Biogeosciences Discussions

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Comment on bg-2020-463

Nadja Steiner (Referee)

Referee comment on "Evaluation of ocean dimethylsulfide concentration and emission in CMIP6 models" by Josué Bock et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-463-RC3, 2021

Review Bock et al.:Evaluation of ocean dimethylsulfide concentration and emission in CMIP6 models

The paper by Bock et al evaluates the ocean dimethylsulfide concentrations and emissions in CMIP6 models.

Only two of the DMS models are actually prognostic DMS models, while the other two use different diagnostic algorithms. Hence the comparison is difficult to evaluate. Quite a bit of work has been done comparing the various algorithms estimating DMS based on Chl, light MLD etc. The way the simulated DMS is used (or not used) in the models also varies significantly - One model calculates DMS prognostically but does not use it, one calculates DMS prognostically and uses it in the atmosphere chemistry module for conversion and presumably aerosol formation, one model calculates DMS diagnostically and uses it directly in the aerosol module and the last one calculates DMS diagnostically and uses it in the atmosphere chemistry and aerosol formation module.

Essentially, the authors ran into the not unfamiliar problem to try compare multiple models which are not only vastly different with respect to the parameterizations of the variable in question, but also with respect to several other components such as gas exchange velocity, atmospheric feedbacks etc. In addition the climatologies used for evaluation have their own issues and potential errors. This makes it extremely difficult to understand respective differences among the output.

However, I feel that despite these difficulties the authors did an excellent job in comparing

the models, and identifying and describing the cause of differences. The evaluations are well linked and brought into context with earlier estimates and evaluations which helps to assess advancement from those and the uncertainties and concerns are well stated. This does provide scientific value despite the variety of parameterisations in the models.

The manuscript is well written and the evaluation procedures are sound. In fact the manuscript provides an excellent template for future analysis in (hopefully) more coordinated DMS model intercomparisons (Maybe consider adding a note with such a recommendation in the paper).

Hence, I recommend publication of the manuscript with minor changes.

What I am missing is a brief note on the potential impact of DMS emissions in the atmosphere, i.e a note indicating that areas with highest emissions are not necessarily those where the emissions have the highest impact, particularly with respect to the Arctic (see notes below).

Since the authors do include a focus section on the Arctic, I would also recommend to include a sentence or two on the missing ice algae component (see note below).

There are a few spelling mistakes which I noted (if I caught them)

Detailed comments:

l23 insert space after DMS) , rm "as" after considered

l26 "sulfate aerosols formed DMS" - from DMS?

l27 could mention the Arctic here, too (Abbatt citation?)

l60 measurements

l107 "the" marine DMS cycle

l135 adjusted to compensate... for what?

l205 unclear what "these " refers to in "to compare the skills of these methods" which makes the following sentence confusing. Please clarify what is compared to what etc.

l206 The yearly mean of this climatology - what does "this climatology" refer to? ANN?

L220 also issues with CDOM in coastal areas (see Hayashida et al. 2020)

L344 higher than

L363 The is paragraph seams a bit too generalized. E.g. it might be relevant to note that some of the regions indicates as poorly represented show hardly any variation and generally a much smaller range than the regions which have a clearer signal in both model and obs. I notice that the lower seasonality is discussed at the end of the section, but would help to briefly mention at time of the figure discussion.

L417 mirror - mirrors

L506 a weakly

L517 To help understanding => To help understand

L526 The coastal biome

L527 I am a bit concerned with the statement: "improving the models in the low latitudes regions is needed to gain confidence in the predicted global trends of DMS" => While this may be true, the question is if the global emission is the relevant one or if the emission is more important to improve in regions where DMS emissions have significant impact (as in the clean polar atmosphere) eventhough it might be a smaller contribution to the global mean (e.g, Abbatt et al. 2019,https://doi.org/10.5194/acp-19-2527-2019, and references therein)- maybe something to pick up in the discussion???

L615 suggest including reference to Hayashida et al 2020 (10.1029/2019GB006456) DMS

model for the Arctic (also provides detailed comparison with G19), including a note on the ice algae contribution which is not represented in the described ESMs (see note below)

Also suggest a note here on the impact of DMS in an otherwise clean atmosphere (Arctic spring summer, see note above)

L634/635 "This means that the models consistently predict lower DMS concentration below the sea-ice, in line with reduced photosynthetically available radiation." suggest adding a note on ice algae DMS production here