Referee report

LOAC: a small aerosol optical counter/sizer for ground-based and balloon measurements of the size distribution and nature of atmospheric particles: 1. Principle of measurements and instrument evaluation

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General comments:

The LOAC project is clearly a very ambitious project in view of its versatility, the large range of platforms able to host it, its objective to provide information on aerosol size and type as well, and mainly, over the extremely large targeted particle size range, spanning not less than almost three orders of magnitude.

The main question is obviously to determine if this instrument is able to achieve these ambitions.

If the extended description of the measurement principle and intercomparisons with other instruments seem convincing for some aspects, it also shows in a recurrent way limitations of the technique for some others, making the performances claimed by the authors somewhat questionable.

Very satisfactory values of uncertainties are provided for some limited aspects of the measurement processing chain (e.g. uncertainty on the counting in well-controlled experimental conditions and for very particular cases of particle size distributions, uncertainties on the laser intensity and on the pumped air flow, standard deviation between a set of successive measurements). But an estimation of the global resulting uncertainty for measurements in real conditions is never given, and the analysis tends to show that many aspects could lead to questionable estimates of the particle concentration: effect of the inlet, which could be poorly controlled concerning the sampling of large particles, bias in particle counting when particles cross simultaneously the laser beam (cf figure 3), insensitivity of the calibration curve to particle size for very thin particles, multiple values of particle diameter corresponding to a given value of the measured “flux” (in mV) around the 3-10 µm particle size range (cf. Figure 4), saturation effects at high concentrations of large particles (> 10 µm; cf. Figure 6), etc. Altitude dependence (possibly through temperature/pressure) of the uncertainty is never really tackled.

Consequently, LOAC, which seems to give very useful information for the detection of aerosol types, could miss the ambition to provide quantitative estimates of the particle size distribution over a quite large part of the claimed 0.2-100 particle size range.
The authors should present their study in a much more critical way to give better and more reliable information over the exact performances which can be expected from LOAC. This is an indispensable condition to bring confidence in the quantitative results which have been provided and will be provided in the future by LOAC.

Specific comments:

Abstract

- L. 4-5 p.9995: To be precise, in [Deshler et al., 2003, cited in the paper], the authors mention the value of 150 nm as smallest particle size measured by optical particle counters (OPC) at Laramie. However, these authors also measure condensation nuclei (radius ≤ 0.01 μm) by growing particle with sizes down to 0.01 μm in the sample stream.
- L. 5-6 p.9995: "sensitive to nature of the particles": Do the authors refer to the value of the refractive index? This could be mention for the sake of clarity.
- L. 23-24 p.9995: The authors should remove the sentence "All these tests indicate that no bias is present in the LOAC measurements and in the corresponding data processing." See the specific discussions further in the text.

1. Introduction

- L. 9-10 p.9996: The authors should also mention their role in ozone photo-chemistry, e.g. through the formation of PSC.
- L. 15 p.9996: The residence time is not always short ! As the authors will know, aerosols from the Pinatubo eruption in 1990 left a significant signature in the stratosphere at least for 5 or 6 years! Hence, aerosols may be used as a tracer in some circumstances.
- L. 23-25 p.9996: It seems difficult to make a correct overview of in situ aerosol measurement without citing Deshler. This place ("aerosol collecting instruments") seems the right one to add some reference to Deshler's work.
- L. 24 p.9996-L. 25 p.9997: I am not sure that using speciation index charts to determine the nature of the particles is very different from choosing a priori hypotheses in the retrieval process.
- L. 3 p.9997: The authors could precise that it concerns the **local** size distribution.
- L. 15 p.9997: The authors could add: "[… a given value of their refractive index] and some assumption on their shape".
- L. 26 p.9997, L. 3 p.9998, L. 15 p.10001: Following [Lurton et al., 2014, fig. 8], there is still some dependence int the refractive index. Hence, the authors should use a less strong expression for "such non-dependence", for instance, "Such **weak** dependence …". In the same way, "the light scattered is dependent only on the

size" should be replaced, for instance, by: "the scattered light mainly depends on the size…". And "the 12°-channel, which is insensitive to the refractive index of the particles" in L. 15 p10001, should be replaced by "the 12°-channel, which is poorly sensitive to the refractive index".

- L. 20 p.9998: The concept of "aerosol topology" is not standard, and refers definitely not to transparency properties of the particles. At best, it could refer to the study of the aerosol surface. Do the authors mean "aerosol typology"?

2. Principle of measurements

2.1 Instrument concept

- L. 10-11 p.9999: The fact that the optical chamber is open is obviously not sufficient to say that the pressure is the same as outside. If a pump is used to focus the air flux into a flexible tube toward the optical chamber, many elements let think that the pressure might be highly variable, and definitely often different from outside! This sentence is actually in contradiction with what is mentioned in statement in L. 28-29 p.10000. The description of the device should be clarified, possibly with a quantification of the importance of the pressure variations, and/or by completing figure 2a.
- L. 23 p.9999: The reference is already written in the caption and might be removed to shorten the text.
- L. 5 p.10000, L. 25 p.10001-L. 2 p.10002, L. 18 p.10003, L. 1 p.10010: I guess that the "beam homogeneity" refers to the sectional distribution of light intensity. Aren't there any other issues related to the beam characteristics or stability in time, also possibly related to pressure/temperature conditions (e.g. sensitivity of the laser wavelength to the temperature), and that should be taken into account in the estimation of the uncertainty? LOAC is supposed to measure an extremely broad range of particle sizes, what necessarily requires a large dynamical range. Hence, one can conceive that any sensitivity of the beam intensity to external conditions could affect the dynamical range and introduce a bias in the measurements.
- L. 14-15 p.10000: I understand that a particle transiting in 500 μs is detected about 20 times (=electronic sampling x transit time)? I guess that size-dependent inertia, or complex motion of irregular grains, etc. may affect the scattered flux. E.g., the small peaks found in Fig. 3 and identified as the contribution of small particles might just be caused by the light scattering by a rotating irregular grain (as indicated by the authors a few lines further). How do the authors estimate the uncertainty on the particle flux due to this kind or effect?
- L. 14-15 p10000: The authors mention a particle transit time "equal or lower than 500 μs". But the pulse duration shown in Fig. 3 is about 0.75-1 ms. The authors should adapt their values or explain how to reconcile this difference.
- Figure 3 shows an example of a 1-ms long pulse corresponding to a 5 μm particle (See text). This allows us to conclude that the flow velocity is about 2*5 μm/ms =

10 mm/s. Using this value and following the same rationale as the authors (L. 10-19 p.10002), I tried to have some idea on how LOAC is able to detect high concentrations as found by other instruments cited in the paper. I made some intercomparison between the theoretical upper limit of what LOAC is able to detect (supposing a hypothetical population of particles in single file and infinitely close to each other), and the concentration detected by other instruments. This is the result of these calculations for several plots given in the paper. Corresponding figures and instrument used for the comparison are indicated.

Particle size (µm)	Width of the corresponding detection peak (ms)	Max Nb. Of particles detectable per 10 s	Corresponding maximum concentration (cm-3)	Figure; instrument used for comparison	Maximum concentration found by the reference instrument (cm-3)	Corresponding Max Nb. of particles detectable per 10 s using LOAC's measurement principle
0.2-0.5	0.04-0.1	*250.E3-100.E3*	**750-300**	10; SMPS	**1000**	*300.E3*
5-6	1-1.2	*10.E3-8.3E3*	**30-25**	13; WELAS	**100**	*30.E3*
0.3-0.5	0.06-0.1	*167.E3-100.E3*	**500-300**	15; Grimm	**60000**	*20000.E3*
0.5-0.7	0.1-0.14	*100.E3-71.E3*	**300-215**	15; Grimm	**4000-6000**	*1300.E3-2000.E3*
0.7-1.0	0.14-0.2	*71.E3-50.E3*	**215-150**	15; Grimm	**1000-3500-5000**	*300.E3-1150.E3-1700.E3*

I conclude that, in most of these cases, the maximum concentration which can be detected by LOAC is much smaller than the values found by the reference instruments. If I understand correctly, higher values derived from LOAC measurements (using the method given further in the paper) are thus some kind of estimation based on (highly) saturated measurements. Hence, I have some doubts about the reliability of this approach.

- Figure 3: The authors also mention the presence of a few submicronic particles. Are these particles corresponding to very small peaks at time 0.4-0.5, 1.5, 2.3, 2.4 and 3.3 ms? How is the threshold corresponding to the red line determined? Is the quantification of the noise reliable enough to allow the correct detection of real particles producing the weakest signal (1 mV for the smallest particle, following L. 6-8 p.10002), and this for any atmospheric conditions?

2.2 Calibration

- L. 15 p.10001: I don't see why the 60° channel doesn't need to be calibrated: I guess that the flux measured at 60° also depends on the size and the shape of the grains, and that the response is equally affected by the complexity of the particle population, even if the refractive index is the main driver in the measurement.
- L. 4 p.10002: The authors should quantify the expression "low air temperature".
- L. 16-18 p.10002: "… size classes [smaller than 0.5 µm] having a width of 0.1 [or 0.2] µm": a reference to Table 1 should greatly clarify what the authors mean.
- L. 28 p.10002-L. 3 p.10003: This limitation for stratospheric measurements is important and should be shortly reminded in the conclusions.
- L. 13 p.10003: The authors should precise "electric flux" in order to avoid the possible confusion with particle flux.
- L. 16, 21-22, 27 p.10003: The expression "scattered flux", without more explanation, looks a bit strange. The authors are using the word "flux" possible for 3 quantities: "electric flux", "particle flux" and "scattered flux", what can be confusing and requires some caution while using the term "flux". Concerning the expression "scattered flux", the authors could move the definition given in L. 22 ("which corresponds to the photodiode output voltage…") to L. 16. I guess also that this concerns the scattered intensity at 12° only. This should be mentioned.
- L. 14-17 p.10003: I would like to point out the fact that all scales are logarithmic, what makes relative the expression "no significant dependence". As an example, following Renard 2010a, there is a factor 1.6 between the reflectance of fluffy silica and sand.
- L. 18-25 p.10003: The formulation of the text is a bit confusing. After explaining the nature of the particles used to make the calibration (this concerns obviously measurements), it is mentioned that an offset is added to some "calculation" of the calibration curve. The authors should explicitely make the distinction between measured and theoretical aspects (where the offset is added) for the clarity of the text.
- Figure 4: (1) Given as is, this figure raises many questions concerning the experimental validation using 2 kinds of particles ("beads" and "irregular grains") without any overlap in the size range, and following totally different behaviour. In particular, the discrepancy between the Mie scattering curve and the other response, apparently common to all kinds of possible irregular particles, seems really surprising. Many answers are given in Lurton's paper, and the authors should explicitely mention that this paper is devoted to an extended study of these aspects. The key-aspects explaining the difference in the behaviour of spherical and irregular grains are only summarized here by a sentence "copied-pasted" from Lurton's conclusions, and which is in my sense particularly obscure. The authors should revise this explanation to make it understandable by people who didn't read Lurton's paper. Joining disparate explanations (L. 25 p.9997-L. 6 p.9998, L. 4-6 p.10004 and maybe others) about the fundamental reason of the choice of the 12°-16° angle, mentionning Bragg diffraction, and explaining that the "irregular

grain" behaviour is obtained even for particules with a very small roughness parameter would really clarify many things.

- Figure 4 and L. 14-19 p.10004: (2) Despite Lurton's study, I am not fully convinced by this "calibration". Amongst the particle types the authors want for sure to measure, we find soot (not considered in Lurton's paper). Do the authors know the value of the roughness parameter of soot, that possibly looks like aggregates of spherical particles? In view of this particular structure, soot could possibly have a very low value of roughness parameter which could make its respond much more close to the Mie scattering regime. Hence, it could present some oscillatory behaviour, similarly to the glass beads studied by Lurton et al.
- Figure 4: (3) In all cases of rough particles considered in Lurton et al. , 2014 (even for the most irregular ones), their roughness model (which agrees well with their measurements) shows a range of particle sizes for which the scattered intensity presents an oscillatory behaviour (See figure 8 in their paper). This is of particular importance for the purpose of the present work, where the value of the "flux" is used to determine the diameter of the particles. Unfortunately, these theoretical curves have been omitted here, what masks in my opinion a problem making LOAC probably not effective for this range of particle size (around 3 to 10 μm).
- Figure 4: (4) Similarly, for all values of the flux up to about 100 mV, each flux value can correspond to multiple values of the diameter. Below 0.5 μm (range relevant for stratospheric studies), the "flux" is almost constant. Hence, taking into account comments (3) and (4) on this figure, I don't see how to discriminate unambiguously the particle size for particles smaller than ~10 μm.
- L. 18-21 p.10003: Comments (3-4) on Figure 4 make me conclude that the estimates of the uncertainty on the size determination from the "flux" measurement is much higher than what is mentioned, due to the multiple values of the particle size corresponding to a given "flux" value.
- L. 4 p.10004: The concept of "roughness parameter" should be defined. The authors could possibly refer to some work on the subject or using this parameter (e.g. Lurton et al., 2014).
- L. 7-13 p.10004: This explanation tends to show that the power law fit should follow much more closely the Mie scattering curve in Figure 4. Actually, Weiss-Wrana's observations seem to differ significantly from what is observed here (Figure 4), and in Lurton et al., 2014 (their Figure 8). It is not clear to me what the conclusions of the authors are.
- L. 20 p.10004-L. 2 p.10005: Figure 5 does not validate anything, it is just an example of result which may be credible, but is not confirmed nor invalidated by any other data. In view of the length of the manuscript and of the presentation of other similar curves later in the paper, I would remove this paragraph and the figure.

- L. 24-25 p.10004: I don't understand the meaning of this sentence, and more particularly, I don't see what the authors mean with "calibration error": this is probably just an effect of the impossibility to inverse a multi-variate function (i.e. the "calibration curve"), reflecting the limitations of LOAC in the 3-10 µm range (See comment on Figure 4 (3)).
- L. 29 p.10004-L. 2 p.10005: I don't agree on the explanation given by the authors. It seems to me very unlikely that Mie theory fails to describe droplets because they are possibly slightly stretched while passing through the OPC. This would question many decennia of atmospheric research. The use of the fitted power law does bias the size determination. This is clear, e.g. from Figures 10 (See my comments on this figure). It is also very clear from Figure 4 that "flux" measurements by LOAC are not able to discriminate spherical particles with a size below 5 µm. While it is much easier to use the fitted power law for which a flux value always corresponds to one single value of the particle, for particles larger than ~0.6 µm, the Mie curve shows that this value will generally differ from the real value, possibly by several microns for the largest particles. If the authors pretent to be able to discriminate unambiguously the size of particles supposed to be spherical in the range 0.6 – 10 µm, then they have to provide convincing results.
- L. 3 p.10005: Considering all the comments I made about section 2.2, I can't agree with this statement. It is true that the authors qualify their statement further in the paragraph, but the inability to discriminate particles probably up to 10 µm (See comments on Figure 4), should be mentioned. Further, the sampling cut-off of the inlet discussed later (L. 9-10 p.10005 and mainly section 2.6, in particular L. 27-28 p.10010) could make the detection of very large particles (above a few tens of µm) poorly quantified (See comment on L. 27-28 p.10010). Hence, this conclusion looks really hasardous.

2.3 Concentration measurements

- L. 19-20 p.10005: See remarks on Figure 4 (Section 2.2).
- L. 23 p.10003-L. 4 p.10006 : The number of undetected small particles depend on the particle size distribution. The more large particles are found, the higher the amount of undetected small particles. A Monte-Carlo calculation can at best provide "detection efficiency coefficients" giving some indication the number of undetected particles *for the size distribution assumed in the calculation*. But this is only an indication with a reliability decreasing with an increasing number of large particles, and its provides absolutely no information about the real particle size distribution.
- L. 5 p.10006: I guess that there is no maximum for the transit time! Isn't the peak width increasing linearly with the particle size?!
- L. 10-14 p.10006 and Figure 6: How do the author derived this inverse proportionality between real and detected concentrations (i.e. not what is shown in Figure 6!)? I guess that in this extreme case, the system just reaches the saturation, and the detected "flux" tends to keep a value higher than the threshold,

giving asymptotically a number of “one particle detected” independently of the real flux. Concerning the correction factor (for particles larger than 1 µm) shown in Figure 6, I guess that statistical fluctuations make the uncertainty on this correction very high.

- L. 13-14 p.10007: For the sake of clarity, the authors could make the link between this offset and the “threshold value” in red on Figure 3.

2.4 Aerosols typology

- L. 25 p.10007 : Do the authors mean “the **scattered light** detected by the 60° channel decreases …”?
- L. 15 p.10009: I guess the authors mean: “… works well **only** in case of a homogeneous medium”.

2.5 Reproducibility

- L. 11-12 p.10010: The concept of “reproducibility uncertainty” looks strange; the authors should express it in a more standard way.
- L. 13 p.10010: Do the authors mean “a total uncertainty” of ± 15%? Taking into account all the discussion, I think that the total uncertainty is much more than the sum of these 3 contributions of 5% each. The authors precise “assuming no systematic bias”. But I don’t think that this assumption can be done, and anyway, this conditions can’t be promptly omitted as done by the authors (See e.g. caption Figs 10-13; Paper 2, L. 19, p.10061)
- L. 16-17 p.10010: The expression “power law size distribution” is unclear. Do the authors mean “irregular grains for which a power law fit is a valid estimate of the calibration curve”? And if yes, how was the particle size distribution? Like described in L. 4-8 p.10003?
- L. 17, 19 p.10010: What do the authors mean by “standard deviation”? Do they refer to the standard deviation of the set of measurements they made in fixed experimental conditions? This is of course very different from the total uncertainty on the concentration, which could be highly biased by the many aspects that raised in Section 2.

2.6 Inlet sampling efficiency

- L. 27-28 p.10010, L. 12-13 p.10011: In an “Application Note” of the firm TSI specialized in precision measurement instruments (Application Note ITI-58, “Measuring total suspended particulates (TSP) with aerosol photometers”, easily available on their website www.tsi.com/uploadedFiles/_Site_Root/Products/Literature/Application_Notes/ITI-058.pdf), the sampling efficiency is calculated to a configuration very close to the one used in the paper (TSP inlet, ~30 cm long tube with ~0.6 mm inside diameter, 90° bend and flow rate of 1.7 l/min). Their calculation give penetration efficiencies of 100% for particles in the range 0.1-1 microns, decreasing for larger particles to 75%, 25%, 6%, 1% and 0% respectively for particles of 10, 20, 25, 35 and 50 microns. These values are far less optimistic than the "efficiency close to 100% up to a few tens of µm" claimed by the authors (L. 28 p.10010). The authors of this note also question the ability of a TSP inlet to provide effective sampling of particles larger than ~20 µm, sizes that seem not considered by the authors in their calculations of the sampling efficiency (cf. L. 27-29 p.10011). Did the authors investigate this issue into detail? Does the use of a beveled metal inlet during balloon flights make the situation more favourable for particles larger than 20 µm.

- L. 21-26 p.10012: This validation seems very important and should also assess the performances of LOAC in the detection of particles larger than 20 µm.

3. Cross-comparison with other instruments

- L. 14 p.10013: If used, the meaning of the acronym "OAG" should be clear at this stage. The authors might possibly refer to the section where it is defined.

3.1 Concentrations and size distribution

- L. 6-9 p.10014: The analysis of Figure 10 is particularly short and empty. It is a pity because this figure shows quite well features of the method that are consistent with what is expected from Section 2. For instance, particle class 0.2-0.5 µm is very poorly sensitive and fails to detect almost all the particular events or, at least, their amplitude. This is fully consistent with the absence of sensitivity shown by the "calibration curve" in Figure 4. Singularly, some peaks are found by LOAC (e.g. on January 9, 10, 13 and 14) but with the opposite sign with respect to SMPS (minimum peak instead of a maximum or vice versa). Have the authors any explanation for this discrepancy? The same weak sensitivity is found for the class 0.4-40 µm, although some more events are detected. I guess that this is probably due to the fact that LOAC is more sensitive in the upper part of the 0.4-40 µm size range. For particle class 1-50 micron (2nd panel), LOAC finds on the contrary a much stronger variability than the fog monitor. Would it be due to a wrong estimation of the size owing to the oscillatory behaviour of the calibration curve found (also for irregular grains) by Lurton et al. (2014) in the range ~0.6-10 µm? (See also Figure 4 of the present paper and my comments in this figure). The authors should analyse carefully this figure and the weaknesses it reveals (or rather, it confirms) to really assess what are the exact performances of LOAC, more particularly here for what concerns the sensitivity to size.

- Figure 11: Could the strong underestimation of the concentration for the large particles (bigger than ~3-4 µm) with respect to the Fog Monitor confirm a possible strong bias due to the detection blocking in case of large particle (See comments of Figure 3), possibly worsened by aspects of sampling efficiency for large particles (See comments on L. 27-28 p.10010 etc.)? Do the authors have any idea why such a strong underestimation does NOT occur in the case of the fog event illustrated in Figure 13?

- Legend Figure 11: concerning the uncertainty, See "General remarks".

- L. 19-22 p.10014: I don't think these are the main reasons to explain the discrepancy. See my comment on Figure 10.
- L. 26 p.10014: Between fog events, LOAC and WELAS disagree most of the time, by up to one order of magnitude!
- L. 26 p.10015-L.12 p.10016: I think that all what was presented before also reveals cleary identified weaknesses on LOAC's side in the size range 0-5 µm. The authors should not focus too much on conjectures about what features of other instrument could explain, and just focus on an objective analysis of LOAC's performances. In particular, there is an obvious problem of sensitivity for LOAC in the range 0.2-0.5 µm confirmed by Figure 10, lower panel. If think that the authors should be satisfied with this kind of explanation, this looks the most evident one.
- L. 23-25 p.10015 and Figure 15: I really don't understand how the authors are able to distinguish the particle classes, and more particularly 0.2-0.3 µm and 0.3-0.5 µm, from the calibration curve. Do they make use of sieves? These figures mainly show the weak sensitivity of LOAC, except maybe in the class 0.5-0.7 µm (thanks the use of sieves?).
- Figure 16 and L. 10-12 p.10017: Even in this figure which shows a very good agreement between volume densities computed/observed from LOAC and AERONET, LOAC's weaknesses are visible: (1) Below ~0.5-1 µm (i.e. 1-2 µm for diameters), the weak sensitivity leads to increased discrepancies between both instruments; (2) For radii ~1.5-5 µm, error increases. This corresponds to diameters of 3-10 µm, for which calibration curve shows (See Lurton et al. 2014) an oscillating behaviour, making ambiguous the determination of the size distribution. This could be mentioned.
- L. 13-17 p.10017: So far, the authors don't present any analysis of the variation of the (randow, systematic) uncertainty with the altitude. Hence, they should not draw any conclusion at this stage on systematic bias.
- L. 18-22 p.10017: One more time, all these cross-comparisons seem to show in a consistent way, the same weaknesses of LOAC. Several plots present strong evidences of systematic biases between LOAC and other instruments.The authors should revise this paragraph consequently, or just remove it.

3.2 Tropospheric vertical distribution

- L. 10-12 p.10018: This is only partly true: For the flight illustrated in the lower panel, LOAC missed an important structure seen by WALI. And in both cases, from ~4 km toward higher altitudes, LOAC's sensivity seems to decrease very rapidly to become fully insensitive above ~5 km. Do the authors see any reason for this behaviour?

3.3 Topology of the particles

- Figures 18-22: All these examples seem to demonstrate the effectiveness of the use of LOAC for the determination of the aerosol type. Still, it is interesting to note that, if the “speciation index curves” overall remain within a given “speciation zone”, in the size range corresponding to the smallest particles, these curves show an irregular behaviour with points outside, at the boundary or crossing rapidly the speciation zone of interest. This reflects one more time the lack of sensitivity of LOAC for this kind of small particles, accordingly to the behaviour of the calibration curve (Figure 4).

3.4 Mass concentrations

- L. 14 p.10021: wrong value of the mass density for water.
- L. 28-29 p.10022: I don’t agree with the range of ~0.2-20 µm. All the previous intercomparisons confirm a problem of detection for very thin particles, expected from the behaviour of the calibration curve. The reason why this problem does not affect very significantly the mass concentration comparisons, is probably that the relative contribution of mass concentration of thin particles is small with respect to the mass concentration of large particles. This can be verified from the various examples of size distributions given in the paper. The lower limit of 0.2 µm is thus not correct.

4. Conclusions

- L. 8 p.10023: One more time, submcronic particles should be removed from this range.

Technical corrections:

- L. 4 p.10013, L. 14 p.10001: missing/wrong punctuation.
- L. 21 p.10002, L. 8 p.10022: incorrect word and symbol.
- L. 13-14 p.9998: incorrect sentence.
- L. 3-6 p.10013: The very long sentence should be splitted.
- Figure 10: Typo in the legend: “Fog Monitor”.
- Figures 18-23: The authors might consider reducing the figures (e.g. combination left/right panel for Figs. 18-22) to shorten the length of the paper.