

Interactive comment on “The potential role of methanesulfonic acid (MSA) in aerosol formation and growth and the associated radiative forcings” by Anna L. Hodshire et al.

Anonymous Referee #2

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The manuscript by Hodshire et al uses a global aerosol model to quantify the role of MSA in the aerosol-climate system. The role of MSA has not been quantified previously, with the majority of current aerosol and climate models making the simple assumption that MSA does not contribute to aerosol loading.

The study presents model sensitivity simulations exploring a range of mechanisms that might allow MSA to contribute to aerosol. In doing so, the authors highlight the sparsity of actual observations and lab measurements of MSA, which could be used to constrain the model simulations.

When allowing MSA to condense and nucleate in the model, only modest global ra-

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diative effects (up to -40 mWm^{-2}) are demonstrated. Regionally (e.g. in the Southern Ocean), larger differences in radiative effects are shown.

The study is subject to large uncertainties arising from the lack of measurements, as well as caveats introduced by the modelling approach. These uncertainties notwithstanding, the manuscript is a valid contribution to the field and should lead to further insights about the role of MSA.

Major comments

Please explain the rationale supporting the use multiple anthropogenic NH_3 emissions inventories across different regions. It's not clear why this was done and what impact this may have on the NH_3 concentrations and subsequently the MSA sensitivity tests.

The comparison method described in Section 2.5 assumes that the total sulphate will be additive between scenarios (i.e. subtracting DEFAULT_NoMSA from the sensitivity simulations leaves the MSA-related contribution). This is an imperfect approach as the additional mass from MSA will grow the aerosol size distribution, and therefore increase rates of dry deposition and nucleation scavenging. This limitation should be noted in the method description. I think the comparison remains useful, however.

For consistency with the ATom measurements, was the model data used in the comparison also restricted to the sub-micron size range?

Page 11 line 18 refers to 'Table 4', which has not been provided. The data does appear in Figure 8.

Page 12 line 30 refers to Figure S7, but I think should refer to Figure S6. Please further check labelling of N10 / N80, and for left / right labelling in caption of Figure S6

Minor comments

There are typographic errors through the manuscript that a thorough read-through should reveal, including some sentences that have redundant words, or fragments from

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previous iterations. I haven't attempted to highlight them.

Page 2 line 29: 'from marine particles' should be 'of marine particles'? Or 'from marine emissions'?

Page 3 line 1: VOCs – acronym not defined

Page 3 line 5/6: 'are an important source of marine emissions' should be 'are an important contributor to marine aerosol' (or something similar)? 'Aerosol' and 'particles' is being used in the same sense as 'emissions', which is incorrect.

Page 3 line 13: please state the relative yields of each product of DMS oxidation

Page 5 line 24: not clear what 'Regional EDGAR overwrites' are

Page 6 line 25: for completeness, please clarify whether MSA is assumed to be involved in the binary nucleation process

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-1022>, 2018.

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