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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric black carbon and sulfate concentrations in Northeast Greenland" *by* A. Massling et al.

Anonymous Referee #1

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This study reports on 2+ years of near-surface air measurements of black carbon (BC) and sulfate collected at Station Nord in Greenland. The study also evaluates simulated aerosol fields at this location from one model (the DEHM). The authors find a strong correlation between BC and sulfate, suggestive of a high degree of internal mixing of these two species. The seasonal cycle of aerosol concentrations reported here is similar to that seen at other Arctic monitoring stations. The paper presents new measurements of BC at Station Nord that were determined with a less commonly used instrument (a Multi Angle Absorption Photometer), and it will be useful to have a reference associated with these measurements for future applications of the data (e.g., for model evaluation). Overall, I find the study suitable for publication, with only minor comments listed below. In general, these comments relate to requests for more precise text

to enhance the clarity of the paper. Although it would be nice to know how other aerosol models perform at this location during the period of measurements (2011-2013), I see such an evaluation as beyond the scope of the current study. I suggest, however that the authors indicate where or how their measurements can be obtained, so they can be utilized by the broader scientific community.

Minor comments:

Abstract: I strongly suggest including more quantitative results in the abstract. It is currently written in very general terms. For example, what are the seasonal mean and standard deviations of the aerosol concentrations? What is the correlation between BC and sulfate? How well did EC and BC measurements "compare"? How "good" was the agreement between modeled and measured concentrations of BC and sulfate?

Section 4.4: It is quite interesting that measured BC and sulfate concentrations are more highly correlated than measured BC and EC concentrations!

11467,4: The reference to Flanner et al (2011) is not appropriate here, as this particular paper did not explore BC impacts. Flanner et al (2007, doi:10.1029/2006JD008003) would be more appropriate.

11467,10: "unless they do not appear in very large sizes" -> This would be more clear if it were worded as "unless they appear in small sizes". Would this change retain the meaning?

11467,22: "IPCC, 2013" -> The accepted practice is to cite specific chapters of the report.

11468,16: "However, Huang et al (2010) demonstrated that a reasonable agreement can be obtained..." - Were there any specific model adjustments made by Huang et al to achieve this agreement? Otherwise, I suggest at least listing the model applied in that study.

11468: Somewhere in this section I suggest clarifying that the measurements reported

in this study were made near the surface, and that radiative forcing depends on the entire column burden.

11470,11: "Multi Angle Absorption Photometer" has already been referred to as "MAAP" earlier in the sentence, so it doesn't need to be spelled out again.

11470,13-20: Logically, this paragraph could perhaps be moved up or merged with the earlier discussion on ranges of measured absorption coefficients for BC, before describing the absorption coefficient value that was settled on (lines 8-12).

11471,15: Please briefly describe the sources of this local pollution.

11473,24: Please describe these "simple time profiles". How, precisely, are the annual emissions distributed in the model?

11474,6: "Particle diameter of 1um". Earlier (11473,10), 1um is listed as the upper particle size limit. Please describe more precisely the BC and sulfate particle size distributions that are assumed in the model.

Section 4.1, first paragraph: I suggest listing the annual mean values in this paragraph, to accompany the maximum and minimum values that are already listed. (Later, seasonal mean values are described.)

11477,12-18: Are there any other indications or evidence of biomass burning that could be introduced to support this discussion?

11478,3: "about 1.5 to 2.5 higher" - Do you mean: "a factor of 1.5 to 2.5 higher"? If not, please include units.

Section 4.3: Emissions are described as being the likely source of model-measurement differences, but the model physical representations could also be responsible for some or most of these differences, and I think this should be acknowledged more clearly here. This seems especially true because the model represents BC and sulfate as external mixtures, whereas the observations seem to indicate an internal mixture.

11478,21: "changes in the emissions" - Do you mean interannual changes in emissions? Please clarify.

Related, were year 2011-2013 emissions from GFED applied in this study? Please specify this on p.11473.

11480,3: What is meant by "modern fuels"?

11481: "Sibiria" -> Siberia

11481,20: "and and"

11481,23: "substantially higher": By what factor? As with the abstract, please quantify the key results in Conclusions.

11482,15: "...The most likely reason for this discrepancy is seen in the uncertainty of emission inventories for black carbon..." - The modeled sulfate concentrations are also substantially higher then measurements, however, so it seems the error in modeled SO4/BC ratio is also associated with sulfate biases. This should be acknowledged. More generally, I wonder if some of this bias could be associated with the model treatment of BC and sulfate as external mixtures (?).

Figure 2: It would be helpful to add a subfigure showing a scatterplot and correlation statistics, similar to Figure 1.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 11465, 2015.