



Interactive comment on “Dimethyl sulfide and its role in aerosol formation and growth in the Arctic summer – a modelling study” by Roya Ghahreman et al.

Anonymous Referee #2

Received and published: 7 August 2019

Ghahremaninezhad et al. present a modeling study of DMS emissions and chemistry in the Arctic using the Canadian chemical transport model GEM-MACH. The improvements in the simulations when using either satellite derived or in situ measurements of DMS(aq) will be of interest to some Atmospheric Chemistry and Physics readers. There is also an interesting and valuable discussion of the contributions of the different oxidation pathways to DMS-derived SO₂ and to size-resolved sulfate aerosol concentrations. The manuscript is well written, although I have provided some minor suggestions for improving the clarity of the text below. I support publication after the following comments have been addressed.

C1

General comments:

Defining geographic locations and regions: The manuscript is full of references to specific regions of the Canadian Arctic that would be unfamiliar to most researchers who don't work there. In addition, there are several sensitivity studies run by the authors where the emissions are modified in some of the regions but not others, and the exact boundaries of the regions where the modifications have been made are vague. Given that the actual spatial distribution of the DMS emissions is being altered, it is important that the authors provide a map indicating the major geographic regions discussed/analyzed (e.g. Hudson strait, Foxe Basin, Lancaster Sound). This addition would also make the manuscript easier to read for non-Canadian audiences.

Vertical profiles: The results shown in Figure 5 are confusing. Firstly, how is it possible to plot measurements that are below the method detection limit? By definition such data are not quantitative. Secondly, it is not clear what are the data labeled “CLIM+ave-obs”, since this model run has not been defined yet in the text. (I acknowledge it is defined later.) More generally, I found this figure very hard to understand since many points are plotted at the same altitude and it is not clear which measurements and simulations correspond to the same time and location. To make the figure clearer, I would suggest binning and averaging the individual data as a function of altitude similar to what is done with the model results. Lastly, it might be helpful to plot the horizontal axis logarithmically to better show the results at lower DMS concentrations, but I will leave that to the authors' discretion.

Source Sensitivity Tests: The authors initially present a DMS dataset for the month of July and the beginning of August (Figure 7), and the DMS predictions match the measurements fairly well in August. However, only the data for July are used when the sensitivity tests are done to evaluate different emission scenarios for DMS. The authors conclude that their CLIM11+ave-Obs scenario best simulates the measurements and closes the negative bias in the base scenario (Figure 8). However, I wonder if the CLIM11+ave-Obs scenario might have a positive bias in August, since the concentra-

C2

tions of DMS(aq) have been increased substantially. The conclusions of the sensitivity tests would be more convincing if the entire (both July and August) measurement dataset was used, not just the part that initially showed the negative bias. I realize that extending the GEM-MACH runs might not be possible because of limitations on computational resources, but if it is possible, then I think including the August data in the comparison in Figure 8 would be very interesting and make the conclusions stronger.

Minor comments:

Abstract: The months considered in the study are a little unclear here and also seem to change throughout the text. July and August are mentioned in the first paragraph, but then in the last sentence of the second paragraph only July is named.

Line 43: Can ship emissions be a potential source of anthropogenic sulfate that is emitted in the Arctic rather than transported from southern latitudes?

Figure 1: Given that DMS(aq) doesn't exist over land surfaces, the panel showing the difference in concentrations should be white over land, similar to the panels for the SAT and CLIM11 datasets. Also, if data for August was used, then it would be potentially useful to show that data in this figure as well.

Line 200: I think there is a mistake here and it should be written that the model was run for July and August 2014, at least for some runs.

Figure 2: It would be helpful to add a lat/lon grid to the maps.

Figure 3: The height corresponding to the data that is given in the Figure caption is different from that given in the first sentence of Section 4.1. Please clarify.

Figure 6: The author's should specify the linear regression method used since that will influence the slope obtained. This comment also applies to Figure 10 as well. Also, in the figure caption, "Polar6" should have a space before the number.

Figure 7: It appears there is an error in the figure caption. It states that the CIMS

C3

data runs to the end of July, but from the figure, there is data through the first week of August.

Figure 9: I think that the name "Lana", which I assume stands for "Lana et al.", should be replaced with the actual names of the model run as defined in Table 1 (i.e. CLIM11).

Figure 10: The legend should use run names that are consistent with Table 1.

Figure 12: What is specifically plotted in Panel B? Is the value the percentage of SO₂ from DMS or the percentage increase in SO₂ over the background value after DMS is implemented in the model?

Line 360: Do the authors have any explanation why the GEOS-Chem results are different from GEM-MACH?

Line 443 – 444: I suggest using units of ng/m³ for the aerosol concentration to facilitate comparison with measurements, and also since those units are used later in the manuscript.

Line 460: It would seem pertinent to reference Croft et al. ACP 2019 here as well.

Lines 476 – 478: The wording is incorrect here. It is stated that "biogenic sulfate particles are the dominant non-sea-salt particles", however the findings of Ghahremaninezhad et al. 2016 are for non-sea-salt sulfate only and not the total particle mass. This is an important difference that should be clearly stated here as there is much new evidence from the same field study that there is an important organic contribution to arctic aerosol in the summertime (e.g. Croft et al. ACP 2019; Tremblay et al. ACP 2019; Burkart et al. GRL 2017)

Line 502: The measured concentration of 10 – 35 ng/m³ corresponds to what size range? The authors should explain why there is a range. Does it represent the measurement uncertainty or something else?

Table 1: The last sensitivity study, in which a 75% yield of SO₂ from the OH-addition

C4

pathway is considered, has been omitted from the table. I also suggest that the authors add the basic statistical metrics for each sensitivity run (e.g. mean bias, normalized mean bias, R, RMSE). Currently, these values are scattered throughout the text and not easily found.

Typographical and grammatical errors:

Line 32: Delete “the” in the first sentence.

Line 229: Atmospheric DMS(g) SAMPLES were collected. . .

Line 319: effects of the potential uncertainty IN sources. . .

Line 382: Here and elsewhere in the text, quotation marks are used occasionally with the name of the model run. I would delete them in the text, since it is not clear what purpose they serve.

Figure 11: Scientific notation should not be used in panel (d) to be consistent with the other panels.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-470>, 2019.