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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On-line single particle analysis of ice particle residuals from mountain-top mixed-phase clouds using laboratory derived particle type assignment" *by* S. Schmidt et al.

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Editor comment:

After the authors posted their responses to the referee comments on MS No.: acp-2016-365, I received a "follow-up comment" from one of the referees of this manuscript. To expedite the review process, I am posting this "follow-up comment" as an editor comment. The authors of MS No.: acp-2016-365 should also consider this "follow-up comment" when revising their manuscript. Below is the initial comment from the reviewer, the response by the authors, and the "follow-up comment".

Initial comment from the reviewer:

3- In Section 2.3 when discussing the lab spectra it is stated "only those mass spectra that represented the majority of the different fragmentation patterns were considered". What % defines majority? Moreover, this % should be used to define a correction factor (with corresponding uncertainty propagated into the result). If only 60% of particles were measured for salt but 90% for soot, then the reported IPR numbers should be scaled up by 1/0.6 and 1/0.9 respectively.

Response by the authors:

Correction of the reported IPR numbers can't be done in this way. First, we might end up with more particles that were actually measured. Second, it is only possible to assign marker ions of atmospheric particles to those reference spectra containing these marker ions. Speculating about spectra not containing the markers would only increase the uncertainty.

"follow-up comment" from the reviewer:

I'm not quite convinced by the arguments in this response. It would be appropriate to end up with more particles than were measured, if a correction for missed particles was applied. There are 2 ways the ALABAMA may miss particles. The obvious way is if the instrument only obtains a signal for, say, 10% of all particles (90% "missed"). Then a correction factor 1/0.1 should be applied. The second way is if the instrument obtains a full mass spectra (with marker ions) for 10 of 100 biomass burning particles and only a partial mass spectra (eg no signals except potassium) for 30 of 100 biomass burning particles. We can suppose that their remaining 60 are missed particles. In the second case, the correct number to be reported for the biomass burning class is 10*1/0.1. There is ambiguity about what to do with the 30 partial mass spectra: they could be put into a category "other" but careful thought would be needed before reporting total estimates for number concentration.

It is essential to include such a correction factor if the goal is to compare the abundances of different particle types. If a correction factor is not applied, it must be proven that the conclusions are unchanged by its omission – it is only valid to omit the correction if it would be similar for all particle types, in analogy to weighting or not weighting a linear regression.

Therefore I still think that the manuscript should include a report (discussion/table/graph) of the fraction of laboratory samples which included the marker ions and which did not. If this fraction was very different between classes, the comparative statistics in the abstract (13% dust, 3% aged particles in IPR) would be incorrect.

There is another subtlety which I would like to state but do not expect the authors to address. A biomass burning particle missing certain markers may possibly be classified as aged, which would mean a second-order correction of overlapping particle classes could be made (this is just an example, and not based on the authors' marker ions). Without this correction, particle type numbers could indeed be overestimated, but I imagine this would be pushing the data analysis past what is reasonable.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-365, 2016.