

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.

Anonymous Referee #1

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This paper presents a comparison in between different four passive samplers that are commonly used for dust deposition measurements. In order to compare how these instruments perform, the authors have used a set of techniques (SEM, classic deposition velocity models, computational fluid dynamic simulations and other aerosol sampling instruments) that allow them to identify biases in these samplers. Although the used techniques are robust and the presented data is very relevant and will be very useful for the mineral dust community, I would only recommend this paper for publications after many major changes are addressed.

In spite of the good quality of the experimental work, at the present moment, the

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manuscript seems to be in a very early stage of the publication process. There are many inconsistencies, typos, unexplained data and figures and it is extremely difficult to follow, particularly in the results and discussion section. In addition, some of the given conclusions are not really supported by the data, or they do in a vague way.

General comments.

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 figure in the text but this is not always the case and it makes it difficult to read the manuscript. See specific comments. In addition, the manuscript presents a large amount of data in different figures and tables but in some occasions, the discussion of these data is too short.

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 specific comments. In addition, there are many formatting issues. Some of them are pointed in the specific comments.

The geometry and computational fluid 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 fluid analysis of this fourth sampler should be included or at least justify its absence.

Many comparisons are presented all over the manuscript, but it seems that a significant 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 specific comments.

In this manuscript many comparisons in between different instruments are presented. Were the sampling times of each instrument overlapping 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 specific comments.

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 significantly affect the measurements.

Specific comments.

Line 17. "This study focuses on the microphysical properties". This is too vague.

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.

Line 20. Acronyms in the abstract have not been defined before.

Line 26-28. Acronyms defined after they appear for the first time.

Line 97-98. What about the sampling time of the Flat plate sampler? Were the filters ran for one hour or 24 with the passive samplers?

Line 149. This section needs a bit more of detail.

Line 159. The acronym SMPS hasn't been defined. In addition, there are no other references to the SMPS in the main text. Was the data used for this work?

Line 164. How were the samples transported and stored?

Line 170. "Randomly selected areas". Were they randomly generated or were they selected manually by the user?

Line 194 and 222. Was the temperature dependence considered in the density and dynamic viscosity choice?

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Line 258. I think this section needs to be better explained and describe why and how different models were applied to different samplers.

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?

Line 430. In the mentioned tables, the size distribution for each collected sample is given in both mass flux and number flux. 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?

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 figure captions.

Line 435. In this section, the terms “deposition flux” and “mass flux” 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 first.

Line 449. “we can clearly see that that there is high temporal variation in deposition flux between dust event days and non-dust event days”. Fig. 9 doesn’t clearly show this. There is a significant difference for the MWAC sampler, but for the other 3 instruments, the difference doesn’t seem “high” for the first four bins (up to a factor 2-3?). This is difficult 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 flux during a dust event and a non-dust event.

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 difficult 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

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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 fluxes. This has been done for most of the other figures 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 appreciate 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?).

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.

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?

Line 474-485. It is very difficult 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? The y-axis is labelled as deposition velocity ratio, however, ratios of dry deposition flux has been plotted as well. Are these ratios equivalent as one would expect from the equation 7? In general, I think that this figure and what has been plotted in it needs to be much better explained than it is now.

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 first case, add

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some references.

Line 487. What has exactly been plotted in Fig. S2? It is not obvious from the description. Explain this properly.

Line 489-491. The anti-correlation reported by the authors in the number flux-wind speed data cannot be seen in Fig. S2. Remove this or justify based in some quantitative statistical analysis.

Line 496. Again, two different notations for the dust deposition flux have been used. How is this data related to Fig. S2?

Line 499. What has it been shown in the table 2? From the caption, the reader can understand that dust deposition flux (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 flux data to obtain this concentrations?). This section is very confusing and unclear and it needs to be much better explained.

Line 503. Do you mean from the correlations in Table 2? If so indicate it. In the description of the table it says that the flux was correlated with OPC number concentration, but here the authors mention here PM10. Do you mean number concentration below 10 μm ?

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 figure caption.

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?

Line 516. "Small particle". Is this a common notation in dust deposition studies to refer to the 1-10 μm size range?

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Line 520-521. “The effect of wind speed on deposition velocity is negligible”. Why?

Line 522. In the text, the apparent deposition velocity concept has been introduced as the ratio of the number flux to number concentration. I suggest to use deposition velocity in the y-axis label.

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?

Line 532. Isn't the mas flux 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.

Line 532. Why does the max flux 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.

Line 532. What is “impaction curve & Piskunov” in the legend? The concept of “impaction curve” hasn't been mentioned before.

Line 540. How were the number size distributions calculated from the flux measurements? This should be better explained here or in the caption.

Line 544. Why have these specific samples (and these specific 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 the OPC measurements? It seems too arbitrary to show only 4 comparisons out of many and extract some generalist conclusions.

Line 545. This caption needs to be rewritten in a more clear way. Were the SEM obtained mass flux distribution converted into mass size distributions using the different

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approaches and then transformed into number size distributions using a density value?.

Line 550. The Momentum flux approach data looks black not green.

Line 555. “the above figure (Figure 16)” should be referred as Figure 16 or Fig. 16.

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.

Line 563. In order to calculate the mass concentration measured by each sampler, don’t you have to use the SEM obtained mass flux measurement and assume one of the mentioned models? You haven’t mentioned yet a direct method to measure mass concentrations from the passive samplers.

Line 563. When were these samples collected? Why only 2 samples were shown?

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 hour. Why have you plotted data that has been collected in such a different time interval?

Line 563. It is very difficult to see the y-axis scale. Could you add some minor ticks?

Line 569-571. What could be causing the disagreement at large sizes?

Line 578. Have you used a model to calculate the mass concentration from the mass flux measurements and then transformed this to number concentration?

Line 578. Why only BSNE measurements have been shown? Are they representative of the other passive samplers?

Line 578. In the methods section, the authors indicate that the sampling time for the passive samplers was about 24 hours while for the filter samples was only one hour. Why have you plotted data that has been collected in such a different time interval?

Line 581. Was the data in this section obtained following the same SEM approach as

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for the flat plate sampler? These measurements need to be described more precisely.

Line 595. When were these samples taken?

Line 599. As mentioned before, why hasn't the BSNE been included in this analysis? Explain.

Line 609. It is difficult to see agreement between the Stokes model and the CFD for the MWAC sampler in Fig. 20.

Line 623. Why haven't the errors been propagated?

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).

Line 627. Has all the collected data been presented here?

Line 645-646. "atmospheric concentrations can be calculated from different sampler deposition fluxes, 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.

Line 648-649. "In particular when considering the size-resolved deposition velocities and flux ratios, great discrepancies show up". More detail in which deposition velocities and flux ratios is needed here.

Line 652-656. This paragraph describes again about the size-resolved concentration. Reduce it and merge it with the first paragraph that describes this (643-647).

Line 664-667. It seems that not all the data has been shown, therefore the reader cannot check this conclusion.

Technical corrections.

Line 205. "Ati". There is an "i" after the t in the denominator of the equation. There is missing a p if it is referring to particle density.

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Line 207. Spaces must be included between number and unit (e.g. 2-4 μm).

Line 227. Does u-s means us? I suggest use the same notation.

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}).

Line 236. "Wood1981". Wood 1981.

Line 351. Missing coma or full stop.

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.

Line 581. "upward/downward-facing measurements"

Line 584. "Up-ward" and "Down-ward". Is this the right notation or is it upward and downward?

Line 603. "V-dp" was referred earlier in the paper as Vd. Use a consistent notation

Line 642. "variability of dust".

General: Missing a, b, c... labelling in the multi panels. Sub-indexes haven't been written in many figures (E.g. u-s instead of us).

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