

Atmos. Chem. Phys. Discuss., referee comment RC1
<https://doi.org/10.5194/acp-2020-1270-RC1>, 2021
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Comment on acp-2020-1270

Anonymous Referee #2

Referee comment on "Large seasonal and interannual variations of biogenic sulfur compounds in the Arctic atmosphere (Svalbard; 78.9°N, 11.9°E)" by Sehyun Jang et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1270-RC1>, 2021

The paper introduces a very nice comprehensive dataset of sulfur aerosol and would be useful data for many continuing studies. It is a bit misleading to say 2015-2019 as only includes March-August however measurements of seasonal and interannual variability are very useful and the paper justifies the suggestion that considering an average number for these important parameters or for the R_{Bio} ratio is not valid.

Comments on abstract

The abstract summary seems to concentrate on the MSA findings/impacts e.g. growth of new particles to CCN yet it is stated later that a more significant impact could be on the formation of Bio-SO_4^{2-} which potentially impacts more on new particle formation and therefore has a direct 10 -fold impact on cooling. Is this less significant in this study because the absolute amounts of Bio-SO_4^{2-} aerosol are small compared to the MSA aerosol? It is unclear from the R_{Bio} ratio how significant this relative contribution is and whether it should be stated a bit more in the abstract?

Specific comments

Line 23: How can 50% of the NSS-SO_4^{2-} be Anth-SO_4^{2-} ? Do you mean it was produced from it? Rephrase.

Line 151: Reference needed for sources of black carbon from fossil fuel, burning?

Section 3.4: Could a table be included to summarise the R_{Bio} values in different conditions/air masses, maybe include the temperature/light intensity if dependence is interesting (with reference to lines 320-333)?

Line 288: Missing 'hyphens' in brackets.

Figure 5 b: Set max y-axis scale to 1500?

Line 380: R_{Bio} , rather than just R.

Comments on conclusions

Since the concentration of OH plays such a big role in the DMS oxidation, is there anything that can be said about the climatic potential of the effect of increasing or decreasing OH concentrations over time on these findings (e.g. increasing global methane could lead to decreasing OH). Is there any potential trend over the years or just interannual variability?

I agree that more work should include integration with DMS, is this really beyond the scope of this paper?