

Interactive comment on “Patterns and drivers of dimethylsulfide concentration in the northeast Subarctic Pacific across multiple spatial and temporal scales” by Alysia E. Herr et al.

Anonymous Referee #2

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As the title indicates, this manuscript describes DMS and DMSP spatial and temporal distribution patterns with high-resolution field data collected in coastal waters of the NE Pacific. The DMS patterns are compared to historical field data in a publicly available database, collected mostly in previous decades and further offshore. Measurements of some of the rates involved in DMS cycling allow for interpretation and discussion of the likely controls (or drivers) of the next patterns observed. No single physical or biological parameter accounted for the DMS/P variability observed and described as a whole in the region; rather probable controls change in relative importance within sub-regions, as has been shown in other studies. This variability only confirms the already described complexity of the DMS/P/O biogeochemical system at any one place and

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time. . The manuscript is very well written and is a pleasure to read. . However, the authors should decide whether they will focus this report on the NE Pacific ONLY and hence solely references for this region will be used. Right now, the manuscript ignores many references to similar conclusions in other regions or even in nearby SE Bering Sea (Barnard et al.) while occasionally using references from other regions to support its own conclusions (eg. North Sea, Southern Ocean). The authors miss a unique chance to strengthen the conclusions of this manuscript. . . Summary of Comments on bg-2018-411 review.pdf Page: 3 -Line 1: what is L11? Lana et al. 2011? Please check throughout ms -Line 18: Holligan et al 1987 first reported the link between DMS and fronts; even if it was not in NESAP waters but NAtlantic waters . Page: 6 -line 7: Please report BP data in carbon units, not leucine units so they can be compared with PP data and with other studies. -Line 8: Hence, as done previously by Kiene et al, DMSPp can be estimated such that the DMSP/chl ratios are estimated with both parameters in the particulate fraction; only makes a difference where and when [DMSPd] are high. Fig 4 shows a match for DMSPt and DMSPd measurements; hence, DMSPp can be calculated. -line 9: with a GC-FPD discrete method -line 12: sampleS -line 15: “The estimation formulas” used? . Page: 7 -line 6: where were the SSS and SST matches obtained from? The PMEL data set does not provide them. . Page: 8 -line 21-22: I had come to assume that L11= Lana et al 2011. If yes, please reword this sentence -line 23: please insert “The PMEL” data were first... -line 32: replace ‘that’ with ‘those’ . Page: 10 -similar DMS/P-NPP relationship by Bell et al for the North and South Atlantic along the AMT transect and by Matrai et al for the Barents Sea. Should be addressed in the Discussion. . Page: 11 -2nd paragraph: because similar conclusions of prymensiophytes vs other phyto groups and DMS/P patterns were drawn by Barnard et al 1984? in the SE Bering Sea, they should be definitively mentioned in the Discussion. . Page: 15 -line 1: something is missing before ‘and’; or remove ‘and’; or replace by ‘a’? . Page: 16 -section 4.1 and elsewhere: Since references beyond the NESAP are already included, other -mostly older- very pertinent references have been suggested in this review and should be included to strengthen

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the arguments made. -line 11: but not in polar waters (Turner et al for southern ocean; Matrai et al for Barents Sea) -line 13: please insert after 'physiological state' ", as previously shown by Gabric et al. (1999)" [Barents Sea] -line 14: please insert 'e.g.' in front of the refs listed, as there are other pertinent refs as well -line 27: please insert 'and elsewhere' after NESAP. -line 31: which studies? add references! . Page: 17 -line 6: waterS -somewhere in this page: A similar conclusion on the influence of prymeniophytes in 3 coastal domains just a bit north in the NE Pacific was reported by Barnard et al 1984. Please include. -line 19: update the McParland and Levine ref, as the ms has moved on in its review process -line 26, after the Sunda et al. 2002 ref. Please address the observation that a post-bloom = also when bacterial activity is highest and DMS_{Pd} > DMS_{Pp}, as phyto cells become leakier (eg, Matrai and Keller 1993 and Malin et al 1993 for cocco blooms; Stefels et al review as well) . Page: 18 -line 3: add a few references after 'cell lysis' for all processes mentioned -line 6: instead and/or in addition to the variables reported herein? -line 8: which 'studies'? add refs (e.g., xxx) -line 9: is this only for coastal waters of the NESAP? Or elsewhere also? Please specify. This is not a new observation for other regions (e.g., Turner et al Southern Ocean, Matrai et al. Barents Sea) -line 21: please insert "in other regions" after 'previous studies'! -line 29: please insert "in other regions" after 'previous studies' -line 31-32: check punctuation . Page: 19 -line 2: please convert to carbon units! -line 7: it IS possible -line 21-23: delete this paragraph. It is naive and does not add anything . Page: 20 -section 4.5: Both Hind et al 2011 and Deutsch et al 2009 in the Eastern SPacific and globally, respectively, combined Longhurst provinces and DMS-based algorithms to test their predictions. Both studies should be referenced and included here as they discussed the strengths and weaknesses of such DMS predictive algorithms. Hind et al. also include many of the variables discussed in this study, even the presence of eddies and upwelling. . Page: 22 -line 1: supportS -line 1: please insert "in summer" after 'hotspots' -line 10: By US NSF rules, shouldn't all data be submitted to a long-term data repository? . Page: 24-32 References -check subscripts and italics for scientific names throughout -format references; remove all

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caps throughout . Page: 34 -please insert "(in parenthesis)" at the end of the Table 2 title. That's what is in (xxx), right? . Page: 41 -Fig 4d y-axis: why not DMS_{Pp}/chl a? both are particle-bound variables These are discrete stations. -Fig 4f: can you please report BP in Carbon units? otherwise it cannot be compared with PP or other studies . Page: 42 -Fig 5: the y-axis scale is missing . Page: 43 -same comments as for Fig 4 . Page: 44 -same comments as for Fig 4 . Page: 46 -Fig 9: Given the tables, fig 8 and the fact that the differences in the DMS flux estimates is so small, this figure does not add much and could be removed

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