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Comment on acp-2020-1260

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Referee comment on "Measurement report: Molecular composition, optical properties, and radiative effects of water-soluble organic carbon in snowpack samples from northern Xinjiang, China" by Yue Zhou et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1260-RC1, 2021

The authors present results form detailed measurements of the absorption determined in a number of melted snow samples collected in northwest China. The samples are grouped into different categories like rural, urban, and influenced by soil. The authors further analyzed the chemical composition of the snow samples with advanced analytical techniques to derive information on the water-soluble organic fraction at the bulk as well molecular level. This study generates a wealth of information on the composition of the snowpack in this region. Unfortunately, the number of analyzed samples and the characterization of the input sources for the different categories of organic compounds remain to limited to derive more general conclusions on the snow composition under different conditions. In a second part, the authors examined the impact of the absorption related to the organic compounds on the snow albedo and compared it to the impact of the also measured black carbon (BC). It appears that under specific circumstances the contribution of the organic compounds to the instantaneous radiative forcing can be non-negligible compared to BC. This is an important finding, especially for a region that has not received yet much attention in the literature. In summary, this manuscript describes an important exploratory study for a comprehensive chemical characterization of organics in the snow and how their impact on the absorption and the snow albedo can be analyzed. Below I have listed a number of comments that the authors may want to consider before the publication of the manuscript.

Major comments:

In ch. 2.1 it is mentioned that snow samples were taken in 5 cm intervals. However, no further information on these samples is later found in the manuscript. Are these samples analyzed separately or are they mixed before or after melting? If they are analyzed separately how do the absorption or chemical profiles look like? Moreover, the parameters snow depths, snow density, and snow temperature are also mentioned. Snow depths and density are listed for each snow pit in table S3, but only an average value for the density can be found. It would be useful to provide all data for all collected samples.

Ch. 3.2: The authors compare the measured absorption with absorptions obtained in other studies described in the literature. However, the snowpack properties and further conditions during the sampling in the different studies are not sufficiently described. For example, a thin or patchy snowpack is susceptible to the input of absorbing compounds by local sources. Apparently, this was the case for some of the here reported samples. What were the conditions during the sampling for the other cited studies? For example, the samples described by Voisin et al., 2012 were collected well before the melting period with a complete snow cover, while the samples in the study by Zhang et al., 2020 were collected in April and May. Were at this time the snow and sea ice cover still intact? Depending on the size of the glaciers the local impact should be much reduced for the samples examined by Yan et al., 2016. These effects should be considered for the comparison of the results of the different studies.

Ch. 3.2: Although HULIS2 is introduced as potentially stemming from marine sources, this seems not to be tested by the authors, who attribute this fraction to anthropogenic sources. Did the authors check any correlation with the sea salt components that were also measured in the samples?

L. 426: “These results provide a useful framework for representing snow BrC optical properties in climate models.” This statement should be explored further or deleted.

Fig. 4, 6, and 7: Based on a simplistic hypothesis, one could expect that the imprint determined in the soil-influenced samples would also be reproduced in the urban and rural samples with additional compounds stemming from further, potentially anthropogenic sources. While this could be deduced from the patterns shown in Figures 4, 6, and 7 for the urban samples, this is apparently not the case for the rural samples. Do the authors have any explanation why the soil-induced pattern is not present in the rural samples? In my opinion this is a topic that should be explored in the manuscript.

Minor comments:

Fig. 1a: The photographs are not convincing. For example, a difference between "Grassland" and "Dessert" is not obvious to me and it is unclear how these sites are distinguished. The other two photographs do not contribute further information for the sites.

L. 213: "An ultrapure water (18.2 MΩ cm) was used for.." Not complete.

L. 254: Please rephrase "…extracts were blown down to 200 μL…" (and also "… and blew them down by pure N2." In text S1). For non-chemist this could be difficult to understand.

L. 312: "**field**-measured"?

L. 331: "the broadband albedo (ɑ) of each scenario need**s** to be…"

L. 362: "It follows that in addition to the snow and glaciers from polar or alpine regions, the seasonal snow in Northern Xinjiang is also an important organic carbon source for the covered ecosystems." The meaning of this phrase is unclear.

L. 366: "the mass contributions of sulfate ions at U sites (Table S1, mean: 33%±7%), which is a commonly-used marker for fossil fuel burning…" This actually applies to non-sea-salt-sulfate. By looking at the sodium fractions, the sea salt sulfate appears to be limited, but it would be useful to calculate the non-sea-salt-sulfate in this context.

L. 378: "… therefore, pollutants had been potentially accumulated…" So far in the manuscript, the snow was characterized in terms of WSOC. Why are the authors are here referring to pollutants?

Fig. 9: I'm not convinced that this figure is needed since the strong impact of BrC on the absorption at wavelengths below 450 nm is already obvious in Fig. 8.

L. 848: "This study presents a comprehensive overview of WSOC and its BrC properties in seasonal snow of northwestern China…" I am not convinced that the limited number of snow samples can constitute a comprehensive overview. Such a characterization is also contradictory to the fact that the sample from site 120 has unique absorbing and chemical features that do not fit into the patterns found at the other sites. A more cautious statement is preferable.

Text S1: According to the presented data the recovered compounds account for less than 80% of the initial absorption (loss of 16% during the charging of the cartridge, another 6% loss during the two-step elution). It would be useful if the authors can explore what the potential impact of this missing fraction could be on the results and on the conclusions.

Table S1: Like for BC and WSOC the measured concentrations of the soluble ions should be given, not only the mass fraction of the total ion mass for each specie. A further table with all measured concentrations would be useful. Moreover, all numbers should be reduced to their significant digits (also throughout the manuscript), i.e. instead of "4.53±3.06" it should be "5±3". The mass fraction for the single site 120 should accordingly be reduced, i.e. instead of "3.98" use "4" for sodium.

Fig. S4: Any unit for the "sum of squared error"?