

Interactive comment on “Spatiotemporal variability of elemental and organic carbon in Svalbard snow during 2007–2018” by Christian Zdanowicz et al.

Anonymous Referee #2

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Reviewer ID# Manuscript No.: ACP-2020-491 Authors: Christian Zdanowicz et al. The title of manuscript: Spatiotemporal variability of elemental and organic carbon in Svalbard snow during 2007-2018

General comments:

This paper is very informative and integrative. It includes a large dataset covering spatial distribution of concentrations (C_{snow}) and column loading mass (L_{snow}) for both EC and OC (water insoluble OC only) across Svalbard archipelago as well as temporal variation of C_{snow} over the period of more than 10 years (2007-2018) from central Brøgger Peninsula, involving 49 sites and 324 samples. Those data were used

C1

together with meteorological and aerosol measurements in combination with snowpack modeling. The variation of EC and OC deposition in Svalbard snow across latitude, longitude, elevation and time were investigated. The averages of snowpack loading of EC and OC across Svalbard for the winter of 2015-2016 were estimated. A range of possible snow scavenging rates of EC were obtained by comparing the estimated C_{snow} (via Cair) with the measured C_{snow} of EC. Overall, no spatial and temporal trends are detected. The authors dedicated lots of effort to this work and is worthy to be published. This work would make more data available in the arctic region (although with large uncertainties) and facilitate the arctic climate research. However, I have some concerns as follows:

- The weakest aspect of the work is the methodology itself, which could not be avoided likely. Thus, it weakens the impact of the work in general. To ensure a methodology able to archive the expected objective, standards/references should be run using the same procedure before any ambient samples is measured. Although there is no consensus on BC standard/reference, at least, some proxy of standards (e.g., graphite or fullerene soot or NIST SRM 1649a for EC; whereas water insoluble organic carbon are easy to find...) could be utilized. However, no information available in the paper as well as in the cited references regarding using standards/references in the filtration process, transferring BC particles from the melting snow water onto filters.
- The title should be modified since not the total organic carbon (OC) instead of only water insoluble OC contents were measured here (without knowing the filtration recovery rate). Authors should clarify the definition of OC through the entire paper accordingly.
- Due to many possible sources of errors in these snow sample processing and analyzing, it is needed including more error-analysis to get overall uncertainties properly, including but not limiting to filtration loss for both EC as well as water solvable OC, underestimated EC due to Fe₃O₄ (reduction-oxidation chemistry under 500 degree C) from dust & overestimated EC and OC due to carbonate etc.). As stated in Summary and Conclusion, no discernible spatial and temporal trends as well as patterns accord-

C2

ing elevation are obtained. It is not sure to what extent these statements are true or due to the large uncertainties caused by the methodology itself.

- In addition, authors include too many contents in one paper to keep the logic flow clearly and concisely and to link them via a main theme harmonically. It is noticed that due to a large dataset covering different spatial, temporal and discipline, too many names of site/glacier and terminology are introduced, making it hard to follow the paper through. For examples, 49 sites are indicated in Table 2, but only 19 sites on Fig. 1. (map). What are the relationship between Table S1, S2 & Fig. S2 (map)? Why are all site descriptions not integrated within one table and one map, clearly arranging them into three categories as follows; 1). 2015-2016: 7 glacier snowpack campaigns (22 sites) 2). 2008-2018: Brogger Peninsula surface snow (3 sites) 3). 2016-2017: 6 glaciers Northwestern Spitsbergen (? sites) The descriptions in section 2.1 "Field sampling" are not consistent with the descriptions on Table 2 for the 4 categories! Very confusing. . .

Hope these issues and concerns be addressed before the publication.

Specific comments:

L42: While its importance is realized for climate forcing, it is understood that BrC is the kind of organic carbon, including water-soluble organic carbon (i.e., WSOC), mostly polar organic compounds (e.g., oxygenated OC or organic nitrogen compounds or organosulfur compounds). However, the OC in this manuscript does not include the fraction of WSOC, which was lost by $\sim 80\%$ through the filtration process. Please re-write this part accordingly and relate BrC to the water insoluble OC as WIOC (e.g., solvent soluble OC are also part of BrC).

L60-65: There are some confusion here since this study followed the filtration method by Forsstrom et al (2009, 2013) and utilized the EC/OC analysis by EUSAAR_2 but providing the citation by Chow et al, 2004. Please clarify.

C3

L66: "OC" should be defined and clarified here

L70-71: I would prefer "scavenging ratio" to "washout ratio".

L75: it would be better replacing the section title with "2.1.1 April 2016 glacier survey"

L84: Fig. 2 is hard to follow; particularly for the caption, (the color in lower panel does not match these in upper panel). Please modify the figure to make it easier to understand or take it out (which not belongs to the key figures).

L87: please replace "stable oxygen isotope ratio (d18O)" with "stable oxygen isotope ratio (d18O) in water"

L104: Please spell out "ERA" for the first time.

L111: Suggest modifying the section title to "Surface snow monitoring (2007-2018), Brogger Peninsula"

L125: These additional samples in Table S2 are confusing as the sampling sites and the date are partially overlapped with these samples in Table S1. Are there any special purpose for those samples in Table S2? If not, it make more sense to include them in section 2.1.1 and change the current title to "2.1.1 2016-2017 glacier survey". Suggest to make 2 sub-sections: one is about the major survey in April 2016 and the other is about the additional irregularly sampling by NPI staff (2016-2017).

L130-140: As for EC and OC analysis of the snow samples, there are several steps involved, including snow processing (melting & filtration of particles, i.e., EC & WIOC on to filters) and EC/OC analysis. Large amount uncertainties would associated with these steps and the uncertainties of the snow processing are unknown largely. Author stated following the procedure by Forsstrom et al., 2009. However, the procedure by Forsstrom et al., 2009 was only for EC and the procedure regarding how to deal with the WIOC was not described, which is very important to this study. Author should provide more details of description for the snow processing steps, e.g., how many minutes were used for melting snow? and what kind of device was used for filtration of snow ?

C4

and how many minutes were used for the filtration? It is suggested to use some proxy of reference EC and WIOC (mentioned above) and ionized water to get the recovery rates for the filtration process.

L 136: The definition of OC mass measured (i.e., WIOC) should be clarified here also.

L141-153: It is suggested that more detailed error analysis is added here, as mentioned in general comments.

L67: Please modify the section title to “2.3 d18O analysis in snow water”

L168: Please replace the expression of “The stable isotope ratio of oxygen (16O:18O)” with “. . . (18O:16O)”.

L167-172: A sentence should be included here regarding why the authors should include d18O data in the study (Fig. 8). Otherwise, the d18O should be moved to “supplementary materials”, together with all support data. Thus, the main theme could be better presented without detracting from those supporting data.

L173-L216: It is suggested to move the content in section 2.4 to “supplementary materials” to make the theme stick out.

L219: Please use plain language to explain “skewness” as a statistic concept. What does it mean applying to your data?

L218-L230: This paragraph is full of numbers and hard to follow what the author would like to express. It is suggested either totally deleting this paragraph or focusing on the description of data structure including Skewness and LOD and how would the data structure affect the interpretation of this dataset and the comparison it with other studies.

L231: L75: Please modify the current title to “3.1 April 2016 glacier survey”, being consistent with 2.1.1.

L261-270: It is suggested to include all the annual median concentration data (i.e.,

C5

Csnow on EC and OC) from both Ny-Alesund and Austre Broggerbreen shown on Fig. 5c in a table, in order to better understand the paragraph.

L272: Ensure that the section title be consistent with 2.1.1 and 3.1

L273: the general statement for “no discernible zonal or latitudinal gradient of Csnow on EC or OC across Svalbard” is not convincing, as the uncertainties are not well investigated.

L278-279: highest among what ? This sentence is not clearly expressed. In fact, there is not much difference shown on Fig. 3 and 4.

L282-283: This statement seems not supporting by the data shown in Figs, S4-S5, and Figs S8-S9. As suggested, it is better to table all the data in the Supplementary Material to see clearly. L299-L302: Please check the numbers mentioned in L299-L302, which are not consistent with those in Fig 6a and 6b.

L272-365: The Section 4.1 is too long to follow, covering the discussion on different topics, including - surface Csnow of EC and OC - Lsnow on EC & OC and the relationship between Lsnow and hSWE - Snowpack modeling and simulated hSWE, and Lsnow (via extrapolating the relations shown in Fig. 6) - Simulated total EC and OC accumulated mass during the winter of 2015-2016 (Sept. 2015-April 2016?) across Svalbard and derived monthly and daily deposition rate (i.e., mg C m⁻² mo⁻¹ or mg C m⁻² day⁻¹) in the area. - Simulated snowpack profiles and related them to the cumulated mass of EC and OC (i.e., Lsnow of EC and OC) shown in Fig. 8 and Fig. 9 and discussion snow dynamics impacts.

I would suggest to re-arrange those contents in one section 4.1 into several sub-sections as 4.1.1, 4.1.2, 4.2.3 etc. accordingly based on topics mentioned above. Author may consider moving all snowpack modeling & snow dynamics related contents to the Supplementary Materials and briefly include the most relevant results here accordingly to make the main theme clear.

C6

L366 -427: Similar to section 4.1, suggest to re-arrange this section 4.2 into two sub-sections as: - L367-397: 4.2.1 “Temporal variation of C_{snow} on EC and OC” - L398-427: 4.2.2 “Scavenging rate of EC, Brogger Peninsula”

L395-397: It is suggested to begin with the sentence via using "To understand the possible role snowfall anomalies over center Brogger Peninsula..." instead of "To control for the possible role of snowfall rate. . ."

L429: Suggested to modify the section title as “Comparison of C_{snow} EC in Pan-arctic perspective”.

P34, Fig. 9 All the sites labelled in the plots here should be included in a map identified each of them. ALB3 could not found in Fig. 1.

In Supplementary Section: Under “EC and OC analyses: Additional information”: Uncertainties estimation and analysis should be paid more attention and include more discussion as mentioned in the General Comments. In addition to the uncertainties of σ_{sh} , σ_u and σ_f , for EC, there are σ_{ue} (Under Estimate) due to dust (e.g., Fe₂O₃) and σ_{oe} (Over Estimate) due to carbonate; whereas for OC, there are σ_{lwsoc} , i.e., loss of WSOC ($\sim > 80\%$ of total OC, Hagler et al., 2007) during filtration processing, and σ_{oe} due to dust and carbonate. The overall uncertainties should be derived via error propagation.

Fig. S2: Please show the relative location of Fig. S2 to Fig. 1.

Fig. S4-S5: Since the log scale is used, it looks no much variation observed. In fact, large variation may exists. It is suggested to list all the data in a table,

Please also note the supplement to this comment:

<https://acp.copernicus.org/preprints/acp-2020-491/acp-2020-491-RC2-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-491>,
C7

2020.