

Interactive comment on “Long-term sub-micron aerosol chemical composition in the boreal forest: inter- and intra-annual variability” by Liine Heikkinen et al.

Anonymous Referee #1

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This manuscript reports an ACSM measurement study of sub-micron particles conducted at the SMEAR II atmospheric research station of Finland for a period of 7 years from 2012 to 2018. Discussions are made on temporal, diurnal and seasonal variations of PM1 components, gaseous compounds including NO_x, SO₂, and monoterpene, and meteorological parameters such as temperature, solar radiation, wind speed and wind direction. Additionally, the influence of radiation, temperature and wind direction on major aerosol and gaseous species are examined. This is a worthy paper and a timely submission as it reports the longest online measurement data, to date, on sub-micron aerosol chemical composition in a boreal environment. It is suitable for publication on ACP and I recommend acceptance by the journal after the authors respond to the

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following comments.

While the title of the paper highlights aerosol chemical composition, the discussions focus more heavily on the inter- and intra-annual variations of PM1 mass loading and meteorological conditions. The authors mention that more detailed discussions on organic aerosol factors determined from analysis of the ACSM mass spectra will be presented in a separate paper. While this decision is understandable considering the length of current manuscript, it is important that relevant discussions, such as biomass burning organic aerosols, are backed by measurement data such as variations in the ACSM f60 time series.

Another issue is that this manuscript cites a lot of previous publications from SMEAR II but sometime without providing sufficient contexts. Readers who are less familiar with the literature may feel somewhat lost or unconvinced. In addition, the creation of an Appendix section and the placement of several figures in there appear a bit haphazard and may introduce confusion. More detailed comments are given below:

What type of interpretation was applied to the image plots in Figure 1?

Line 182, change “evaporating at 600oC maximum” to “flash evaporate at 600oC” .

Line 184, add “of” after “the signal”.

Line 224, spell out the acronyms that have not yet been defined.

Line 307, it is briefly mentioned that large discrepancies between ACSM nitrate and MARGA nitrate were observed, likely introduced by organic nitrate. This is quite interesting, it would be helpful that the authors provide a bit more details and expand the discussions.

Line 316 – 317. Is this sentence referring to the CE values used in this study or those typically used for ACSM measurements?

Line 322, give details on how DMPS-derived mass concentration is determined

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Lines 328, 330, change “Figure 1a” to “Figure 2a”.

Check the texts at Line 357 – 358.

Figure 2 caption, check the text for (b)

Figure 4, what's the explanation for the large year-to-year variations in average SO₂ concentration?

In 2017, SO₂ was nearly 0 in all months but sulfate concentration was not too much different than those in the other years. Why so?

Line 476, “exceptionally long-lasting period with high atmospheric pressure”, can this statement be a bit more quantitative, i.e., what does exceptionally long-lasting mean?

Line 479, quote the 7 year mean July temperature.

Line 699, what Figure c?

Line 702, revise this sentence “ but shows . . .”

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-849>, 2019.