Response to Reviewer 1 comments for manuscript ID egusphere-2024-173. The comments are given in an italic typeface, and the responses are given in a bold typeface. The corresponding changes in the revised manuscript are highlighted in red.

1.1) ***General comments:***
Sankirna et al. (2024) have presented an important evaluation of three of the most recent dimethylsulfide (DMS) climatologies, plus a few online model parameterisations, providing a timely guide to modellers who may be deciding on how DMS should be represented in their systems. This paper also provides an important benchmarking of the updated DMS climatology interpolated from observations (Hulswar et al. 2022), which may be expected to replace the most commonly used Lana et al. (2011) climatology. While this paper is not long, and does not provide analysis beyond statistical comparison, it addresses an important question clearly. I have only minor comments on this manuscript and would recommend its publication after they have been addressed.
Response: We thank the reviewer for the above comments and for identifying that this paper is a timely study that will be useful to the modelling community. The answers to the specific comments are given below.

Specific comments:

1.2) Line 13: 'Most models' – I think you should be more specific here – you are talking about atmospheric models that represent aerosol processes.
Response: This line has been updated to *'Most atmospheric models that represent aerosol processes ….' (Line 13).*

1.3) Line 26: '... the impact of DMS on the radiative budget are very sensitive to the estimate used' – I'd perhaps remove the word 'very', as even if it's a 100% increase, if its 1nm to 2nm I don't think that would have a 'very' large impact on the radiative balance… Until it has been shown what the impact on the radiative balance is, I'd temper this argument.
Response: Updated to*: '… the impact of DMS on the radiative budget is sensitive to the estimate used.'* (Line 26)

1.4) Line 35: Sentence beginning with 'Thus ...' - this sentence is a bit long and a little bit confusing. I think you need to make more clear the feedback that you are alluding to.
Response: The sentence is rephrased as: *'CCN contribute to the formation of clouds, increasing cloud albedo. Due to this, DMS emissions have the potential to decrease solar radiation at the ocean surface, resulting in negative feedback'*. (Line 35-36)

1.5) Line 37: I think you need to make mention of the comparatively large amount of literature indicating that the CLAW hypothesis likely is not plausible in the complexities of the real world (e.g., Quinn & Bates, 2011), but you can at the same time quantify its importance to the global energy balance (e.g., Fiddes et al. 2018).
Response: The following lines are now added as suggested: *'Past studies have shown that this feedback cycle is more complex than the original CLAW hypothesis (Quinn and Bates,*

2011) However, it is undeniable that DMS affects the radiative budget on a global scale. For example, Fiddes et al. (2018) showed that the removal or enhancement of marine DMS can change the atmospheric radiative effect at the top of the atmosphere by 1.7 and -1.4 W m^{-2}, respectively. (Mahajan et al., 2015b) showed that the difference between model simulations with and without DMS can result in an aerosol radiative forcing difference of -.179 W m^{-2}, with the difference exceeding 20 W m^{-2} in the Southern Ocean. Hence, there is a need to understand the DMS cycle within the context of uncertainties and biases of the climate models (Fossum et al., 2018; Fiddes et al., 2018).' (Line 37-44).

1.6) Line 74: Can you provide a reference here: 'A recent study…'
Response: Added *(Galí et al., 2015). (Line 82)*

1.7) Methods section: I was a little bit confused here, you are using three data sets that are publicly available, but your writing makes it sound like you have re-run some of this analysis? Perhaps you can revise your writing a little in this section to make clear that you are describing the data sets and not your own methods.
Response: Indeed, for the comparison between the climatologies, we are using published datasets. To calculate long-term trend, we had to re-calculate the datasets of W20 and G18. This has now been made clear *'As only monthly climatologies of DMS are available from G18 and W20 public data, the models from these two papers were re-run to get monthly estimates of DMS from year 1998 to 2010 in order to calculate the trends of seawater DMS.'* (Line 104-106)

1.8) Line 100: Can you clarify that the input parameters you are discussing are those that went into the G18/W20 parameterisations?
Response: Clarified *as 'The parameters used for W20 and G18 are …' (Line 106-107)*

1.9) Line 103: Were G18 & W20 data sets available over the exact same time periods (1998-2010)? Can you explain a little bit here why this time period? (I think you do later, but would be good to have it upfront).
Response: This has now been added in the methods section too, as suggested (Line 121).

1.10) Line 130: In light of your results here, could you comment on how effective using chlorophyll as a proxy is?
Response: Chlorophyll is one of the primary predictors for DMS at it indicates presence of different types of phytoplankton. However, it is true that it is not an effective predictor by itself due to the complex biogeochemistry of DMS. We have added a sentence regarding this: '*Thus, making chlorophyll a; a poor predictor by itself*' (Line 145-146).

1.11) Line 142: I wonder how many observations the H22 data set has in these regions? How would that impact the results?
Response: There is a total, 43,002 observations used in G18, 89,569 in W20 and 872,427 in H22 globally. Out of these observations, 1,610 points are available in the region > 60º S for G18, 12,666 for W20 and 620,454 for H22. The estimations are obviously impacted

by the number of observations and hence we conclude in this paper that we need more observations for increased accuracy. The below Figure S1 is now added to the supplementary text to show this clearly.

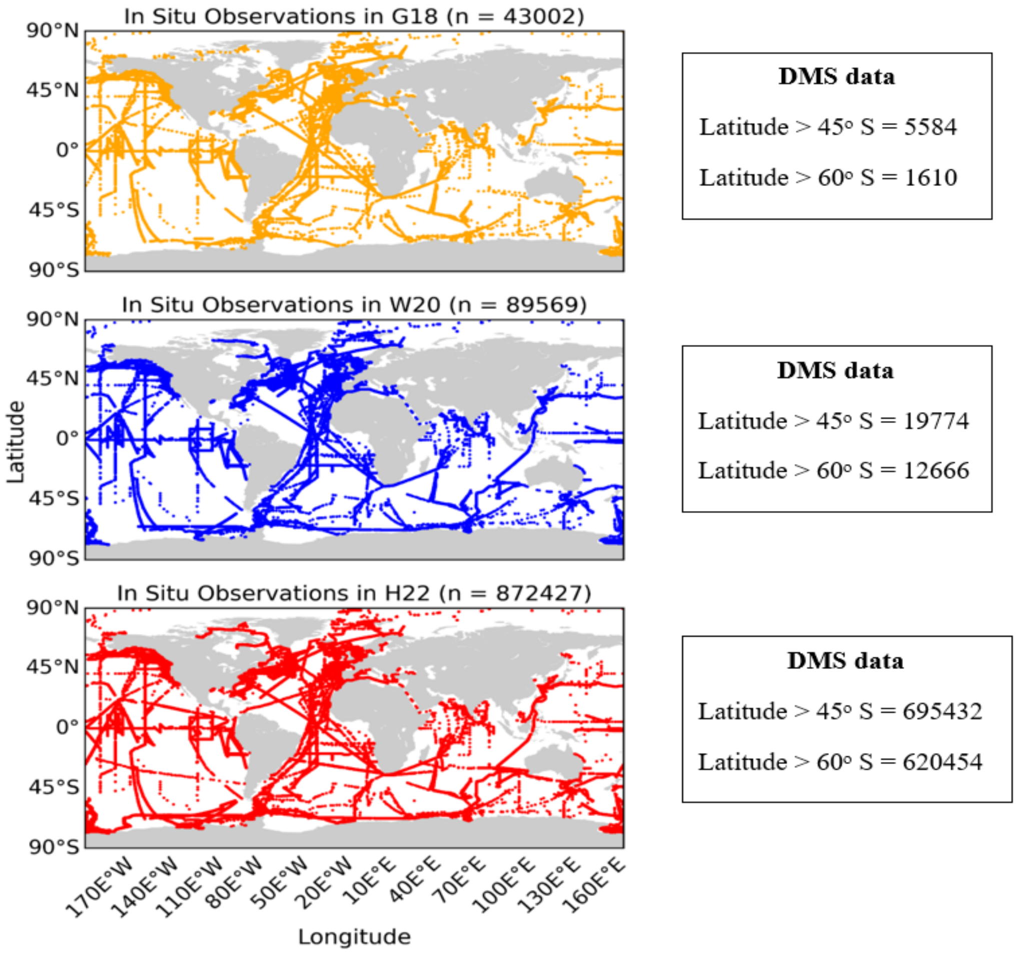

Figure S1: In situ DMS observations used in G18, W20 and H22.

1.12) Line 209-214: Can you speculate on why these differences exist across methods? Is it due to poor data availability to train on? Or the quality of the inputs?

Response: The main reason is the availability of observations across different regions and season and the second the data quality. We have added this in the manuscript. '*The difference in the methods is driven by various factors. The sensitivity of methods to certain parameters (or observation bias in the case of H22) is the primary driver. However, the main reason for this is the availability of high-resolution observations across different regions and seasons and also the quality of the observations. In the future, more observations will help resolve some of these differences*.' (Line 204-207)

1.13) Line 221: I think 'with' should be 'while

Response: Replaced.

1.14) Section 3.3 Long term trend: I think this section needs to have an acknowledgement that 12 years is in fact not a long-term trend, certainly not enough to understand the full variability *of a system with respect to important climatic and oceanographic events (e.g., ENSO). I think it's still a valuable contribution and the trends to appear quite large, but I think it just needs to be recognised that this is still really quiet a short period! (And starts with one of the strongest El Nino's recorded – I don't know how ENSO might affect DMS, but I would be surprised if it didn't!).*

Response: Yes, we agree and have included the following in the revised manuscript: '… *Even though an increasing trend is obtained in G18 and W20, this period is not sufficient to understand the long-term variability of the Earth system and the DMS response to it.'* (Line 263-264)

1.15) Line 230: 'We used monthly …' – I'm not sure why this is here, as you don't mention any results from these data sets?

Response: This line has been removed and section modified.

1.16) Line 247: What do you mean by 'predictors obtained from CMIP5 and CMIP6 reconstructed models? I'm not confident that 'this issue can be resolved' using climate model output – I think there are lot of issues in the CMIP6 models still around these processes, so I wouldn't really trust what they suggest (as you just said – CMIP5 and CMIP6 suggest opposite trends for DMS, so there is large uncertainty there still!).

Response: We have modified this section to remove the reference to the CIMP models in light of the comments from the reviewer. We have replaced this by: '*In theory, this could be addressed using the machine learning code and proxies from climate model projections, although this has large uncertainties too*'. (Line 265-266)

1.17) Line 255: this paragraph and those below is pretty dense – could you perhaps use dot points to describe each model so it flows a bit more clearly? Also – perhaps this would be better in a methods section? Also, can you describe the time period you are using here? And where did you get this data from.

Response: Bullet points are added before description of each model as suggested '… *These models are described as follows: ….'* This is a comparison of the area weighted means and hence does not read well in the methods section. The citation for each data is mentioned. (Section 3.4)

1.18) Line 285: I would really love to see this geographic breakdown and perhaps a similar analysis to what you did with the other three data sets (I know that this has been done to a degree already e.g., Bock et al. 2021, but it would be nice to have it all in the same place & in comparison to H22).

Response: The new figure below is now added in the supplementary text (Fig. S3). Text is added at line 307-308 in modified manuscript: *'... the geographic breakdown distribution of DMS (Fig. S3) can show large differences …'*

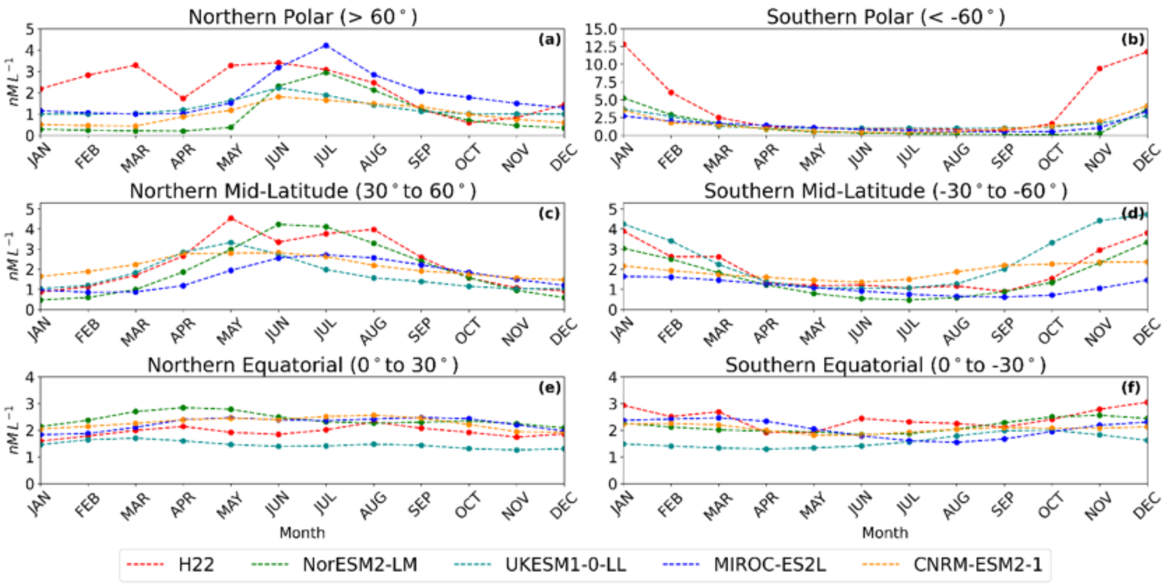

Figure S3: Latitudinal means for each month of CMIP6 models described in section 3.4 along with H22 climatology.