We sincerely thank the reviewer, Dr. Yuanxu Dong, for his thorough review of our manuscript and for the many constructive suggestions provided. Below, we respond to the comments point by point. The reviewer's comments are shown in red, our responses in blue, and excerpts from the revised manuscript are presented in *italic black*.

Air-sea gas exchange is a fundamental process in the Earth system. Dimethyl sulfide (DMS) acts as a potential climate-active gas, and its air-sea flux plays an important role in climate regulation. Understanding how surface ocean DMS concentrations and fluxes respond to future climate change is therefore essential. This study applies a novel neural network approach to investigate historical and future DMS distributions under a specific climate change scenario. Although similar studies exist, this work improves data selection for both training and projection phases and produces some distinct results. The drivers of DMS variability are further examined through sensitivity tests. The paper is well written, the topic is timely, and the method is appropriate. It could be a valuable contribution to the community after addressing the following comments.

We thank the reviewer for their positive comments and precise summary.

Line 16 – I am not a climate modeling expert, but why was SSP5-8.5 specifically chosen? Please clarify the rationale.

Thank you for your question. SSP5-8.5 represents a high-emission, fossil-fueled development pathway that induces the most pronounced changes in the climate system among the SSP scenarios. Given that oceanic DMS production and emissions are tightly coupled to marine ecosystem dynamics—which are highly sensitive to climate forcing—this scenario allows us to investigate the upper bounds of possible DMS responses. Since the primary objective of our study is to assess how DMS concentrations and fluxes might evolve under future climate change, we selected SSP5-8.5 to capture the strongest and clearest signal in the system.

Line 116 – Consider explaining the full name of CESM2-WACCM and adding a reference.

CESM-WACCM is the abbreviation of the Community Earth System Model Version 2 coupled with the Whole Atmosphere Community Climate Model. We have spelled it out in the revised manuscript (See lines 114-117), and added corresponding reference.

"For the input data to our ANN model, we use the surface and monthly environmental outputs from CESM2-WACCM (the Community Earth System Model Version 2 coupled with the Whole Atmosphere Community Climate Model) (Danabasoglu et al., 2020)."

Line 117 – Possibly a naive suggestion, but did you consider using the best-performing variables from different models? For example, IPSL-CM6A-LR performs well for SiO_4—can it be used selectively?

Thank you for this thoughtful suggestion. While certain variables—such as SiO_4 from IPSL-CM6A-LR—may individually perform better than those from CESM2-WACCM, we chose to use a consistent set of input variables from a single Earth System Model (CESM2-WACCM) to maintain internal coherence and avoid potential biases introduced by mixing outputs from different models. Notably, CESM2-WACCM demonstrates the best overall agreement with observations across the key variables relevant to our study, making it a robust and internally consistent choice for driving our simulations.

Line 118 – There is no Fig. S6; this likely refers to Fig. S4.

Thank you for pointing this out. We have adjusted the order of figures and referred to the corrected figure in the text (lines 117-119).

"This model ensemble is selected because it demonstrates the best overall results among the CMIP6 ensembles when compared to observational data (see Fig S2)."

Table S2 – Do the last two columns represent the number of components? Please clarify.

Thank you for your suggestion. The last two columns in Table S2 represent the number of available ensemble members used to calculate the means. We have added explanations for the last two columns in Table S2.

Line 132 – A reference is needed to support this sentence.

Thank you for pointing this out. We have added a supporting reference (See lines 131-133).

"The specific version employed, PISCESv2-gas, includes a dedicated module for simulating the cycles of climate-relevant gases (Aumont et al., 2015)."

Line 141 – If interpreted correctly, Aranami and Tsunogai (2004) did not introduce a new parameterization but used the Nightingale et al. (2000) formulation.

Yes, indeed. We cited the wrong reference and have corrected it in the revised manuscript (See lines 140-141) as shown below.

"DMS flux is calculated using Nightingale et al. (2000) parameterization."

Line 142 – Please specify which gas exchange parameterization is used in this model.

Thank you for your suggestion. We have added the explanations of gas exchange parameterization in the NorESM2-LM model (See lines 145-146).

"Sea-to-air flux DMS fluxes are calculated according to Wanninkhof (2014)."

Line 149 – The Liss parameterization differs substantially from Wanninkhof (2014) and Nightingale et al. (2000). This likely causes significant differences in DMS flux estimation.

We partially agree with this comment. Different air–sea gas exchange parameterizations can indeed lead to notable differences in flux estimates when applied to the same DMS concentrations. To ensure consistency in comparison, we adopted the same gas transfer scheme as used in Joge et al. (2025)." (see lines 156–163).

"As discussed above, the four ESMs employ different gas exchange parameterization schemes, which can lead to differences in estimated DMS fluxes. For instance, the scheme by Liss and Merlivat (1986) tends to underestimate fluxes at high wind speeds due to its limited representation of bubble-mediated transfer processes. In contract, the parameterization of Wanninkhof (2014) and Nightingale et al. (2000) are generally considered more reliable, as they are based on broader field datasets and better capture wind-speed dependencies. To ensure consistency in comparison, we adopted the same gas transfer scheme as used in Joge et al. (2025)."

Line 154 – A link to Fig. S2 would be appropriate here.

Thank you for this suggestion. We have deleted the original figure S2 since it is too general and is not related to the content of the study (See lines 165-166).

"The ANN model is a branch of artificial intelligence (AI), which builts with a fully connected network of nodes and neuron."

Line 174 – Wang et al. (2020) is not listed in the bibliography.

Thank you for your carefully review. We have added the reference of Wang et al. (2020) in the bibliography list.

Line 211 – The value 0.6673 should be approximated as 0.67, not 0.66, for the testing/validation datasets.

Thank you. Corrected. (See lines 224-226).

"The ANN model successfully captures the major variability in the observed data, achieving a coefficient of determination (R^2) of 0.68 for the training dataset and 0.67 for the testing dataset (Fig. S3)"

Lines 224–241 – It would be informative to show the absolute concentrations and fluxes in both historical and projected periods. This would help assess whether high or low DMS regions are increasing or decreasing. Consider including these our subplots near Fig. 1 or in the Supplement.

Good points. We have followed your suggestion and added the corresponding figures in Supplement. (See Fig. S4) and also as follows,

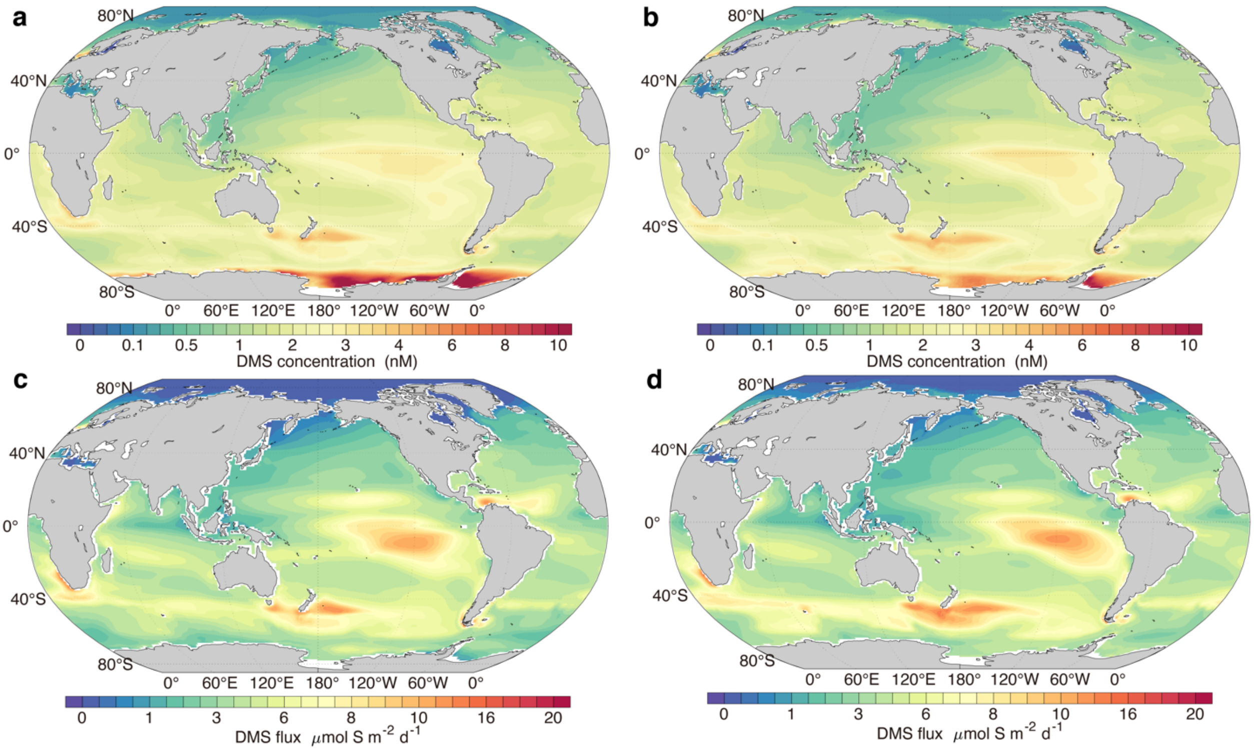

Fig. S4| Surface DMS concentrations and DMS flux over historical (1850-2014) and SSP5.85 (2015-2100) periods. a, DMS concentrations in historical periods. **b**, DMS concentrations in SSP5.85 periods. **c**, DMS flux in historical periods. **d**, DMS flux in SSP5.85 periods.

Fig. 2 – It is notable that both this study and Joge et al. (2025) use neural networks, yet they predict systematically different historical DMS concentrations. Both presumably rely on the same observed DMS data for training. Even with different training variables, similar observed values should be reproduced, right? Please comment on this discrepancy.

Thank you for this insightful comment. We acknowledge the discrepancy and have discussed its underlying causes in our original submission. We reiterate the key points here for clarity (see lines 246-256):

"The discrepancy arises primarily from two factors: 1) Data sources for training: Our model was trained using primarily observational variables, whereas Joge et al. (2025) used Earth system model outputs as inputs. Given the known biases in model-simulated biogeochemical fields, we consider observational data to better capture the true environmental relationships governing DMS variability. 2) Model ensemble differences: We used output from a single model (CESM2-WACCM), which demonstrates the best agreement with observations among the models considered (see Fig. S2). In contrast, Joge et al. (2025) used an ensemble of eight models, some of which show significant biases in key parameters. These differences in both training data and model ensembles likely explain the divergent historical DMS concentration patterns predicted by the two studies."

Table 2 and Fig. 2 – Although the focus is on concentration, flux is also presented and highlighted in the abstract. Therefore, some clarification is needed. What gas transfer velocity (K) formulation is used in Joge et al. (2025)? That study predicts lower DMS concentrations but higher fluxes. Is this due to a different K? The same issue may apply to the green and blue models in Fig. 2. Flux comparisons should use a consistent K to be comparable. Since wind speed modulates flux trends, the choice of K is likely significant. A discussion of this would be valuable near the flux results.

Following is the way that Joge et al. (2025) used to calculate the *k* value:

$$k = \left(\frac{1}{k_w} + \frac{H}{k_a}\right)^{-1}$$

and k_a is the airside transfer velocity in cm h^{-1} which is calculated following Yang et al. (2013),

$$k_a = 8814 \times U^* + 6810 \times (U^*)^2$$

and U* is calculated following Johnson et al. (2010).

$$U^* = Windspeed \times \sqrt{1.3 \times 10^{-3}}$$

The waterside transfer velocity (k_w) in cm h^{-1} is calculated following Marandino et al. (2009),

$$k_w = (0.46 \times Windspeed - 0.24) \times \frac{100}{24} \times \left(\frac{S_C}{720}\right)^{-0.5}$$

and is the Schmidt number which is calculated following Saltzman et al. (1993).

$$S_C = 2674 - 147.12 \times SST + 3.726 \times SST^2 - 0.038 \times SST^3$$

The Henry's constant (H) is the dimensionless water over air solubility of DMS and is a function of SST as follows

$$H = 0.0053 \times e^{\left(3500 \times \left(\frac{1}{SST+273.15} - \frac{1}{291.15}\right)\right)} \times 0.08206 \times (SST + 273.15) \times 101.325$$

As suggested by the reviewer, we adopt the same parameterization to calculate the flux for all models to ensure consistence.

Indeed, after adopting the same sea-to-air flux parameterization, the differences between our study and that of Joge et al. (2025) were substantially reduced. However, a noticeable discrepancy remains in the final ~20 years of the simulation: while both studies predict similar DMS concentrations during this period, Joge et al. (2025) reports higher fluxes. This difference is primarily attributable to our inclusion of sea ice coverage in the flux calculation, which was not considered in their study. Since sea ice suppresses gas exchange, especially in high-latitude regions, its omission can lead to an overestimation of DMS fluxes in those areas.

Lines 286–291 – This section requires supporting references.

Thank you for this suggestion. We have added a supporting reference in the manuscript (See lines 314-320).

"In the VMLD experiment, DMS exhibits a decreasing trend, likely driven by the shoaling of the MLD under global warming. This shoaling MLD suppresses the upward transport of nutrient-rich deep waters (Fig. 3a), thereby limiting vertical mixing between deeper, nutrient-rich layers and the oxygenated surface ocean. As a result, primary production is reduced, particularly in low- to mid-latitude regions, ultimately leading to lower DMS concentrations at the surface ocean (Sigman &

Hain, 2012)"

Section 3.3 – The first two paragraphs offer plausible explanations but would be more convincing if supported by literature. The last paragraph includes many citations, consider distributing some of these to earlier parts of the section.

Thank you for this suggestion. We have added a supporting reference in the revised manuscript.

Fig. 5 – This is a strong figure but does not reach its full potential, which is a shame. I think captions should emphasize the key message, and the main text should include dedicated discussion for this figure.

Thank you for this suggestion. We added the discussions in the updated manuscript (See lines 392-403).

"Consistent with these findings, our results show that DMS concentrations are regionally influenced by a combination of environmental drivers (Fig. 5). Within the DMS production pathway, phytoplankton-derived dissolved DMS is negatively regulated by nutrient availability (DIN, DIP, SiO_4) and mixed layer depth (MLD). Its subsequent conversion to the gaseous phase and emission into the atmosphere are positively correlated with SST and surface wind speed (SSW), and negatively correlated with sea ice coverage.
Moreover, DMS plays a key role in the climate feedback loop. Once emitted into the atmosphere, DMS is oxidized to form non-sea-salt sulfate aerosols (nss-SO_4^{2-}), which act as cloud condensation nuclei (CCN). This enhances cloud albedo, reduces incoming solar radiation (positive radiative forcing), and ultimately cools the ocean surface (negative feedback)."

Data availability – The journal likely requires a data sharing statement. Please check if the data repository link is missing.

Thank you for this suggestion. Data availability statement has been added to the text and as follows (See lines 441-445),

"Data Availability
All data produced in this study are archived in the following repository: https://figshare.com/articles/dataset/Data_for_Trends_and_spatial_variation_of_oceanic_dimethyl_sulfide_under_a_warming_climate_revealed_by_an_artificial_neural_network_model/29653277?file=56589284."