Review of ACPD paper by Zdanowics et al.
Spatiotemporal variability of elemental and organic carbon in Svalbard snow during 2007-2018

This paper presents the mass concentrations of EC and OC in snowpack collected in Svalbard using TOT technique. Authors discuss the variability of these values, especially versus altitude. There have not been such extensive data in this area, as are presented in this paper. The features shown are potentially valuable. However, there are important problems that need to be addressed in considering the publication of this paper in ACP.

Major comments

In this study, TOT technique is used for the analysis of EC and OC in melted snow. In the present paper, it is simply stated very qualitatively that "C^{EC}_{snow} in snow samples that contained visible dust may be underestimated". Although potential errors are discussed in some detail in the Supplement, no quantitative estimate of the uncertainty of the data could be given after all.
Generally, the application of TOT technique to snow samples is not well established. Characteristics of the problems associated with TOT method for airborne particles may not be the same as those for the measurements of EC and OC in water samples. Authors cite the previous study of Lim et al. (2014), which compared EC concentrations with those of BC measured by an SP2 instrument. The methodology used by Lim et al. is quite straightforward and supported. However, it was made for a limited number of samples and it is difficult to generalize their results. It is a responsibility of the authors of this paper to make similar inter-comparisons using their snow samples.
It is very difficult to understand to what extent the observed features presented in this paper are natural phenomena or due to measurement errors. The methodology used in this paper lacks sound scientific basis, considering that the presentation of the absolute values of EC and OC is the central point.
Therefore, much more detailed analysis of the TOT technique must be made. The situation is worse for section 4.3, as described in specific comments. If there are no substantial improvements, I recommend rejection of this paper for publication in ACP, although there seems to be important findings and implications derived from the data.

Below, I have given specific comments.

pages 6-7.
Snowpack model is used for this study. It is desirable to give uncertainties of the model estimates.

Page 7
C^{eBC}_{air} is used for some data interpretation. Here, no critical evaluation of this data is given. Single parameter MAC is used to derive this quantity. However, no basis is given to show that this methodology is supported. In addition, different instruments are used to derive this quantity. Associated uncertainties should be given.

Page 8.
Table 2. It will be useful to include average altitudes for these sites to show altitude dependence of the quantities given in this table.
Table 2 and Figure 5 will be most important in this paper. It will be useful if these

locations are clearly shown in Figure 1, for example. There are a number of snow sampling locations given in Figure 1. It is difficult to relate them with Table 2 and Figure 5 for readers unfamiliar with the geography of Svalbard. You may consider using abbreviations, such as used in van Pelt et al. (2019).

Page 10, L298-302 and related discussion.
L^{EC}_{snow} is almost proportional to h_{SWE} up to h_{SWE} = 800 mm. Apparently, the relation becomes non-linear due to the two data points at h_{SWE} > 1200 mm. There is no physical basis to fit the data by an exponential function. In the linear region of h_{SWE} < 800 mm, the slope gives an average C^{EC}_{snow} for this data set. I consider that C^{EC}_{snow} for h_{SWE} < 800 mm is constant as a first approximation, especially considering that the uncertainty is not well defined. It is likely this slope agrees with the average C^{EC}_{snow} of individual sample values and this should be mentioned.

Section 4.1, page 9-10.
Spatial and temporal variabilities of EC and OC are discussed here. It is difficult to follow them in a short time due to many numbers presented here. This part should be re-structured, may be by using a figure or table. Using abbreviations may also help. Details should be moved to Supplement.

Section 4.1, page 12, L354-365 and page 15, L 460
Two data points at h_{SWE} > 1200 mm are interpreted as the result of inflow of polluted air from south associated with large cyclonic storms. Only L^{EC}_{snow} is discussed here, but it is the linear slopes of these points from the origin (C^{EC}_{snow}), that are most relevant here. It will be useful to check if C^{eBC}_{air} at Zeppelin showed large increases during these events. It should be enhanced by large factors if air transported from south was polluted. It is stated that storms and landfall caused large L^{EC}_{snow}. It may explain large h_{SWE}, but it will not explain large C^{EC}_{snow}. The effects of storms on h_{SWE} and C^{EC}_{snow} should not be mixed.

Section 4.2 and other parts
Variation of C^{EC}_{snow} with altitude is an important point, again depending on the reliability of the data. Considering its importance, more detailed discussion on possible causes of this feature should be given.

Section 4.2 and other parts
The effect of dry deposition is considered to be small from the analysis Figure 6. I agree with this analysis. However, the effect of dry deposition may be substantial for other snow samples. More considerations should be made on this point.

Section 4.2, page 13, L 398-427
The analysis made here assumes that C^{eBC}_{air} at the surface is directly linked with C^{BC}_{snow}. No detailed analysis is made to support this assumption and the discussion based on this assumption is very weak. It does not add solid materials to this paper. Therefore, it should be delated.
A minor comment. W is not non-dimensional. A proper unit should be given to it.

Section 4.3.
In this section, C^{EC}_{snow} data from different groups using different TOT methods are collected. There are two problems here. In this part, no detailed analysis and discussion are made on the uncertainties of EC data by different groups. It is not scientifically

sound to simply collect the data without any critical evaluations.
A second problem is that this section has little relevance with the major points of this paper and deviates from the mainstream of this paper. The circum-Arctic data are best analyzed and discussed in detail as a separate paper. Therefore, this section should be deleted.

Minor comments
Numerical values of parameters such as C^{EC}_{snow}, C^{OC}_{snow}, L^{EC}_{snow}, and L^{OC}_{snow} are given in 3-5 digits at many places, including abstract. Considering that the uncertainties cannot be given and are potentially large, these numbers should be given in 2 or 3 digits.

Page 2, L45
BrC is referred to here. But no discussion is made in the discussion of OC in this paper.

Page 14, Line 413
"f" is not clearly defined. Is it zero or one ?

Page 16, Line 484
UL may be UW.