Review 3: Nadja Steiner

Dear Nadja Steiner, Thank you very much for your positive review of our paper, and for the interesting points that you raised. Your suggestions are very useful and help to improve the manuscript. Please find below our responses to your comments, which appear below in italics. Our proposed amendments to the text of our paper appear in blue. Page and line numbers refer to the version of the paper you reviewed.

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).

Please see our response to your comment about L527 below that covers also this comment.

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).

See our answer to your specific comment at L634/635.

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

- *l23 insert space after DMS) , rm "as" after considered*
  corrected
- *l26 "sulfate aerosols formed DMS" - from DMS?*
  corrected
- *l27 could mention the Arctic here, too (Abbatt citation?)*

  Thank you for this suggestion, we completed this sentence as follows: "Among natural aerosols, sulfate aerosols formed from DMS represent a major part of the aerosol-climate interactions in large pristine regions such as the Southern Ocean (Mulcahy et al., 2018, and references therein) or the Arctic (Abbatt et al., 2019, and references therein).
- *l60 measurements*
  corrected
- *l107 "the" marine DMS cycle*
  corrected
- *l135 adjusted to compensate... for what?*

  We rephrased this sentence:
  "During calibration of UKESM1-0-LL, the minimum DMS concentration (a) was changed to 1 nM and the threshold ($s = 1.56$) adjusted to compensate (Sellar et al., 2019)"
  which now reads:
  "During calibration of UKESM1-0-LL, the minimum DMS concentration (a) was changed to 1 nM and the threshold (s) was adjusted to 1.56 (Sellar et al., 2019).
- *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.*

  The text now reads:"Along with this ANN method, the authors also performed conventional linear and multi-linear regression analyses, to compare the skills of the three methods."
- *l206 The yearly mean of this climatology - what does "this climatology" refer to? ANN?*

  The text now reads:"The yearly mean of the climatology derived from the ANN method is shown in Fig. 1."

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

  Thank you for pointing out this specific issue. We mentioned it in an additional sentence:: "In coastal regions, the remotely sensed Chl signal may be biased by the presence of riverine coloured dissolved organic matter (CDOM), and ultimately lead to an overestimation of the computed DMS concentration (Galí et al., 2019; Hayashida et al., 2020)."

- *L344 higher than*
  corrected

- *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.*

  We agree that the low seasonality and small concentrations contribute to the poor match between observations and models. However, regions 37 (Southern Equatorial Pacific) or 38 (Northern Equatorial Pacific) illustrate that the agreement between models and climatology is sometimes much better despite the weak seasonality and low DMS concentration.
  We rephrased the last sentence of this paragraph:
  "Conversely, the seasonal cycle in equatorial regions of both Atlantic (regions 7, 8, 14, 17) and Pacific Oceans (regions 39, 40, 41) are poorly reproduced by all the models."
  to:
  "Equatorial regions are characterised by a weak seasonality and low DMS concentrations, and models poorly reproduce the climatology in these provinces (regions 9, 17 and 41 for instance)."

- *L417 mirror - mirrors*
  corrected

- *L506 a weakly*
  corrected

- *L517 To help understanding => To help understand*
  corrected (and L410 as well)

- *L526 The coastal biome*
  corrected

- *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???

What we meant in this sentence was that globally mean values we analyse, i.e., annual global DMS sea surface concentrations and DMS fluxes, are determined to a large extent by trade/low-latitude values. It is defacto necessary that models perform reasonably well in these low-latitude regions if one wants to be reasonably confident in conclusions concerning global mean quantities. This being said, we agree fully with your statement, regions with the highest emissions do not necessarily have the highest climate impact. We looked further in the literature to better apprehend this point but we could not identify fully relevant statements. The literature is quite abundant with regards to DMS in the high latitude regions, i.e. Abbatt et al. (2019); Galí et al. (2018) for instance with all references thererin, but it is much less so with regards to DMS in the tropical regions. In these tropical regions an emerging topic is that of the role of DMS in local climate of coral reefs is recognised as amongst the strongest individual sources of natural atmospheric sulful (Jackson et al., 2020). Furthermore, measurements of DMS air-sea fluxes have been reported in the western Indian Ocean (Zavarsky et al., 2018) and have illustrated the regional coupling with aerosol products.

We included additional details in our introduction and the text now reads: "A recent estimate deduced from three CMIP6 models (GISS-E2-1-G-CC, NorESM2-LM, UKESM1-0-LL) suggests a slight amplification of global warming due to a positive feedback of 0.005±0.006 W m^{-2} K^{-1} (Thornhill et al., 2021). These global estimates hide large regional differences both in terms of radiative forcing and in terms of changes in DMS emissions under global warming (Thornhill et al., 2021). So far, studies have focused more closely on high latitudes regions eventhough a few recent ones demonstrate the dominant role of DMS on marine low cloud albedo over most oceans (e.g., Quinn et al., 2017) or illustrate regional impacts on low latitudes (Zavarsky et al., 2018). In polar regions, studies ..."

- *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)*

  We have added Hayashida et al. (2020) reference in several parts of the text, including in this section 4.3 that focuses on the Arctic, as you and Martí Galí suggested.

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

  We have added Abbatt et al. (2019) reference on this issue in the text of our introduction (see 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*

  We added the following sentence in the text : "This means that the models consistently predict lower DMS concentration below the sea-ice, in line with reduced photosynthetically active radiation. We want to note here however that a number of recent studies highlighted the large DMS production of ice algae and acknowledged that models likely underestimate the contribution of bottom ice DMS (Hayashida et al., 2020, and references therein).

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