

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

Anonymous Referee #2

Received and published: 19 March 2018

General Comments:

The manuscript “Diagnosing sea-surface dimethylsulfide (DMS) concentration from satellite data at global and regional scales” highlights the need to improve spatial and temporal scale of DMS through satellite data sets. The science community has relied on the available climatology data for couple of years. However, validity of current data sets (Kettle et al., 1999; Lana et al., 2011) are limited because of significant uncertainty and lack of interannual variability. I think the algorithm presented in this manuscript coupled with preceding studies on DMSSAT are very important to assess the challenges with global and regional DMS data sets. The algorithm proposed relies on the non-

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linear relationship between phytoplankton light exposure, DMSP and DMS. Again this research is a step in the right direction to produce appropriate observation-related data of reduced sulfur from the ocean that could be employed to constrain Earth System Models (ESMs). Suitable DMS dataset could ultimately help to reduce uncertainty associated with the impact of tropospheric aerosol forcing on global radiation. However, satellite generated dataset are not immune from uncertainties. Thus, there is a general concern of compounding uncertainty transferred to derived dataset such as $DMSP_{SAT}$ and DMS_{SAT} . Algorithm and dataset presented in this manuscript could therefore be improved in different biomes. The paper addresses relevant scientific question and the overall presentation is well structured and clear. I encourage the paper to be published after addressing the following concerns:

Main Comments: 1) Global DMS_{SAT} concentration seems a little bit low. In-situ measurements at BATS station in late summer to early spring of 2006 and 2007 show higher concentration than DMS_{SAT} from equation 2f. With this regional underestimation, it will be reasonable to assume that area weighted global mean DMS obtained from equation 2f (Table 3) could also be a little bit underestimated. Thus, An annual emission of $16\text{--}18 \text{ Tg S yr}^{-1}$ could significantly lower the formation of sulfate aerosol in ESMs below atmospheric measurements at the boundary layer. Could you combine optimize local scale concentrations (for BATS and any available region) with the global scale to improve the overall concentrations?

3) Authors should discuss if any extrapolation method was used to compute DMS concentration at high latitudes where SeaWiFS chlorophyll concentrations are limited. If none, then authors should be careful to note the spatial coverage of DMS in the winter. Otherwise, authors could also be quantitative on the overall polar concentrations reported in L11 climatology but missing in this study due to limitations in satellite chlorophyll measurements.

5) The authors should clarify why they computed global DMS_{SAT} fields with SeaWiFS data and regional DMSSAT with MODIS data. I was wondering if the authors made

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a global scale optimization (MLongh) with MODIS data? Monthly DMS climatologies derive from DMS_{SAT} (MODIS-Aqua) tend to be more agreeable with in-situ measurements. The authors should report how the global climatology computed from MODIS differs from SeaWiFS and/or Lana et al. 2011. Specific Comments:

Page 2, line 32 needs reference "...10% is emitted to the atmosphere through turbulent diffusion [ref]".

Page 3, line 6 needs reference "... and low DMS yield [ref]".

Page 4, line 15: I have the impression you used MODIS-Aqua (2003-2016) for the satellite matchups. Please clarify if you used 2003-2012 data for something else.

Page 5, line 17: which environmental variables?

Write out what N means in table 1

Figure 3b: In the caption, do you mean DMS/DMSPt ratio vs PAR (or vs DMSPt)?

Figure 7: Caption for DMS_{SAT} algorithm should be (b)

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-18>, 2018.