Referee 1 comments on the revised manuscript by Waza et al.

I would like to thank the authors for their detailed response to the referee's comments. The manuscript looks much better now, it has gained a lot of coherence, it is easier to read, more compact and with a better formatting. However, there are still some points to address before it is ready for publication.

Most of the comments of the previous review have been responded correctly and therefore accepted. However, there are some for which I still have some comments. In addition, I have some other comments and suggestions on other parts of the paper to address before it is ready for publication. See below:

- Referee's previous comment 37 and 60: In the previous version of the manuscript, only comparisons from a few days had been shown. Now, extra comparisons have been added to both the MS and the electronic supplement, which is good. Have all the possible comparisons been added? If not, I still understand that showing all the possible comparisons taken during the whole campaign could be excessive. However, I recommend to the authors that at least take a look on all the possible comparisons of the campaign and explain of the conclusions extracted from the shown samples are still valid. This is valid, in general whenever some comparisons are omitted.

- Referee's previous comment 48: I would add to the MS the reason why BSNE has been chosen.

- General comment on the samplers, FWI and filter comparisons: (line 580-610, related to referee's previous comment 44): The main caveat of the comparisons between the FWI vs deposition samplers and filter sampler vs deposition samplers is the different sampling time. I understand the need to use different sampling times for different instruments but the comparison should be justified, since in some cases you are comparing a 1 hour sample with a 24 hour one. It seems very surprising that PM10 values changed only up to 2 percent over a 24 hour period since there is a big variability (more than one order of magnitude) over the whole campaign for values such as the deposition rate or number concentration. As an example, in Fig. 13, the authors have shown the size distribution calculated from the filters on the 26th and 27th of July (I understand that they are two 1 hour samples separated 24 from each other) and the difference seems about a factor 2, which is not small. I strongly recommend to extend the analysis (maybe not in the main part of the paper but in the electronic supplement) on how the number concentration of different size bins change over the 24 hour averaging periods in order to justify that 1 hour samples can be compared with 24 hour samples.

- Sect 2.3: this is not very crucial but as a suggestion, I would organise it as follows:

2.3 Particle sampling

2.3.1 Dust deposition samplers

2.3.1.1 Flat plate sampler

2.3.1.2 Sigma-2 sampler

2.3.1.3 The Modified Wilson and Cooke (MWAC) sampler

2.3.1.4 The Big Spring Number Eight (BSNE) sampler

2.3.2 Free-wing impactor (FWI)

2.3.3 Filter sampler

And I would add the Upward-downward sampler to the Sect 2.3 or 2.3.1 since they are essentially dust samplers that are analysed in the same approach.

- Sect 2.11. Could you add a short description of the inlet biases? From figure 11 it seems that they are negligible? If the differences are so small, is it worth it to show this correction?

- Table S1: I don't understand when it says "Minimum, maximum and median daily basis mass deposition rate". From what I see in the data, it seems that you have reported the mass deposition rate size distribution (average I guess) for each day, but I don't understand where the maximum and minimum are. This happens for other tables in the SI.

- Fig. 5 and Fig 6. Inconsistent notation in the axis labels

- Fig. 6. Maybe mention in the caption that the median, percentiles and standard deviations shown there correspond to the variability of the whole campaign for each instrument and bin.

- Line 464-466 "The deposition rate ratios obtained from the measurements are identical to the deposition velocity ratios, when the sampling time and concentration are the same". What does this mean? Which deposition velocity ratios do you mean in the second case?

- Lines 498-492. Table 3 shows that there is a positive dependency in between the concentrations and number deposition rates (r coefficients are positive, so if you increase one variable, the other also does) but it doesn't show a linear correlation since the r2 values are not close to 1 for the first two samplers (particularly the MWAC). However there is a good correlation for the last two samplers. Please clarify this in the text. Also, when it is written "see Figure S 16" it should be "see Figure S 16a".

- As a suggestion, you probably don't need to mention "in the electronic supplement" (e.g: "see Figure S X in the electronic supplement") every time since the label Figure S X already means electronic supplement.

- Line 524. "Negligible". Wouldn't it better to use another concept to describe the relation in between the wind speed and the deposition velocities? The analysis shown shows that the wind speed doesn't correlate with the deposition velocity and rate at all but it doesn't necessarily show the effect is negligible.

- Figure S 9 (and also previous referee comment 32). How can the errors of the campaign average be so high? For the MWAC, as an example, you are basically reporting a range of about 4.5 orders of magnitude for each bin. I suggest checking how errors have been calculated here and reconsidering if it is worth it to call that agreement considering the massive uncertainties.

- Line 555. “Overall, the number concentrations obtained from OPC measurements are slightly higher than the ones”. This is not that clear, particularly for the Sigma-2. Please it discuss more.

- Line 560. Isn’t his statement also supported by the Figure S11? If so indicate.

- Line 564. Legend of the panels a and c and d should be reordered: OPC, Momentum flux, Momentum flux PM10, Wood 1981 and Wood 1981 PM10. This would be make easier to realise that the difference in between applying the correction and not applying it is so small. In addition, I think the axis limits should be chosen differently since more than half of the graph is empty. (5 orders of magnitude are shown while the data only scatters over 2 orders of magnitude in the y axis).

- Line 569. “Deosition” should be deposition.

- Fig S11 and S12. Why not to split each of this figures into part a and part b, and having the multi panel with all the information for each sampler under the same title? In the caption of the Fig S11, yellow and green are mentioned while data has been plotted in red and green.

- Line 586. “The blue curve shows the concentration measurements by the OPC”. This is not necessary since it appears in the legend.

- Figure S 10. Why is there such a massive discrepancy between the Sigma-2 and the rest of the instruments on the 29th of July? Also, the colour choice seems a bit arbitrary, why does cyan appears twice?

- Line 600. I would mention clearly somewhere that graphs like the Figure 13 but for the other samplers have been presented in the SI, as well as the reason why the BSNE has been shown primarily (see response to referee’s pervious comment 48). Also, this section in general needs a bit more of discussion on the agreement or disagreement of the results. For example, the disagreement for the flat plate (I the supplement) is not as significant as the others.

- Line 601-602 “The reasons for this weak correlation – in particular in comparison to the ones 601 from Sigma-2 and BSNE – remain unexplained by now”. Does this mean the reasons for the disagreement between the different samplers and the OPC/filters? If so explain better.

- Line 638-641. I would add some lines summarising the comparisons shown in the electronic supplement.

Other general comments:

- In some occasions, a few examples of a comparison are shown in the MS, while the bulk of the data is presented in the electronic supplement. (e.g: Fig 11, S11 and S12). I would state it more clearly in the main text for the sake of simplicity.

- Many figures, particularly in the electronic supplement, don't have a legend (and it is explained instead in the caption). In general it is better to have legends than explanations in the caption if possible.