

Interactive comment on “Organic Functional Groups in the Submicron Aerosol at 82.5° N from 2012 to 2014” by W. Richard Leaitch et al.

Anonymous Referee #1

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Summary and Overall Recommendation:

Multi-year studies, such as the one presented here, on the chemical composition of submicron aerosol are highly needed in the literature and I feel are at times underappreciated by the atmospheric chemistry community. This study is highly unique in that it presents multi-year data (i.e., April 2012 to October 2014) on the organic functional groups (OFGs) found in submicron aerosol collected from the Arctic at the Alert Observatory. A lot of important observations are made about OFGs in submicron aerosol collected from the Arctic during this study. For example, the authors found that a secondary marine source is likely a general feature of summer OM, but during years where there is likely more combustion-related sources (such as biomass burning) the contribution of alkane groups to the OM can be higher. Overall, I think this study will

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be publishable in ACP. However, I do a few comments below that the authors should consider before full publication is considered.

1.) Generally, I feel at times the text in the discussion section can be a bit dense and hard to follow. I couldn't think of an easier way to reorganize the text, but I just thought to point this out to the authors, especially if this comment concerned them enough to consider reorganizing the discussion section.

2.) Abstract, Line 12: Change "lower organic mass concentrations (OM)" to "Lower organic mass (OM) concentrations"

3.) Abstract, Lines 16-17: If you are going to list the initial of the month in parentheses after each season, shouldn't you also do this for summer to be consistent?

4.) Abstract, Line 25: change "most persistence" to "most persistent"

5.) 2.1 Instrumental Methods, Page 4: What was the temperature of the freezer at the Observatory? This is important to know so that readers can judge if potential changes in composition might have occurred.

6.) 2.1 Instrumental Methods, Page 5: When the authors state "Prior to OFG analysis by FTIR spectroscopy, the filters were equilibrated in a temperature and humidity-controlled cleanroom environment for 24 h," what do you mean exactly? Is the temperature and RH always the same for all samples measured? What is the temperature and RH of this room? Does this change the composition since the aerosol were likely collected at much colder conditions in the Arctic?

7.) 2.1 Instrumental Methods, Page 5:

Did the authors consider conducting ammonium sulfate calibrations with the ACSM? Budisulistiorini et al. (2014, AMT) found this was necessary with the multi-year measurements of submicron aerosol in the southeastern U.S.

8.) Related to # 7 above, why didn't the authors consider presenting and comparing

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OFG data with PMF analysis of ACSM OM? Was it that the signals were too low for PMF analyses?

9.) For OFG analyses by FTIR, one thing that really never comes across are the uncertainties of this technique, especially at low mass concentrations. Are the uncertainties accurately estimated? I worry that FTIR may have issues at these lower mass concentrations, which can affect all of the downstream analyses you conduct in this manuscript. This also relates to how well your peak-fitting method really works when you are limited by low amounts of OM collected on these filters. Are you missing any important functional groups? I would think offline mass spectral analyses of these filters should be something the authors consider in the future (not in this manuscript of course).

10.) When you say a "Mixed" factor this is very confusing to me. Is this mixed because PMF fails to resolve this potentially mixed statistical solution? I wonder if this is the case due to the robustness of OFG analysis by FTIR. As the authors now, this OFG analysis is not as specific as mass spec in resolving finer details in the chemistry. I guess this "mixed" factor is resulting from this underlying issue with OFG analysis by FTIR.

11.) I'm intrigued by the authors potential observation of secondary marine OM source. Do the authors think this could be BVOC-related emissions from plankton?

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