Review 1: Martí Galí

Dear Martí Galí, We want to thank you for your very thorough review of our article. Your suggestions for adding clarifications, precisions and references throughout the manuscript will help improve it. 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.

GENERAL COMMENTS

The article by Bock an coauthors gives a comprehensive overview of sea-surface DMS concentrations and sea-air fluxes in four CMIP6 models, which feature DMS parametrizations of different complexity (two diagnostic and the other two prognostic). The models also differ in their coupling (or lack thereof) between marine DMS emission and atmospheric chemistry and aerosol/cloud radiative forcing, although this aspect is not dealt with in the discussion. The main conclusions of the article are (1) the uncertainty in present-day model estimates, with spatial patterns and seasonal cycles that differ from observational products in many oceanic regions, and (2) the diverging trends in global DMS emissions in end-of-century projections. These divergences reflect the different factors that drive DMS concentrations in each model, rather than the complexity of the parametrizations. The findings reported by Bock and coworkers will be useful to advance the modelling of marine DMS cycling because they highlight the mixed success in representing DMS cycling appropriately in CMIP numerical models and, more generally, the gap between experimental/observational knowledge and model parametrizations.

Below I summarize my main criticisms, with the hope that they will help improve the manuscript:

1. *On the potential overestimation of global DMS concentration by the Lana et al. (2011) climatology, and the failure to capture extremes.*

   The comparison between ESM results, global DMS climatologies based on statistical relationships and EO datasets (G18, W20), and the L11 climatology based on objective interpolation of in situ data, indicates greater consistency between the former two. The authors conclude that L11 overestimates DMS globally, a conclusion that is supported by previous works (Tesdal et al. 2016; Galí et al, 2018). Although the conclusions of the latter studies still hold over many regions, here I would like to note that all model and statistical representations of sea-surface DMS fields fail to account for extreme DMS concentrations. As the authors note, extreme concentrations were removed to compute the L11 climatology, although they were not systematically identified as measurement artefacts. Webb et al. (2019, SciRep) showed the importance of extreme DMS events in Antarctica. Recently, Bell et al. (2020, Frontiers) showed that both the L11 *climatology and the observations-based G18 and W20 diagnostics failed to capture high-DMS events in the North Atlantic. In the light if these findings, the message that L11 overestimates DMS globally should be nuanced. Obviously, there is a dichotomy between the ability to capture the mean state and the extremes, and the latter might play an important role in ocean-atmosphere interactions.*

Thank you for pointing out that one of the messages of our paper should be clarified. The references that you suggest are indeed useful to illustrate that neither the models nor the observation based products are currently able to reproduce the observed high DMS concentrations. As you say, one difficulty is to distinguish between a climatological mean state and extremes that are localised in space and time.

For instance, Bell et al. (2021) analyze that both the L11 climatology and diagnostics based on monthly satellite observations made over a given month fail to capture high DMS in situ measurements made over that same month in the North Atlantic. However, in this study, measurements from a given month of the year have been made only once, and one could question to which extent such high DMS concentration appear rather repeatedly throughout the years and could become a climatological feature. More observations are clearly needed. Bell et al. (2021) indicate that "The aim of this paper is to present an overview of the seawater DMS observations during NAAMES and some of the environmental factors that influence DMS variability." and later "The DMS climatology (Lana et al., 2011) captures the seasonal progression well but, unsurprisingly, does not accurately represent the substantial variability in DMS over short spatial/temporal scales in the North Atlantic." So clearly, from this paper one cannot conclude whether the high DMS concentration values measured during the NAAMES campaign are a climatological feature (that L11 fails to represent), or if the small spatial and temporal scale of these features are insufficient to imprint the climatological concentration.

The Webb et al. (2019) paper is somehow more affirmative in its conclusions for the West Antarctic Peninsula (WAP) coastal zone where DMS measurements have been made over a 5 year period. In this "highly productive region" (as designated by the authors), very high DMS concentration ("exceeding 30 nM in four out of five summer seasons") were measured. In particular Webb et al. (2019) conclude that "the L11 climatology is not accurately predicting WAP summer DMS production, and in particular is missing peak-DMS production events." While these measurements carried out over several years are likely representative of climatological values in this area, the extrapolation to the entire Longhurst APLR province to estimate a resulting flux, evaluated twice as large as that in L11, is more questionable. Overall, one important conclusion of this study is that elevated DMS concentration are associated to the sea-ice break-up. Since models do not include this driving factor, they unsurprisingly fail to reproduce the early summer maximum DMS concentration.

We propose to nuance and clarify our wording about the L11 climatology in three places as follows:

L185-189: "Thus, as pointed out by Tesdal et al. (2016), small scale features are transformed into large scale ones by the interpolation procedure. Tesdal et al. (2016) also suggested that the extrapolation of a small number of data points could lead to unrealistic distributions. This was confirmed by Galí et al. (2018, Sect. 4.1) who analysed some sources or errors and biases in L11, and pointed out that the distribution of DMS concentration is right-skewed as compared to DMS concentration derived from satellite chlorophyll measurements. These authors suggested that a preferential sampling of DMS-productive conditions could explain this positive bias."

replaced by:

"Thus, as pointed out by Tesdal et al. (2016), small scale features are transformed into large scale ones by the interpolation procedure, and anomalous values observed at local scale could induce bias when extrapolated across data-sparse regions. This is illustrated by Hayashida et al. (2020), who show that the entire Arctic region in L11 is based on extremely limited data (0–4 % areal coverage north of 60° N). The resulting extrapolation of open water DMS concentration to sea-ice covered areas, where primary production is presumably lower, may lead to a positive bias in L11. Another potential positive bias in L11 stems from the overrepresentation of biologically productive conditions in the in-situ DMS database from which L11 is built upon. This is supported by the study of Galí et al. (2018, Fig. 7 and Sect. 4.1) who pointed out that the distribution of DMS concentration in L11 is right-skewed as compared to DMS concentration derived from satellite chlorophyll measurements. Conversely, recent studies report on high DMS concentrations measured in the North Atlantic (Bell et al., 2021) and in a coastal station of the West Antarctic Peninsula or in the Ross Sea (Webb et al., 2019; del Valle et al., 2009, respectively) which are not represented in L11."

l. 308: "Conversely, the very high concentration displayed in L11 around Antarctica, and to a lesser extent in the Indian Ocean and south of Alaska, are not predicted by any model or by G18 or W20, suggesting that this could be a bias in the L11 climatology."

replaced by:

""Conversely, the very high concentration displayed in L11 around Antarctica, and to a lesser extent in the south of Alaska and in the Indian Ocean, are not predicted by any model nor by G18 or W20. For the former two regions, high concentrations have been reported in long-time measurements, at a site of the West Antarctic Peninsula, 2012-2017 period (Webb et al., 2019), and at the Ocean Station P in the North East Pacific, 1996-2010 period (Steiner et al., 2012) and 2005-2017 period (Galí et al., 2018). Further investigations would be required to explain these discrepancies between measurements and models or climatologies. Some specific processes, such as the DMS concentration enhancement following sea-ice break-up (Webb et al., 2019) are not accounted for in the models, but are not sufficient to explain all discrepancies. Overall, assessing the relevance of high DMS events at the global scale and the spatial resolution of climate models is mandatory to improve them."

l. 694 "As concluded by previous authors (see for instance Galí et al., 2018, Sect. 4.1), the widely used L11 climatology likely overestimates surface DMS concentration due to sampling biases."

replaced by

"As concluded by previous authors (see for instance Galí et al., 2018, Sect. 4.1), the widely used L11 climatology likely overestimates climatological surface DMS concentration at the spatial resolution of climate models due to the combination of scarce and biased sampling."

2. *Global NPP-DMS relationship Understanding the global NPP-DMS relationship would be useful to place emergent constraints on present-day and future DMS emission. The attempt made in this article to pinpoint this relationship is very welcome, but the discussion of the underlying factors is poor. First, I would not expect that studies covering small spatiotemporal scales (e.g. those cited in L601) could give relevant insights into the NPP-DMS relationship over multiyear periods at the biome scale. Perhaps the work that addressed this issue more explicitly was that of Kameyama et al.(2013, GRL. Strong relationship between dimethyl sulfide and net community production in the western subarctic Pacific); but they related DMS to NCP, not NPP. I also recommend the work of Osman et al. (2019, Nature. Industrial-era decline in subarctic Atlantic productivity). Second, the discussion of the NPP-DMS relationship disregards the complexity of food-web and abiotic processes that control DMS concentrations. The framework proposed by Galí and Simó (2015, GBC) could be useful to understand the contribution of different factors to DMS variability in prognostic models.*

   NB: our response below also answers the question raised by Reviewer #2 in his/her specific comment L601.

   Thank you for raising this issue of emergent constraints and for pointing out that our references may not be fully suitable to support our analysis.

   We fully agree that placing emergent contraints on a DMS-related field would be a very useful tool to the overall climate community. However this is beyond our ambition for this paper.

   The ambition of our paper, quite specific here and that could appear modest in compararison to existing much more specialised litterature, is to broadly assess how some CMIP6 climate models behave in terms of DMS ocean surface concentrations and DMS fluxes, both in the current climate and in the rest of the century.

We are indeed lacking a large-scale observational database that would enable us to draw robust conclusions on the relationship between NPP and DMS concentrations, or emissions, at the scale of global oceans.

We have reworded this part of our text, noting, as you said, that a number of observational studies have highlighted such relationship, at a local scale though, and complementing these local-scale studies with the recent studies of Uhlig et al. (2019) and of Osman et al. (2019) that have been conducted at a basin scale.

However, we think that there are other lines of evidence, other than observations, on the existence of such relationship.

Firstly, previous modelling work of Bopp et al. (2003) and of Kloster et al. (2007) show that the response of the marine biology (i.e., declining NPP) is one of the prominent drivers of changes in DMS emissions. Although the current generation of the PISCES and HAMOCC models derive from previous model versions, key processes have been revised and updated. These changes have implications on model performances and on future projections as reported and documented in Séférian et al. (2020) and Kwiatkowski et al. (2020). In consequence, our work shines light of an emergent property of marine biogeochemical models linking changes in NPP and changes in DMS that is robust across model generations.

Secondly, factorial experiments conducted by Wang et al. (2020) using an artificial neural network show that a 10 % decrease of Chl *a*, a proxy for NPP, leads to a reduction in DMS concentration in large open-ocean domains.

We aknowledge though that a number of studies observed no correlation between DMS and Chl *a*, reflecting the complex mechanisms that control DMS concentrations and fluxes (e.g., Wang et al., 2020, and references therein).

We thus replaced the text : "Local in situ observations (e.g., Simó et al., 2002; Becagli et al., 2016) have shown positive correlations between NPP and DMSP, and the link between DMSP and DMS concentration has been described in several studies (e.g., Stefels, 2000; Yoch, 2002; Asher et al., 2017; Lizotte et al., 2017). The first group of models (CNRM-ESM2-1, NorESM2-LM and UKESM1-0-LL) thus captures a relationship which is consistent with such ocean field experiments, while the response simulated in MIROC-ES2L is not consistent with the current understanding of the DMSP production pathways by marine phytoplankton (Stefels et al., 2007)."

with

"Although the limited current knowledge about the NPP-DMSP-DMS relationships hampers our ability to constrain this emergent property, several lines of evidence tend to suggest that there is a positive correlation between NPP and DMS concentration. Firstly, noting that some studies observed no correlation between DMS and Chl *a* (e.g., Wang et al., 2020, and references therein), a number of other studies showed positive correlations between NPP and DMS production: the link between NPP and DMSP is highlithed at the local scale (e.g., Simó et al., 2002) and at a basin wide scale (e.g., Uhlig et al., 2019), that between NPP and DMS concentration again at a basin wide scale in Osman et al. (2019), and the link between DMSP and DMS concentration has been described in several studies (e.g., Stefels, 2000; Yoch, 2002; Asher et al., 2017; Lizotte et al., 2017). Secondly, factorial experiments conducted by Wang et al. (2020) using an artificial neural network show that a 10 % decrease of Chl a leads to a reduction in DMS concentration in large open-ocean domains. Finally, previous modelling work of Bopp et al. (2003) and of Kloster et al. (2007) show that the response of the marine biology (i.e., declining NPP) is one of the prominent drivers of changes in DMS emissions. The first group of models..."

The framework you describe in Galí and Simó (2015) and that one could apply to these CMIP6 models is largely beyond the scope of our article. Not to mention all the distinct variables involved in your analysis that are not part of the official CMIP6 data request, and thus are not available for a comparable analysis. However, we cite Galí and Simó (2015) in our conclusions as a way forward to progress in DMS climate modelling. The text at L742 now reads:

"Overall, our work shows that there is a major uncertainty in low-latitude ocean where the change in DMS concentration results from the interplay of marine biology factors with many other environmental drivers (e.g., temperature, salinity, stratification, nutrient availability, acidification, large-scale circulation), which and all may affect in both directions the trends in DMS concentration (Wang et al., 2020). Further analysis to disentangle the role of these factors is required, for instance along the lines of the meta-analysis of Galí and Simó (2015) that specifically addresses the issue of the "summer paradox". This would require important coordination among modellers to work in a multi-model perspective as only a few CMIP6 models include DMS and their DMS-related outputs are limited and insufficient at present to conduct such analysis. In turn, ... "

3. *Relevant literature*

   I suggest that authors to dig deeper into the non-modelling literature, which is connected to the point above. The article currently gives the impression that the authors are not familiar enough with some aspects of the DMS and DMSP biogeochemistry because imprecise informations are scattered in the text (see specifics). Suggestions of relevant articles can be found through this review. In addition, I strongly suggest the authors to pay more attention to the body of modelling literature produced by the group of Elliott and colleagues:
   Wang et al. (2015, JGR). Influence of explicit Phaeocystis parameteriza-

tions on the global distribution of marine dimethyl sulfide.
Wang et al. (2018, Biogeochemistry). Influence of dimethyl sulfide on the carbon cycle and biological production.
Xu et al. (2016, JGR). DMS role in ENSO cycle in the tropics.
Another important article that explains the different ability of models to decouple DMS from phytoplankton biomass is:
Le Clainche et al. (2010, GBC). A first appraisal of prognostic ocean DMS models and prospects for their use in climate models

We added precisions and references throughout the text, following your multiple suggestions made below (see our responses below to your specific comments). Thank you for these suggestions. Based on the references you cite in this general comment we have added the following sentence in the text of the introduction: l. 47 "...allowing an assessment of the recent evolution of DMS in this region (Galí et al., 2019). These advances coincide also with those of global models, from ocean biogeochemistry ones (Le Clainche et al., 2010; Séférian et al., 2020) to full ESM ones enabling investigations on either (i) the physical factors that impact DMS behaviour, for instance Xu et al. (2016) demonstrate that there seems to be a two-way interaction between DMS and ENSO in the tropical region, or (ii) the ecological factors, for instance representing in the model more explicitely diverse phytoplankton groups (e.g., *Phaeocystis*: Wang et al., 2015)."

4. *Extrapolation of DMS emission to an ice-free Arctic summer*

   A correction is needed here. In the G19 paper we warned that extrapolations towards a 100% ice-free Arctic summer shouldn't be made using the pan-Arctic linear regression between DMS emission and ice-free extent. This would amount to assuming that the % contribution from each subregion will not change in the future, which we know is not true. The Atlantic sector can't contribute much more than it presently does because it already is mostly ice free; on the contrary, the Central Arctic is far from being ice-free in summer and has the lowest DMS concentrations (Uhlig et al. 2019, Frontiers), thus the lowest potential for an increase in emissions. Of course, this is illustrated by the fact that the sums of regional extrapolations do not equal the pan-Arctic extrapolation in Table 9. I argue that, to compute the range of future ice-free Arctic DMS emissions, only the sum of regional contributions in each model should be used. I also recommend the study of Hayashida et al. (2020, GBC. Spatiotemporal Variability in Modeled Bottom Ice and Sea Surface Dimethylsulfide Concentrations and Fluxes in the Arctic During 1979–2015) who used a regional model with higher resolution to estimate contemporary DMS emission from the Arctic.

   —— We do not completely agree with this comment, for several reasons. First, in the multimodel approach of this study, the main message is about the spread of projected emissions at 100 % ice-free ocean, which is much larger than the difference, for a single model, between the pan-Arctic projected change and the sum of projected changes in the three areas. We do agree that the projected change in the entire pan-Arctic region is a simplification, since the relative emission in the sub-basins might change. However, this shortcoming also applies to a sub-basin, which could itself be further split in smaller parts, whose relative contribution may change. Nevertheless, in historical and ssp585 runs, most estimations are rather close. There are only 2 cases (CNRM and UKESM, historical) where there is an important difference between both projected changes: for CNRM, the sum of extrapolated changes is 17 % larger than the extrapolated change on the pan-Arctic, but for UKESM, it is smaller by ~ 30 %, which does not allow to conclude about a general rule. Lastly, and this is our main point: in your 2019 paper, the regional breakdown is justified by the contrasted biogeochemical regimes. In the models, at least those two with prognostic DMS parameterisation, the differential biogeochemical regime is not accounted for, and the sea-ice retreat dynamics in each regions is likely the main driver. Nevertheless, we noted that the multimodel projected range of DMS emission is larger when using the sum of projected changes, and is thus better to provide a likely estimate. We took your comment into account and now report future emissions as the sum of the regional contributions. The text now reads: l. 669 "In total, the CMIP6 summertime DMS emissions extrapolated at 100% sea-ice free water vary between 72 and 310 Gg S, enlarging the corresponding estimation of 144±66 Gg S in G19."

l. 677 "Extrapolations of annual DMS emissions at 100 % ice-free extent for the 2036-2100 period (from 86 to 282 Gg S for the pan-Arctic region) are comparable to projections inferred from the 1950–2014 period (72 to 310 Gg S).

We also cited the study of Hayashida et al. (2020) as suggested:

l. 682 "This modelled behaviour agrees well with the conclusions of Hayashida et al. (2020), who found that the decline of Arctic sea ice is associated with a quasi-linear positive trend of DMS flux.

SPECIFIC COMMENTS

- *L23: perhaps a "by-product of microbial food webs" is more accurate, given the important roles of heterotrophic processes (grazing, bacterial catabolism) in DMS production. In other words, DMS production can't be understood from phytoplankton processes alone (which is different from saying that it cannot be predicted from phytoplankton variables...).*

  We changed "a by-product of marine primary production" into " a by-product of microbial food webs ".

- *L25: please don't forget MSA, a product of the addition pathway that can be produced in relevant proportions compared to sulfuric acid.*

We agree that other products, such as MSA or DMSO, are produced in addition to SO_2. But as the intention in this introduction was to synthesize informaton on the DMS cycle in the atmosphere, and since the contribution of MSA to new particle formation is expected to be negligible in certain environment compared to that of H_2SO_4 (Bardouki et al., 2003) we did not mention MSA here. We reformulated the sentence into: "Once in the atmosphere, DMS is mainly oxidised into SO_2 and then gas-phase sulfuric acid, which rapidly condenses onto pre-existing aerosol particles..."

- *L30: the original CLAW hypothesis paper (Charlson et al., 1987, Nature) should be given credit here. Regarding the relationship between DMS and downwelling irradiance, please consider citing Vallina and Simó (2007, Science).*

  We had not mentioned on purpose the original paper Charlson et al. (1987) for the "CLAW hypothesis" as it has been largely discussed and revisited since 1987. We chose to cite papers that include references to both the original paper and to follow-on papers. As suggested, we have added the reference Vallina and Simo (2007) in the text that now reads "DMS-climate feedback Vallina and Simo (2007); Carslaw et al. (2010); Quinn and Bates (2011))."

- *L87: This is inaccurate. I suggest: "Dissolved DMSP is then converted to DMS with yields that increase with bacterial nutrient stress". Note: PISCES assumes all DMS production arises from dissolved DMSP, unlike other models, and contrary to observations. To compensate for this, PISCES requires "bacterial" DMS yields that range between 40 and 60% (Belviso et al., 2012), which are clearly too high (Galí and Simó, 2015).*

  Thank you for the suggestion and additional explanation, we have modified the text accordingly.

- *Table 1: In my opinion, this table should cite the original articles where the prognostic or diagnostic DMS models were described. In the case of the diagnostic ones it is very simple: - MIROC uses Aranami and Tsunogai (2004) - UKESM uses Anderson et al. (2001) This may be more difficult in the case of prognostic models that have seen incremental development. In this case, I suggest citing in this table the most recent papers where each prognostic model was described.*

  Thank you for your suggestion on references, but we chose to keep on with the latest references that refer to the exact versions of the models we used in our analysis (CMIP6 version). Indeed these references include both past references, possibly original references, as well as the descriptions of the evolutions implemented, or not, in the CMIP6 version (see for instance the Sellar et al. (2019) paper that provides details concerning their tuning of the Anderson et al. 2001 parametrization).

- *Section 2.1.1: I did a little research on the prognostic sulfur modules.- PISCES: As the authors describe in this section, the original sulfur module of Bopp et al. (2008) was updated by Belviso et al. (2012) based on the model PlankTOM5 of Vogt et al. (2010). This detail should be included.*

  Thank you for the time you spent to look for additional information of the modules. We have added what you propose. The text now reads (l. 83): "... and updated by Belviso et al. (2012) based on the PlankTOM5 model of Vogt et al. (2010).".

- *L187: This is true but the explanation is inaccurate. What is right-skewed is the distribution of satellite-retrieved Chl concentration matched to the DMS database, compared to the global distribution (PDF) of satellite Chl. As the authors correctly point out in the following sentence, this is related to preferential sampling of productive waters that probably had higher-than-average DMS. Ultimately, this could partially explain the right skewness of the in situ measurements when compared to the global DMS fields estimated with the G18 algorithm. But the latter may also suffer from biases caused by the fitted equations and satellite observations.*

  We have changed this, see the answer to the general comment #1.

- *L217: besides sea ice, what limits satellite ocean colour measurements (passive radiometry) at high latitudes in winter is the low solar elevation. In December, no reliable observations are available north of about 48 degrees, in November and January the boundary is slightly above 50 degrees, etc.*

  We rephrased the sentence that now reads: "Another limitation of this approach is the lack of satellite observations over sea-ice and at low solar elevations, resulting in observational gaps in high latitudes ($> 48°$) in winter. "

- *L306-308: I disagree here. For unknown reasons, models struggle to capture high DMS in regions like the NE Pacific (station PAPA) and around Antarctica. In the NE Pacific, occurrence of very high DMS (often ¿15 nM) in late summer has been extensively documented by the Line P program and by many studies from Philippe Tortell's group. The article of Steiner et al. (2012, Biogeochemistry) explored potential reasons. This was also discussed by Galí et al. 2018. In the Southern Ocean, Phaeocystis antarctica blooms can results in seawater DMS of several tens nM, e.g. del Valle et al. (2009, L&O), Webb et al. (2019, SciRep). Highest DMS concentrations, many of which measured around Antarctica, were removed by Lana et al. (2011) before computing their climatology. So there's a general failure at capturing extreme DMS concentrations under certain conditions. See general comments.*

  We changed this according to your suggestions, see the answer to the general comment #1.

- *L324: Note that the high DMS in winter and spring at high northern latitudes in L11 could be an interpolation artifact, as explained by Hayashida et al. (2020, GBC). This is relevant to BPLR panel in Fig. 5.*

  We have taken note of your remark, and the text now reads: "UKESM1-0-LL shows a northern maximum starting earlier than the other models, in agreement with L11. However, as pointed out by Hayashida et al. (2020), this feature could be an extrapolation artefact in L11 (see Sect. 2.2.1)."

- *L349: "six biomes", but only four are listed. Perhaps specify that N and S polar and westerlies biomes are treated separately (but not trades or coastal ones).*

  We reformulated into "six biomes (polar N and S, westerlies N and S, trades and coastal)"

- *L362: Well, low correlations are expected in areas where DMS has low seasonal amplitude. This also applies to the paragraphs below. OK, this was answered in L383.*

  This is indeed analysed later on in the paper. We thought about reorganising this section so that the analysis follows more closely the description, but we keep the original version in the end.

- *L372: Looking at Fig. 5 province by province, and at Table 5, my first impression is that NORESM usually does better at capturing seasonal cycles, except for the trades biome. Also, my gut feeling is that this model better captures summertime DMS maxima at subtropical to temperate latitudes (the DMS summer paradox; check Simó & Pedrós-Alió 1999, Nature; LeClainche et al. 2010, GBC; Galí and Simó 2015, GBC), at least in regions I know best (4-NADR, 6-NASW, 16-MEDI).*

  It is indeed an impression that figures confirm or not. NorESM is indeed the best in provinces 4-NADR and 6-NASW, but the other models are rather good as well. In the 16-MEDI region MIROC has the best score, while still in these subtropical to temperate latitudes, in the 34-NPPF region MIROC again has the best score or in the 18-NASE region scores are the same for NorESM and CNRM-ESM. More generally, over the 54 provinces, NorESM2-LM has the best correlation over 18 regions, MIROC-ES2L in 15 regions, CNRM-ESM2-1 in 13 regions and UKESM1-0-LL in 10 regions. However, such piece of information is not really valuable, since provinces have very different weights (either considering their size or the corresponding emission). So, for the sake of balance between details and generalities we decided not to extend further our analysis of the results presented in Figure 5.

- *Figure 7: please specify somewhere (figure or caption) that CAMS uses the N00 parameterization.*

  We changed in the figure "CAMS" into "CAMS19". The text of the paper explicitely says that CAMS19 uses the N00 parameterisation (Sect. 2.2.2).

- *L480-481: I tend to agree with Tesdal here. See general comments.*

  We have modified our text that now reads: "Accounting only on the four models, the median flux (1980–2009) is 19 ± 3 Tg S year $^{-1}$, thus lowering the best estimate of Tesdal et al. (2016) by 10% but with an identical uncertainty range. Overall, we somewhat contradict Tesdal et al. (2016) who conclude in a low bias of model DMS fluxes. Indeed, apart from the CAMS19 and the L11/Nightingale et al. (2000) DMS flux estimates, the other current observational estimates coincide with our CMIP6 model estimate." However, the current grid size of ocean models and additional processes that are not accounted for, such as DMS enhancement during sea-ice break-up (see Sect. 3.1.1), prevent models from accounting for high DMS events localised in space and time. Thus, simulated DMS emission might represent the lower bound of actual fluxes.

- *L565: "regions or processes"L585: "modelling works". Otherwise a large body of non-modelling literature would be disregarded.*

  Thank you for the precision, we modified accordingly.

- *L601-606: The discussion of the relationship between NPP and DMS is poor, and misses some relevant literature. DMS production is a food web process, not just a phytoplankton process. See thegeneral comments.*

  We have enriched the discussion as described in response to your general comment above.

- *Figure 14: Axis labels are too small, and some horizontal reference lines (or a grid) would be very useful to guide comparisons among models.*

  We have added reference lines to help reading of the figure, and increased the labels font size.

- *L641-648: It is important to note that the non-Atlantic sector includes the Siberian shelves, which seem to be quite productive owing to nutrient inputs from large rivers, recycling on the shelves and coastal erosion (Terhaar et al., 2021, NatComm). However, the satellite data used in G19 may be biased high in the Siberian shelves due to optical interference of continental materials (which was also pointed out by Hayashida et al., 2020). So I would dare to say that uncertainty in satellite DMS is much higher in the non-Atlantic sector, and that ESMs possibly struggle to capture the biogeochemical functioning in shallow Arctic seas, due to both too-low resolution and non-represented processes.*

  Thank you for the explanations. We have modified the text following your wording. It now reads: "Galí et al. (2019) further discussed differences in biogeochemical and meteorological characteristics in the two Arctic sub-sectors to explain why DMS concentration is larger in the non-Atlantic sector. In particular, as the non-Atlantic sector includes the Siberian shelves, which seem to be quite productive owing to nutrient inputs from large rivers (Terhaar et al., 2021), the G19 data may be biased high in the Siberian shelves due to optical interference of continental materials (Hayashida et al., 2020). While uncertainty in satellite DMS appears higher in the non-Atlantic sector, ESMs possibly struggle to capture the biogeochemical functioning in shallow Arctic seas, due to both too-low resolution and non-represented processes. Notwithstanding the biases in models as compared to G19, only MIROC-ES2L and NorESM2-LM correctly capture this difference between sectors with higher DMS concentration in the non-Atlantic sector."

- *L665: Since emissions arise mostly from ice-free areas in both satellite and ESM asessments, additional factors must be invoked to explain scatter in the ice extent vs. DMS emission relationships. I am pretty sure that lower R2 in models results from too-low interannual variability in models compared to satellite observations. In G19, we pointed out that after 2011 interannual variability was controlled mostly by ocean productivity, not ice extent. This was further analyzed, and confirmed, by Lewis et al. (2020, Science).*

  Thank you for pointing that out.We modified our text that now reads: "Over the pan-Arctic region, determination coefficients (R^2) are largely higher in all CMIP6 models than those of G19. This reflects the linear dependence of the flux to the free-water fraction in the models, though in observations additional factors such as ocean productivity can be invoked to explain scatter in the DMS emission versus sea-ice extent relationship (Galí et al., 2018; Lewis et al., 2020). Smaller interannual variability in models compared to satellite observations can also contribute to higher R^2."

- *Figure 15 and the related analysis: see general comments.*

  See our response to the general comments.

- *L687: Please beware that Le Clainche et al. (2010) did a DMS model intercomparison.*

  We rephrased our sentence so the context of our study is more clearly presented, and we added the Le Clainche et al. 2010 reference. The text now reads: "...and while they may have already been evaluated, either in a previous (e.g., Le Clainche et al., 2010) or in their current version, it is the first time that this is done in a common coupled atmosphere-ocean simulation framework."

- *L694: agree on sampling biases, disagree on global overestimation. See general comments.*

  See our response and amended sentence in general comment #1.

- *L701: ...which has been known for a long time (see Le Clainche et al., 2010 and references thererin)*

We added this information and the text now reads: "models better reproduce the annual cycles in mid to high latitudes (polar and westerlies biomes) than in low latitudes (trades biomes), in agreement with past studies (e.g., Le Clainche et al., 2010)."

0.1 Typos, technical corrections

- L23: space after (DMS)
  corrected
- L26: something is missing: "formed DMS"
  corrected: formed from
- L116: open parentheses before "Simó and Dachs"
  corrected
- L212-215: please consider breaking this sentence with a period somewhere.
  This sentence is left unchanged.
- L277: "maxima", not "maximums"
  Wee stick to "maximums", both are correct see: https://www.collinsdictionary.com/dictionary/english/m
- L376: hypotheses
  corrected
- L576: "at play"?
  corrected

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