Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-65-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source Sector and Region Contributions to Black Carbon and $PM_{2.5}$ in the Arctic" *by* Negin Sobhani et al.

Anonymous Referee #2

Received and published: 16 March 2018

General comments:

The study adds to a growing list of studies on impacts of regional emissions of black carbon on Arctic aerosol concentrations. Given large yet uncertain impacts of aerosols on Arctic climate, there is considerable need for research in this area.

The authors of the study analysed at a wide range of model results in great detail. However, there are substantial issues with the design of the study and presentation of the results.

First, important sources of black carbon were not accounted for. Consequently, BC concentrations in the Arctic are underestimated and conclusions about the relative importance of different source regions are biased. More specifically, emissions of black

Printer-friendly version

Discussion paper

carbon from central and south America, central and south Africa, and Australia are not accounted for in the simulations. According to data sets that were used by AMAP for an assessment of the impacts of black carbon on the Arctic (AMAP, 2015; Stohl et al., 2015), these regions contributed about 41% to total (anthropogenic and natural) global emissions of black carbon in 2010 (AMAP region "ROW"). The simulated contribution of these emissions to total black carbon mass in the Arctic atmosphere is 10-20%, depending on the model. Furthermore, it appears that emissions associated with oil and gas flaring were not accounted for. According to AMAP data, about 65 GgC of black carbon were emitted by oil and gas flaring industrial activities in 2010, especially from sources at high latitudes. Efficient transport to the Arctic implies that the contribution of these emissions to total black carbon mass in the Arctic atmosphere is relatively large, i.e. comparable to the impacts of emissions of black carbon from all North American sources.

Second, the analysis of model results in the study seems narrowly focussed on results for the Arctic, which is problematic with regard to an improved understanding of the impacts of aerosols on Arctic climate. For instance, aerosol radiative effects at mid latitudes have a strong impact on Arctic climate by influencing the transport of heat to the Arctic. There are also impacts of black carbon on snow albedos, which are not considered either.

Finally, it is not obvious why results for PM2.5, PM10, sulphate, and dust concentrations are analysed. Comparisons with observations are missing and analysis of model results for these is less complete than the analysis for black carbon. Overall, the relevance of model results is not obvious with regard to impacts of aerosols on climate or air quality. There is no discussion of climate implications or comparison with results from other studies.

Specific comments:

P. 2, L. 22: Another great reference is Bond et al. (2013).

P. 3, L. 10: Sharma et al. (2013) and references given in that paper seem relevant here, too.

P. 3, L. 27: Uncertainties and biases in parameterizations of wet deposition and convective processes should be more emphasized here since they mainly explain differences in simulated aerosol concentrations in the Arctic. See Browse et al. (2012) and Mahmood et al. (2016) and references in these papers.

P. 3, L. 28: The statement that regional chemical transport models capture BC concentrations better is highly questionable. According to Eckhardt et al. (2015), there is no single class of models that outperforms other models. The statement here is based on 2 individual models.

P. 3: The discussion in this section is focussed on simulations of surface concentrations. For climate and radiative forcings, aerosol vertical distributions and deposition of BC on snow are more important than surface concentrations. Please clarify the focus of the study on air quality aspects of Arctic aerosols or include further information regarding aerosol vertical profiles. This is particularly important since comparisons for aerosol vertical profiles are included in the paper (Section 3.1.2). The title and abstract are somewhat ambiguous in that regard.

P. 4, Section 2.1.2: Please indicate whether annual mean emissions are used. Are temporal variations in fire emissions are accounted for?

P. 5, Section 2.1.3: This section lacks a lot of detail. What types of aerosols are consider and how they are numerically represented? Does the model simulate aerosol size distributions? What kind of aerosol processes are accounted for? Is aging of BC accounted for? How are interactions between aerosols and gas-phase chemistry represented?

P. 7, Section 3.1.2: Comparisons for BC are included here but similar comparisons for sulfate are missing. The latter needs to be added (e.g. by comparing sulfate concen-

trations in the Arctic with observations) because much of the subsequent discussion in the manuscript also addresses sulfate. It is not clear how well the model simulates non-BC aerosol species. Similar, it is not obvious that simulated results for PM are realistic. Without validation it would be better to remove results for sulfate and PM from the paper altogether.

P. 11, L. 23: Figure 10 shows the seasonal cycle of aerosol concentrations at the surface. A similar figure needs to be added for the vertically integrated amount of BC in the Arctic. This information is necessary for the interpretation of the relative contributions of different regions to vertically integrated BC in Fig. 13. The meaning of relative seasonal variations in Fig. 13 is not clear without this information.

P. 12, L. 10: Please describe what is meant by a "sensitivity analysis". Were simulations repeated with modified/masked emissions using the same meteorology? Did the approach account for non-linear interactions of aerosols and trace gases from different regions? Did you verify that BC concentrations from individual model experiments with modified emissions add up to the concentrations obtained by running the model with all emissions included?

P. 13, L. 16: Fig. 12 seems to show key results for this study. However, the figure is confusing and needs to be cleaned up and explained. Several sub-panels are included but not explained. Would it be possible to include a table with annual mean results in order to summarize some of the results in this figure more clearly? Also, it would be very useful to compare results to multi-model results from AMAP (2015) for 2010. According to AMAP (2015), domestic emissions from East and South Asia are the largest source of annual mean BC in the Arctic. Emissions from Russian fires, Russian oil and gas flaring are also important according to AMAP (2015). Contributions of emissions from Nordic countries and the rest of Europe are much less important than contributions from Russian emissions. However, results in Fig. 12 show similar relative contributions for European and Russian emissions. Is it possible that a lack of Russian oil and gas flaring emissions in WRF simulations might account for this differ-

Printer-friendly version

Discussion paper

ence? Furthermore, the WRF model domain does not seem to include emissions of BC from fires in Africa and South America. According to AMAP (2015), contributions of BC emissions from the rest of the world (i.e. mainly Africa and South America) to Arctic BC burdens are similar to contributions of BC emissions from North America and Europe. Overall, it seems that results from AMAP (2015) are quite different from results simulated by WRF.

P. 13, L. 28: Vague. Overall, the context of the study is not clear (air quality vs climate processes).

P. 14, L. 22: The title of the section is misleading since no transport pathways are actually analysed in this section. It is not clear how the discussion of concentration cross sections helps to explain the transport of aerosols to the Arctic without information about the simulated circulation. Typically, advection leads to import of BC mass to the Arctic in some regions and export in other regions (e.g. see Iversen, 1996).

P. 15, Section 4: A critical evaluation of results and comparisons with results from other studies is missing. Contributions of emissions from different geographic regions and emission sectors have been studied before (see references in introduction and review comments above). It is not obvious whether the current study adds any significant new results?

P. 29, Fig. 1: Please indicate the base year of the emissions. It seems that emissions due to oil and gas flaring are not accounted for? These emissions are an important source of BC in the Arctic (AMAP, 2015).

P. 31, Figs. 3 and 4: These figures are confusing and need to be modified or replaced. First, very large regions with considerable meteorological and chemical variations are covered by each individual flight track so that mean results for individual flights are not very meaningful and difficult to compare with each other. Second, what is the purpose of using a time axis? Can results for individual days be used to understand the temporal evolution of plumes of polluted air? Finally, the large amount of information

in the figure is overwhelming. A much better way to present the data would be to produce plots similar to Fig. SM2, with meteorological variables plotted along flight tracks. Furthermore, it is not obvious that a comparison of meteorological results is necessary for the validation of simulated concentrations? Perhaps these figures can be moved to the supplementary document?

P. 42, Fig. 14: Why are results for dust shown in this figure? Dust has not been addressed before in the paper. In particular, it is not clear how well the model simulates dust. Furthermore, the labels in the figure are much too small and so it is not clear what longitude range is shown. Locations of Eurasia and Siberia are not clear. What are the units of the concentrations?

Technical corrections:

There are several grammar mistakes. A few examples are given in the following. It will be necessary to check the text for additional grammar mistakes.

P. 5: L. 9: "been" missing. L. 10: remove "the" L. 11: "an" missing. L. 16: "detail" L. 15: "The model used..." (past tense) vs "The model includes..." L. 15: missing "a"

P. 7, L. 27: Redundant information. Tables 1 and Fig. SM2 were already mentioned in the previous section.

P. 8, L. 19: A reference is missing for the MAC value used in the current study.

P. 14, L. 25: "Concentration" instead of "magnitude".

P. 36, Fig. 8: Values on the map are mentioned but are not actually visible in the plots?

P. 38, Fig. 10: What is OPM2.5?

P. 40, Fig. 12: Abbreviations BBSI, BBEU etc. in the figure need to be explained in the caption. It is not clear what these represent.

References:

AMAP: AMAP assessment 2015: Black carbon and ozone as Arctic climate forcers, Oslo, Norway., 2015.

Bond, T. C., et al. (2013), Bounding the role of black carbon in the climate system: A scientific assessment, J. Geophys. Res. Atmos., 118, 5380\u20135552, doi:10.1002/jgrd.50171.

Browse, J., Carslaw, K. S., Arnold, S. R., Pringle, K., and Boucher, O.: The scavenging processes controlling the seasonal cycle in Arctic sulphate and black carbon aerosol, Atmos. Chem. Phys., 12, 6775-6798, https://doi.org/10.5194/acp-12-6775-2012, 2012.

Iversen, T., 1984. On the atmospheric transport of pollution to the Arctic. Geophysical Research Letters, 11:457-460.

Mahmood, R., K. von Salzen, M. Flanner, M. Sand, J. Langner, H. Wang, L. Huang, 2016, Seasonality of Global and Arctic Black Carbon Processes in the AMAP Models, J. Geophys. Res., doi:10.1002/2016JD024849.

Sharma, S., M. Ishizawa, D. Chan, D. Lavoue, E. Andrews, K. Eleftheriadis, and S. Maksyutov (2013), 16-year simulation of Arctic black carbon: Transport, source contribution, and sensitivity analysis on deposition, J. Geophys. Res. Atmos., 118, 943–964, doi:10.1029/2012JD017774.

Stohl, A., Aamaas, B., Amann, M., Baker, L. H., Bellouin, N., Berntsen, T. K., Boucher, O., Cherian, R., Collins, W., Daskalakis, N., Dusinska, M., Eckhardt, S., Fuglestvedt, J. S., Harju, M., Heyes, C., Hodnebrog, O., Hao, J., Im, U., Kanakidou, M., Klimont, Z., Kupiainen, K., Law, K. S., Lund, M. T., Maas, R., MacIntosh, C. R., Myhre, G., Myriokefalitakis, S., Olivie, D., Quaas, J., Quennehen, B., Raut, J.-C., Rumbold, S. T., Samset, B. H., Schulz, M., Seland, O., Shine, K. P., Skeie, R. B., Wang, S., Yttri, K. E., and Zhu, T.: Evaluating the climate and air quality impacts of short-lived pollutants, Atmos. Chem. Phys., 15, 10529-10566, https://doi.org/10.5194/acp-15-10529-2015,

2015.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-65, 2018.

Printer-friendly version

Discussion paper