

Earth Syst. Sci. Data Discuss., referee comment RC1  
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## Comment on essd-2021-236

Giuseppe M.R. Manzella (Referee)

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Referee comment on "Third revision of the global surface seawater dimethyl sulfide climatology (DMS-Rev3)" by Shrivardhan Hulswar et al., Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2021-236-RC1>, 2021

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Comments to:

*Third Revision of the Global Surface Seawater Dimethyl Sulfide Climatology (DMSRev3)*

*by Shrivardhan Hulswar et al.*

The paper is well written and easy to follow. The description of mechanisms producing DMS is very clear also for non expert people. The new climatology includes new data and this is a paper added value.

Weak points are related to statistics, number of useful data for climatology, spatial and temporal distribution. These problems that are presented by the authors but not resolved.

The initial data set consisted of 872,427 data points of which only 48,567 are used after post processing. Therefore, the spatial and temporal coverage is worse than that shown in figure 1. Hence a first series of questions: in each month how many data are available in all geographical areas of  $1^{\circ} \times 1^{\circ}$ ? Does each square of  $1 \times 1$  have a statistically significant number of data points? Are the data for each month and  $1 \times 1$  areas statistically sufficient or should authors examine them seasonally?

The climatology obtained in data-poor areas with similarity-estimated VLS is not convincing, by taking into account that the phenomena under investigation are occurring at high frequency and varying from place to place.

Another point that the authors present but do not investigate is related to methodologies and technologies for data collection. Over the years they have changed and so has the data accuracy. In the paper there should be an indication of what is the final accuracy of the climatology in the various areas.

The authors should also provide information on calibration standard if exists and if used in data selection.

The problem of ecological provinces is well posed, and the authors refer to previously published articles. The 'geographically homogeneous' data can be identified with a cluster analysis. In a non-static environment it is possible that geographic homogeneity may vary over time. Authors should discuss this.

The new data included in the paper makes it interesting and publishable after major revision.

Special comments.

Figure 1 should shows the total raw data (1a) and those used for climatology (1b).

An indication of errors or accuracy in the various regions would be desirable