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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Deposition of ionic species and black carbon to the Arctic snow pack: Combining snow pit observations with modeling" *by* Hans-Werner Jacobi et al.

Anonymous Referee #2

Received and published: 16 May 2019

Overall assessment:

This paper describes a detailed study of sea salt ions and black carbon (BC) deposition on two glaciers of northern Svalbard, and combines snow pit observations with aerosol measurements at Zeppelin station to estimate the relative contributions of wet vs. dry deposition to the observed budgets of aerosols in the snow pack. The paper addresses an existing knowledge gap with parallel measurements of airborne BC concentrations and accumulation in snow, which helps clarify the processes governing the air-to-snow transfer of these aerosols which are susceptible to affect the surface radiative balance, and hence polar climate. The contribution is therefore valuable, and, although not

ground-breaking or highly original, the paper is of sufficient interest to be published in ACP. Overall, the paper is well written, easy to read, neither too wordy nor too brief, and it adequately cites recent and relevant scientific literature. The figures are likewise simple, clear and easily understandable. The choice of methods is appropriate and good care appears to have been taken at most, if not all, steps of the various analyses to ensure results of quality. Sufficient information is provided to justify choices in the various data and parameters used in calculations. The main conclusions are also well supported by the data presented. I do, however, find some lacunae in some part of the method descriptions, as well as some ambiguities in a few parts of the manuscript. These need to be addressed before the paper can be accepted.

Specific questions and comments:

Section 2.1 (Snow sampling) Please explain the snow pit sampling strategy more clearly: Was sampling done at fixed depth increments, or within each stratigraphically distinct layer, or a combination of both ?

Section 2.2 (Snow analysis) How much type elapsed between samples collection, melting, and analysis for ions and for BC ? Please provide more information about the SP2 analyses. Some of this info can be placed in the Supplement. Show the Aquadag calibration curves. You report a mean aerosolization efficiency of only 56 %, much lower than the 72 % reported by Lim et al. (2014) with the same nebulizer. How come ? How variable was the efficiency between analytical runs ? Did you estimate the nebulization efficiency as a function of particle size, or just assumed it to be as described in Lim et al. (2014) ? If you did estimate the efficiency for different particle sizes, show the results. Otherwise, do you have any results from analyses performed on other samples (e.g., ice cores) with the same instruments that could be used to quantify these sources of uncertainty ? This is important since detection efficiency on the SP2 is often lowest for the largest particles that hold moss of the BC mass.

Section 2.6 (Dry and wet deposition) L172: Add the reference (Millero et al.) for the

standard seawater ionic ratios.

Section 3.2 (Impurity profiles in the snow pack) Please clarify what exactly the "adjustments" in the snow pit impurity profiles (Fig. 3) actually mean ? What was adjusted, and how ? This is not really clear.

Section 3.3 (Wintertime snow budgets and deposition of ionic compounds) L315: How was the 20 % figure estimated ? This comes across as a pure guess. Explain more clearly.

Section 3.4 (Wintertime snow budgets and deposition of BC) L355-356: Some of the differences discussed in this paragraph (with the data of Forsström et al.) are as likely or more likely to be due to the differences in methods (SP2 vs TOT) than to spatial variability.

Section 3.5 (Comparison of monthly snow budgets and deposition) L405: What model and forcing data are you referring to ? This sentence is unclear. L409-410: I disagree with this statement: I see no convincing evidence on Fig. 5 that there is a better agreement between the measured and calculated monthly snow pit budgets of nitrate and BC than for sodium. If you have some metrics to support this otherwise vaguely qualitative statement, provide them.

Section 3.6 (Variations of the chemical composition...)

L461-468: Rˆ2 is not the correlation coefficient, it is the coefficient of determination. If R = 0.60, Rˆ2 = 0.36, i.e. only 36 % of the variability is explained by the linear model. You also state that the average ratio of rBC to nssSO4 in snow is almost an order of magnitude than in the atmosphere. I presume that by the "atmosphere" you mean the aerosols at Zeppelin ? (shown on Fig. 7), if so please clarify the sentence. Also, please show the equations of the regression lines, so that the reader can actually compare the mean ratios (slopes).

Section 4 (Conclusion)

L538-539: Explain how you conclude that 1.2 ppb of rBC caused a marginal reduction of snow albedo. Provide a reference.

igures:

Fig. 2, 3, 8: I suggest adding small labels on the edge of the graphs, identifying the monthly subdivisions of the snowpack, for greater clarity.

Fig. 8: I suggest adjusting the x-axis scale on both panels to the same min-max range as on the Konsvegen panel (left-hand). The few large peaks in Br/Na on the Austre Lovenbreen panel (right-hand) obscure the variations of the baseline values, and makes the comparison between the two panels difficult. The peaks can be truncated and the peak values shown in labels.

See PDF attachment for suggested minor changes in wording in some sentences.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2019-215/acp-2019-215-RC2-supplement.pdf

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