

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2020-1260

Anonymous Referee #2

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-RC2>, 2021

Measurement report: Molecular composition, optical properties, and radiative effects of water-soluble organic carbon in snowpack samples from Northern Xinjiang, China

This manuscript by Zhou et al describes the investigation for molecular composition, optical properties, and radiative effects of water-soluble organic carbon in snowpack samples from Northern Xinjiang. The results indicated that they are varied based on the geographic differences and proximity of emission sources. For example, the WSOC in S sites were characterized with high MAC365 values and high oxygenation and saturation levels, whereas more reduced S-containing species with high degree of unsaturation and aromaticity were identified in U samples. This study also demonstrating the important influences of WSOC on the snow energy budget. In all, the experiments were designed and conducted with cautions, and the literary presentation is excellent. I recommend publication in ES&T after the authors address the following comments.

The comments (L=Line):

- L 154-155: The classification of these sampling site should be also listed in a new Table or embedded in Table ***.
- Line 165-167: In this study, the sampling sites were mainly classified based on

- geographical location and proximity. Why the site 120 was not be assigned into one group? Only one sample 120 is very difficult to indicate its environmental application.
- Lines 362-364: Please rewritten this sentence.
 - Lines 351-364: in this paragraph, the concentrations of WSOC in U and S samples were comparably discussed based on many previous studies. Why the WSOC results in R sites were ignored?
 - Lines 374-376: how about the data in previous studies?
 - Lines 428-437: One concern is the data processing of EEM: In fact, the three fluorescent components are not relevant with humic-like and protein-like substances that derived from water (phytoplankton)/terrestrial soil, but they should be resided on the same peak positions. Author should give some explanation like this way using references, otherwise, some readers can mistake to have similarly or dissimilarly.
 - Lines 452-453: the statistical correlation between HULIS-1 and nitrates don't necessarily mean it is scientific reasonable. Please add some references to support that.
 - Figure S5: how can you obtain this Figure.
 - Lines 546-548: The relative content of CHON+ can not simply indicated they from aerosol depositions rather than from autochthonous sources.
 - Lines 576-577: This is only the data in ESI+ mode. As shown in Table 2, R samples showed higher DBE_w and AI_w values. Why the unsaturation and aromaticity of WSOC detected at ESI+ and ESI- are different for these samples? Please give more discussion.
 - Lines 600-602: add references to support that.
 - Lines 623-627: I can not understand this sentence.
 - Fig 7: In many previous studies, plots of DBE versus C atomic number were shown, why plots of DBE versus C+N atomic number rather than C atomic number were shown in this study?
 - Lines 724-728: The total rather than unique "BrC" molecules should be used to investigated for their correlation with their bulk MAC values.