Review of Manville et al. Global analysis of the controls on seawater dimethysulfide spatial variability

This article represents a large-scale analysis of spatial DMS variability in the worlds oceans, building on a growing data base of high frequency measurements and applying a standardized methodology to examine characteristic length scales of variability. The analysis demonstrates a significant range in DMS variability length scales, and a statistically significant relationship between the variability length scales of DMS, sea surface height anomalies and chlorophyll. The paper is notable for its broad-based analysis of a large data set, and I think the results are helpful in characterizing DMS spatial variability across various oceanographic regimes in the context of other key environmental variables. Overall the paper is well written and the analysis seems sound. I do have a number of specific suggestions, which I feel would further improve the manuscript.

General comments

Specific comments:

Line 33 add 'loads' after 'aerosol'

Line 36 aerosol should be plural

Line 38. I found the last sentence of this paragraph a bit long and convoluted. I would shorten it, or split into two for greater clarity.

Iine 41. Add a comma after complex.

Line 60, I think the authors should cite the work of Herr et al., who designed an empirical algorithm for the NE Pacific.

Line 60 – I think the Wange et al. 2020 paper is a global ML climatology. I think it's worth citing the two recent ML-based algorithm by McNabb – one for the S. Ocean, and one for the NE Pacific.

Line 66. At this point, I think there have actually been a pretty significant number of studies looking at sub-mesoscale variability – the authors can pick a few of their favorites and cite them here.

Line 66. Here and elsewhere, I 'm not sure that the authors are really examining 'processes' governing DMS variability. Rather, they are looking for statistical association and speculating on potential mechanisms. I think the distinction is perhaps subtle, but important.

Line 70. The question of appropriate interpolation radii is key to all of the previous climatologies. I think this concept should be introduced earlier, when discussing existing algorithms, and their challenges in resolving fine-scale variability.

Line 78. I'm not sure I'd say there is a 'wealth' of high frequency data – maybe a 'growing number of high frequency DMS measurements' is more accurate.

Line 103. I would have thought that sampling distance, rather than time would be a better cut-off. Based on the data used, can the authors list the maximum sampling interval included in the data? I think it appears later, but would be good here.

Line 105. I think it would be better to cite the earlier papers that introduced mass spectrometry in the early 2000s.

Line 111. Again, I'm not sure that the analysis presented here allows the authors to explore 'processes'.

L117-118 – Is there a reason PAR wasn't included, or even the diffuse attenuation coefficient (kd), to assess light variability? Aqua MODIS has 4 km products for each variable that could be compared with the current suite of variables. This would be highly valuable, in my opinion, and would strengthen the discussion points in L326-327 & L370-375.

Line 118. I presume that the MODIS chl were matched to the month and year corresponding to the DMS measurements, but it would be good to state this explicitly. I realize that lower temporal averaging increased cloud cover data losses, but one month is a long time for DMS to change. It is at least worth mentioning the potential temporal disconnect between in situ DMS measurements (which can change significantly over even just a few days) to a monthly-averaged Chl product. Also, how were the differing spatial resolutions of satellite SST, Chl and SSHA, in relation to DMS, handled? Were the closest matching pixels chosen as "coincident", or were these variables upsampled (interpolated) to a finer resolution to match?

Moreover, the mean VLSSSHA of 15.76 is lower than the resolution of the satellite product used (~18 km). If the SSHA data was not upscaled, shouldn't the minimum VLS determined be constrained by the raw sampling resolution (i.e. VLS shouldn't be able to detect variability within a single, averaged grid cell/pixel)? If the VLSSSHA is more closely representing an approximation, than a caveat should be noted.

General methods question. What is the potential advantage / disadvantage of linearly-detrended the data prior to VLS analysis? I don't think this was done here, and the data in Fig. 2a certainly a show a strong linear trend. What is the impact of this on the analysis?

First couple of paragraphs of the results. I think some more statistical analysis is warranted to examine significant differences between different values mentioned. I would also suggest representing mean and GSD values at xx ± yy.

Section 3.2

I found the first paragraph a bit 'jumbly', as it moved quickly across very different regimes. In general, I didn't think the spatial analysis was all that clear or convincing. I think the statement about 'consistently' small VLSdms in the south tropical gyres has some notable exceptions which seem to be glossed over (same comment for line 266). Line 210 – it would be good to report the actual values for different areas (e.g. East Equatorial Pacific).

Line 231. The highest explanatory power comes from using SSHA and Chl as predictive variables. So I don't understand why all statistically significance is lost when SST is added. To my understanding, you would simply fail to get a lower r2 in the MLR when adding an extra variable with no correlation to the dependent variable.

Line 261 – 'broadly above average' seems a bit vague to me.

Line 262 'invariability' seems a bit awkward – maybe 'the longer length scales of DMS spatial variability ….'

Line 275 Herr et al. (2019) explicitly link DMS variability with SSHA and eddys. See figure copied below

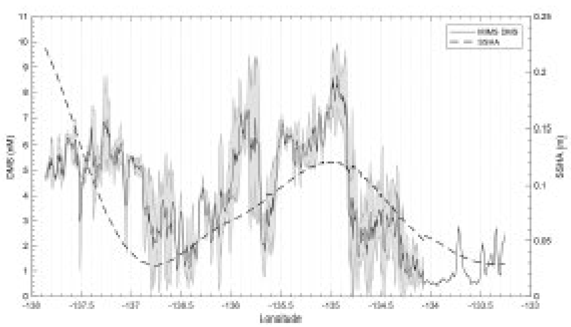

**Figure 5.** Line plot of sea surface height anomaly (SSHA) on 15 July 2016 and observed DMS concentrations between 14 and 16 July 2016 along T1. DMS along the T1 transect is highest in those areas influenced by positive SSHA values.

Line 288. Are the VLSs of Chl and SSHA correlated to each other? If so, is that a problem in the analysis, creating a potential statistical artifact?

Line 294. I think there should be more explicit discussion of Rossby radii here as a structuring mechanism for different length scales of variability. Some of this material comes up later in the text (lines 346 – 350), but I think it would be good here.

Section 4.4. I'm not sure what this title is supposed to mean – I didn't find it too descriptive.

L321-323 – This argument could be extended using this study's results. The VLS for all variables is about 20 km or less, but current regression-based empirical algorithms (cited in L322) have been built using predictor data interpolated to 1o (111 km) or courser resolution. The VLS results do support the choice of predictors used in these studies, but they also suggest patterns associated with mesoscale variability (particularly associated with SSHA) would be obscured at those resolutions, motivating modelling work at finer resolutions (e.g. McNabb & Tortell 2022 cited).

L326-327 - This is partially true, but they haven't assessed light variability which is parameterized in all three algorithms cited in L322. The sentence structure also needs revising here.

Line 378 – first line of conclusions. I think the observations are regional and the data set is global. I would re-write this to clarify.

Fig. 4 (& relevant to S1, S3) – It looks like the colorbar diverges at the global geometric average VLSDMS. It might be helpful to make a note of this in the caption to draw the reader's attention.

Figure 5. I would suggest adding another panel to show the relationship between predicted (from the best MLR model) and observed VLSdms.

END OF REVIEW