

## ***Interactive comment on “Single Particle Soot Photometer intercomparison at the AIDA chamber” by M. Laborde et al.***

**Anonymous Referee #2**

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General comments:

This paper seems overly long, and makes for tedious reading. An intercomparison between six units of the same instrument hardly seems publication-worthy. Far more interesting and useful is a comparison between different types of instruments, for example, the Boston College studies.

It should be noted that the second BC study (Cross et al., AS&T 2010) actually compared 3 SP2s against a CPMA (particle mass measurement), and stated that differences in response were merely due to a choice of detector gains. As for scattering diameter estimates, a similar consideration (appropriate calibration) applies.

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There are certain interesting sections in this manuscript – for example, the comparison of Aquadag and fullerene soot calibration-based estimated BC mass. But that follows the authors' earlier manuscripts, and as such, could just be published as a note, rather than a full-blown manuscript.

A good portion of the current submission essentially boils down to: “Follow the manufacturer's instructions for setting up and maintaining your SP2.” That is not science.

Some specific comments follow:

Page 3524:

Line 12: the verb is “incandesce”, not “incandescence”.

Line 13, minor quibble: Thinking of how the SP2 measures mass, any method other than gravimetry will be indirect (and nobody is going weigh individual BC particles, and even if that happened, there are interferences due to non-BC materials). That qualifier/description struck me as unnecessary.

Lines 20-21: “several research groups” – who are these groups? What is “several” – 5? 25? What modifications have they made? Are any of them included in this study?

Line 24: “two elements APD” – it's actually four elements, of which either two elements are used (e.g. Gao et al. 2007), or the four elements are wired to output two signals.

Page 3525:

Lines 14-15: “Both amplification gains can be varied while the high-to-low ratio remains constant” – this is not easy, as the gains are not linear settings. Further, this practice seems unnecessary.

Lines 24-25: “The low gain output...” – do the authors suggest that the high gain channels from the older SP2s are not the same as the high-gain channels from the C\* SP2s?

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Section 2.1.3: The laser power adjustment on page 3527 is the only adjustment that may not seem standard manufacturer's recommendation. Even then, the laser power is always maximized – not just with the pump laser current, but also with the pump laser temperature. I am not certain that the color temperature is dependent on laser power, except perhaps at very low laser power settings, where one runs into detection efficiency issues anyway – not advisable! All of this section can be deleted.

Page 3528, Line 18: That should be “NIOSH 5040”, not “NIOSH 940.” Further, the NIOSH 5040 does not recommend different temperature set points and residence times, either in itself: <http://www.cdc.gov/niosh/docs/2003-154/pdfs/5040.pdf> or in the reference: <http://www.cdc.gov/niosh/docs/2003-154/pdfs/chapter-q.pdf>

Page 3532: Lines 4-5: Couldn't the authors test the CAST soot with an APM/SP2 combination?

Page 3533, Lines 15-16: LEO stands for Leading Edge Optimization. That sentence does not mention “optimization”. Maybe the authors meant to say “using the Leading Edge Optimization (LEO) method”?

Page 3534, Lines 6-7: “the LEO fit was performed up to 2  $\mu$ s before...” – the justification for this choice is not clear, and perhaps even wrong. For example, with thickly-coated BC, the “incandescent lag” – the gap between the scattering signal peak (after which the coating evaporation is obvious) and the incandescent peak is about 4  $\mu$ s. The rise to incandescent peak is very quick. Here's Gao et al. (2007):

“The chosen end point for the LEO fitting (end of thick line) must occur before the particle boiling point is reached. The choice of about 2.5  $\mu$ s before the coating starts vaporizing yields an amplification factor of approximately 30, consistent with the first method.”

So I don't think the authors' method for LEO is optimal.

Reference:

C1954

Gao, R. S., Schwarz, J. P., Kelly, K. K., Fahey, D. W., Watts, L. A., Thompson, T. L., Spackman, J. R., Slowik, J. G., Cross, E. S., Han, J. H., Davidovits, P., Onasch, T. B., and Worsnop, D. R.: A novel method for estimating light-scattering properties of soot aerosols using a modified single-particle soot photometer, *Aerosol Sci. Tech.*, 41, 125–135, 2007.

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Interactive comment on *Atmos. Meas. Tech. Discuss.*, 5, 3519, 2012.

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