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Atmospheric Measurement Techniques
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Discussions

Interactive comment on "LOAC: a small aerosol optical counter/sizer for ground-based and balloon measurements of the size distribution and nature of atmospheric particles – Part 1: Principle of measurements and instrument evaluation" *by* J.-B. Renard et al.

M. Hamilton (Editor)

murray.hamilton@adelaide.edu.au

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Dear Authors, the AMTD discussion on "LOAC: a small aerosol optical counter/sizer for ....- Part 1 ..." has now ended and you have the opportunity to respond to the reviewers comments, when submitting a revised manuscript for consideration for AMT. As associate editor, I have a number of remarks that I would make before you submit a revised manuscript. regards Murray Hamilton.

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The extent to which the manuscript relies on the earlier work in the Lurton et al. paper is not brought to the fore sufficiently, and this seems to be a major point of confusion for the reviewers, who (reasonably) are expecting the manuscript to be self-contained. In your response to reviewer #1 you say that you will include a summary of Lurton et al.. I agree with this but would suggest that this needs carefully to be given some prominence early in the revised manuscript.

The issue raised by reviewer #2 about figure 3, and the apparent lack of discrimination in particle sizes when they are smaller than 1 micron, is not adequately dealt with in your response. It is a reasonable point to raise on the part of the reviewer since the manuscript claims that this is is an updated version of figure 5 of Lurton et al., and yet in Lurton et al. there is rather more information presented including, crucially, the error bars.

The layout of section 2.2 also, in my opinion, contributes to the problem; paragraph 2 discusses the use of spherical particles for calibration, and then notes that these are not representative of real atmospheric particles. Then paragraph 3 goes on to describe the use of irregular particles (of sizes greater than 5 micron) in the calibration. In paragraph 4, figure 3 is presented, along with a statement that, for smaller particles, large amplitude Mie oscillations are observed. Nowhere is it explicitly said whether spherical particles are, or are not, used for the calibration in the size range below 1 micron. The oscillations in figure 3 and the Lurton paper imply that in this smaller size range spherical particles are used - but then one is left wondering why the emphatic statement is made that real atmospheric particles are not spherical, and indeed repeated in paragraph 5 of this section.

I do not understand why reviewer #2 queries the departure of the data in figure 3, at 7 microns, from the response curve, when I understand the response curve to be the fitted curve. On the other hand the sentence that you quote from the manuscript, in response to their query ("For the larger sizes the evolution of ..."), is to me confusing. What is causing the diffraction, if not the scatterer? But since diffraction is itself a scattering phenomenon, it is not a valid explanation to say that the scattered flux is less than that according to Mie theory because diffraction dominates.

In your response, where you say "The reviewer has badly misinterpreted figure 3", I agree that the scattered signal does increase monotonically when the size increases provided you look just at the power law fit. But the data displayed do not follow the power law fit at the smaller sizes. (Indeed were the smaller particles, which I am guessing to have been spherical, used in the fit?) Looking at the data between about 0.8 and 2 microns, it is easy to see why the reviewer is asking this question. In the same paragraph of your response, where uncertainties are discussed, I find the discussion confusing. It appears that what you mean by a noise of 20 mV is in fact fluctuation of the pulse height, and if I measured 400 such pulses I might expect an uncertainty in the mean of +- sqrt(20)/sqrt(400)=0.05 mV. If instead you mean that a noise of 20 mV standard deviation is additively combined with your 20 mV pulses, then you might be able to still extract an average pulse height with an uncertainty of 1 mV. However either of these scenarios is only valid if the particles in question are known to be monodisperse. Now if you take the instrument into the field, the counting is done with particles of many sizes, and you need to be able to classify a particle based on a em\{single}detection, and presumably the 20 mV noise is still there. In the field, any averaging must necessarily be the averaging of size distributions.

In regard to section 2.4, I share reviewer #2's concern that many unsupported statements are made about the principles behind speciation. You state in your response that laboratory measurements have been made to support what you write, but these are not referenced or presented in the manuscript. For example, a notable omission is a figure (or a reference to one) for the 60° data that is counterpart to figure 3 for the 12° data. This needs to be remedied.

The statement that irregular particles do not result in "Mie oscillations" needs to be

toned down. For practical purposes (in this instrument) it may be true, but it is not in general so. The group of Ping Yang (DOI: 10.1175/JAS-D-12-039.1) has made available a database of scattering matrices for ice crystals, some of which are irregular aggregates of plates and columns. The smaller aggregates do show such oscillations. I suspect that the finite field of view of the detectors will average out any oscillations, especially for the medium and larger size particles, so that you do not need to worry about them, but this of course needs to be checked. The evidence that you cite in the PROGRA2 database (e.g. the curves in the website gallery or the Applied Optics paper by Daugeron et al. 2006) is not convincing because the angular resolution of the scattering and (de)polarisation curves is insufficient.

The Magnus effect is a consequence, not a cause, of a particle spinning. The work that you cite in response to reviewer #2 (Muto et al.) also is consistent with that. Your observations are much stronger evidence for the particles spinning, and ought to be included or referenced.

I am also curious about the statements about the overall mass of the instrument and the dimensions of the Delrin optical chamber given in response to a question by reviewer #1. A box of Delrin this size (20cm x 10 cm x 5 cm) seems by itself to be heavier than 350 g even if hollowed.

In section 2.3, paragraph 2, what are the small peaks referred to in figure 4?

This next comment is less in my role as an editor, and more as an interested reader: I have some concerns about the adequacy of figure 1. that are not addressed by reference to Lurton et al. To me the amount of detail given in the schematic drawing is inadequate. For example, there is no detail on the nature of the tubes leading from the scattering region to the detector(s). The presence (or absence) of baffles is important in understanding the field of view of the detectors, since scattered photons that enter the tubes and strike the walls of the tubes may in fact still arrive at the detectors because they strike the walls at near grazing incidence (if there are no baffles).

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Interactive comment on Atmos. Meas. Tech. Discuss., 8, 1203, 2015.