Interactive comment on "Diagnosing sea-surface dimethylsulfide (DMS) concentration from satellite data at global and regional scales" *by* Martí Galí et al.

Anonymous Referee #1

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This manuscript, entitled "Diagnosing sea-surface dimethylsulfide (DMS) concentration from satellite data at global and regional scales", used climatological MLD, satellite retrieved Chl and PAR, and an embedded sub-algorithm based on satellite data to construct an algorithm for surface DMS concentrations. This work provides a prescribed DMS distribution with seasonal and interannual variability. The paper is well written and well organized. Yet I recommend the following points to be clarified or modified.

Main comments:

1. One major contribution of this algorithm is providing DMS estimate with interannual and seasonal variability. Authors chose some regions to show the variability, but the

results are part of the validation and not representative as mentioned in the manuscript (being the region where the algorithm works the best). It would be better if authors can discuss more about the variability on a global scale.

2. Authors discussed regional tuning and biases as the strength of the algorithm. However, it raises the question about predictive power. In other words, the algorithm is largely built based on statistical regression, lacking fundamental scientific support. Authors should further clarify the optimized formula, differences caused by regional tuning, and regional tuning is required in some cases.

3. The algorithm discussed here largely depend on the sub-algorithm. Though it is described in a previous publication, basic introduction and discussion about the sub-algorithm are needed for readers to understand the strength and limitation. For example, chlorophyll data contains no information about speciation, which plays an important role in the total DMS concentration.

Specific comments: P2, L31: Please add reference

P3, L5: Since diatom-dominated blooms produce low DMSP per unit biomass, how to determine the equation parameters based on chlorophyll concentrations alone? It's clear that blooms dominated by different phytoplankton will require different parameter values, though the chlorophyll levels are similar.

P4. L30: please explain more the motivation of using PAR, instead of the total short-wave irradiance. Using PAR here imply the role of photosynthesis, however it shouldn't play any role in the DMSP to DMS transformation. Though mentioned a bit in the comparison with VS07, it's not clear whether and why the choice in the present work is better.

P 5. L 24: Conclusions about the comparison are vague. More discussion about various choices would be helpful.

P8: Please discuss uncertainties attributable to DMSP estimates. P8 sec 3.1.2 Authors

showed how DMS estimates vary with parameter values, but not physical meanings, which should be included.

P9 L26: estimated DMS concentrations are much lower than L11. Does it suggest an overestimation in L11? Please clarify.

P10 L26 Please explain the cause of the disagreement.

P12: Different regions require different sets of parameter values for optimization, which raises the question about uncertainties. Please elaborate on it.

P13: This section discusses biases of the algorithm without explanation of causes and suggestions on potential improvements.