method to measure mass concentrations from the passive samplers. Authors' response: This is correct; we have added a clarification to the caption in the revised MS. Referee's comment 43: Line 563. When were these samples collected? Why only 2 samples were shown? Authors' response: The purpose of the figure is to show the comparison of concentration measured by different passive samples with that concentration measured by active sampler (FWI) and OPC. Concentration measured by passive samplers through the campaign (see the electronic supplement). We could do an ESEM analysis only for four days' samples from FWI (which is a total of 12 samples) (from July 26, 2017 to July 29, 2017; each day three measurements) due to limited resources. So we compared the available measurements from passive samples with that of FWI for only of those four days. The two-day measurements (samples) shown in the figure are arbitrary taken examples and are daily average measurements. They were collected on 26th of July and 27th of July. The information was added in the caption. The authors have analyzed a total of 6 samples from FWI on 26th of July and 27th of July. A clarification is added in the caption in the revised MS. The comparison for all 4 days for FWI yields the same behavior. In addition, in the revised electronic supplement, comparison with remaining two analyzed days of FWI samples are shown. Referee's comment 44: Line 578. In the methods section, the authors indicate that the sampling time for the passive samplers

was about 24 hours while for the FWI was only half an hour. Why have you plotted data that has been collected in such a different time interval? Authors' response: The reason why we set up different time interval is because one category of the sampler is operating passively while the other one, which is, a FWI operates actively. Therefor we cannot set up the same time interval for both types of samplers. FWI as an active sampler needs less time than the passive ones. However, we calculated from OPC the average PM10 for the hours of the FWI samplings and compare it with the PM10 of the respective deposition samplings from OPC and we found that the average PM10 values of both intervals differ by 2 %. Therefore, we think it is justified to compare samples from FWI and other passive samplers collected with different time interval. Referee's comment 45: Line 563. It is very difïňĄcult to see the y-axis scale. Could you add some minor ticks? Authors' response: The minor ticks are added in the y-axis scale (see the revised MS). Referee's comment 46: Line 569-571. What could be causing the disagreement at large sizes? Authors' response: This is more of a speculation, but the FWIs inherently don't have an inlet at all, whereas all of the passive samplers have an inlet like structure, so the large particles might not be able to enter the inlet, when due to the atmospheric wind direction fluctuations the wind vector is not in parallel with the inlet axis. Also other types of inlet losses in the growing boundary layer might occur, which are not regarded by the models. We have added a cautious sentence on that. Referee's comment 47: Line 578. Have you used a model to calculate the mass concentration from the mass ïňĆux measurements and then transformed this to number concentration? Authors' response: From SEM measurements, both the number and mass deposition rate are obtained for each single particle. So the same size-resolved model can be applied to convert the deposition rates into number size concentrations. We have added an explicit statement to the method section 2.9. Referee's comment 48: Line 578. Why only BSNE measurements have been shown? Are they representative of the other passive samplers? Authors' response: In section 4.1.2 (table 2 (now table 3 in the revised MS)), the authors showed that BSNE is actually a suitable instrument for a PM10 estimation. In this connection, the authors showed

the comparison of number concentration measured with Filter-sampler method, BSNE and OPC. Measurements by other samplers are shown in the electronic supplement. Referee's comment 49: Line 578. In the methods section, the authors indicate that the sampling time for the passive samplers was about 24 hours while for the ïňĄlter samples was only one hour. Why have you plotted data that has been collected in such a different time interval? Authors' response: Please refer to the answer to comment 44. Referee's comment 50: Line 581. Was the data in this section obtained following the same SEM approach as for the ïňĆat plate sampler? These measurements need to be described more precisely. Authors' response: The same SEM approach has been used in this section also. The only difference is that the flat plate geometry with 25mm-stub used here to collect particles where as in the flat plate described in the previous section, the stub was 12mm size was used. Precise description for the upward and downward flux measurement has been indicated in the section 2.4 in the revised MS. Referee's comment 51: Line 595. When were this samples taken? Authors' response: A legend is added to show different sampling dates (see the revised MS). Referee's comment 52: Line 599. As mentioned before, why hasn't the BSNE included in this analysis? Explain. Authors' response: Due to resource limitations, please refer to the comment above. Referee's comment 53: Line 609. It is difïňĄcult to see agreement in between the Stokes model and the CFD for the MWAC sampler in Fig. 20. Authors' response: Indeed, the agreement is poor in general. Regarding the mentioned models and sampler, this appears to be a misunderstanding regarding the 'general agreement'. it has been rephrased. Referee's comment 54: Line 623. Why haven't the errors been propagated? Authors' response: Refer to the revised MS for explanation. Referee's comment 55: Line 626. What do the vertical clusters of data mean? Why are there so many measurements aligned? (Particularly in the d, e and f case). Authors' response: The vertical clusters of data mean that for different wind speed situations, similar ratios are measured, where the models would predict different ratios. This was already seen above, where in contrast to the model prediction, no wind speed dependence was observed. Referee's comment 56: Line 627. Has

all the collected data been presented here Authors' response: All collected data (i.e. simultaneously analyzed samples from different samplers) has been analyzed and is show here. Referee's comment 57: Line 645-646. "atmospheric concentrations can be calculated from different sampler deposition ïňĆuxes, which are more in agreement". The statement about the increase in the agreement is a bit vague. In addition, it seems that only a subset of all the possible atmospheric concentration samples has been shown Authors' response: More data (campaign average) is added and the plot can be seen in the revised electronic supplement. The samplers are better in agreement with respect to the average, when the models are employed to calculate the concentration, but temporal variation correlation does not get better. Referee's comment 58: Line 648-649. "In particular when considering the size-resolved deposition velocities and ïňĆux ratios, great discrepancies show up". More detail in which deposition velocities and ïňĆux ratios is needed here. Authors' response: See the revised MS for explanation. Referee's comment 59: Line 652-656. This paragraph describes again about the size-resolved concentration. Reduce it and merge it with the ïňĄrst paragraph that describes this (643-647). Authors' response: The paragraph is reduced and merged into line 643-647 (see the revised MS). Referee's comment 60: Line 664-667. It seems that not all the data has been shown, therefore the reader cannot check this conclusion Authors' response: More data is shown now in the revised MS. Technical corrections Referee's comment 61: Line 205. "Ati".There is an "i" after the t in the denominator of the equation. The is missing a p if it is referring to particle density. Authors' response: Correction is made. Referee's comment 62: Line 207. Spaces must be included between number and unit (e.g. 2-4 μm). Authors' response: Correction is made in the revised MS. Referee's comment 63: Line 227. Does u-s mean us? I suggest use the same notation. Authors' response: Correction is made in the revised MS. Referee's comment 64: Line 234. Units appear in the exponential notation in some occasions but in some others they don't. I suggest to use the exponential notation through the whole manuscript (m/s should be written as m s-1) Authors' response: Majority of the exponential notation Units through the whole manuscript is written in

the form of 'a/b' and therefore we changed the units from exponential notation to the 'a/b' form though out the manuscript in the revised MS. Referee's comment 65: Line 236. "Wood1981". Wood 1981. Authors' response: Correction is made in the revised MS. Referee's comment 66: Line 351. Missing coma or full stop Authors' response: Correction is made in the revised MS. Referee's comment 67: Line 514. Two notations have been used to describe the observatory. Through most of the text, "Izaña Global Atmospheric Watch observatory" has been used, but here, a different one has been used. You can mention both at the beginning and then use only one trough the text. Authors' response: Correction is made in the revised MS. Referee's comment 68: Line 581. "upward/downward-facing measurements" Authors' response: We do not understand this comment. The collection surface in this measurement is facing to each other in upward-downward direction and thus the name "upward/downward-facing measurements" is used. Referee's comment 69: Line 584. "Up-ward" and "Down-ward". Is this the right notation or is it upward and downward? Authors' response: "Up-ward" and "Down-ward" is replaced by "upward and downward" in the revised MS. Referee's comment 70: Line 603. "V-dp" was referred earlier in the paper as Vd. Use a consistent notation Authors' response: "V-dp" is replaced by "Vd" in the revised MS. Referee's comment 71: Line 642. "variability of dust". Authors' response: We do not understand this comment. General Referee's comment 72: Missing a, b, c... labelling in the multi panels. Sub-indexes haven't been written in many ïňĄgures (E.g. u-s instead of us). Authors' response: multi panels has been labelled with a, b, c... and indicated in the caption. Sub-indexes have been corrected now (see the revised MS).

The answers are all in the supplement!

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2019-187/amt-2019-187-AC1-supplement.zip

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-187, 2019.