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.

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

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

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?

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

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

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

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

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

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

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

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

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

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.

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.

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.

Lines 286–291 – This section requires supporting references.

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.

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.

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

-Yuanxu Dong