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Atmospheric Measurement Techniques Discussions

Interactive comment on "The performance and the characterization of Laser Ablation Aerosol Particle Time-of-Flight Mass Spectrometry (LAAP-ToF-MS)" *by* Rachel Gemayel et al.

Anonymous Referee #1

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Review of

The performance and the characterization of Laser Ablation Aerosol Particle Time-of-Flight Mass Spectrometry

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Atmospheric Measurement Techniques

This manuscripts describes new design parameters of a real-time laser ablation instrument for atmospheric particles. The particle inlet allows analysis of particles ranging from 80-600 nm. Yet, in this configuration, only particles > 350 nm are detectable. Thus, only particles 350-600 nm are actually detected by this instrument, which is quite

restrictive. This instrument does provide some advantages over other LA systems, but it is unclear if the positive aspects outweigh the severely restricted size range. Combining the strengths of various systems would be ideal, if this is possible. It would be very useful to see a discussion of this sort in the paper.

Primary Comments:

1) More description is needed for the inlet system. For example, is the aerodynamic lens a commercial system or home built? If the former, then please provide a literature reference. If the latter, then please provide specific dimensions so that this could be replicated.

2) Line 89-91: "[This technique] would allow . . . quantitative information about ambient particle ensembles. . ."

I disagree that any "quantitative information" could be obtained from this instrument. First, only 2.5 % of the particles are detected. Of those, less than 2/3 are analyzed (hit rate). Second, of the particles which are hit by the laser, there is no evidence that 1) the particle is fully ablated or 2) the species of interest are fully ionized for MS detection. Unless the authors provide other compelling evidence, I would consider this instrument qualitative, similar to other LA instrumentation.

3) The authors do a good job of comparing their technique to other airborne LA techniques. However, since the focus is largely on trace elements (more so than most other LA techniques), the authors should also compare (both positives and negatives) their technique to advances in airborne single particle ICPMS. For example, Myojo T et al, 2002, Aerosol Science and Technology and Suzuki, Sato, Hiyoshi, and Furuta, 2012, Spectrochimica Acta Part B, among several others.

Other Comments:

1) The English needs fine tuning. There are more than several errors, including references not capitalized (line 54), incorrect grammar (line 63), sometimes the reference

is after the punctuation and sometimes before, and other issues which need to be addressed.

2) Line 199: The authors claim “excellent repeatability” for the hit rate. Please explain why an RSD of 18% is considered “excellent” in this application as, in most instrumental analyses, this is not the case.

3) Lines 205-208 and Figure 3: The authors state that, after 4 weeks, the hit rate goes to 0%. Yet, Figure 3 only goes out to 12 days. The authors should either adjust Figure 3 or specify that these data are “not shown” as this could be confusing to the reader.

Related, it would be useful for the authors to give some context as to why the hit rate would be “constant” for 8 days after refilling the laser only to suddenly drop thereafter.

4) Line 244: Authors mention “sulfate” in the text, but show “bisulfate” as the chemical formula in parenthesis.

5) Line 303-306: Can the authors elaborate on which design parameters are responsible for the improved hit rate relative to SPLAT? And, in which size range are these improvements observed?

2/15/2016

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