Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-172-AC2, 2019

Interactive comment on "Acrylic acid and related dimethylated sulfur compounds in the Bohai and Yellow Seas during summer and winter" *by* Xi Wu et al.

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Received and published: 3 September 2019

Wu et al. measured DMS(P) and AA concentrations across different oceanographic regimes, depths and seasons, rate measurements of DMS(P) and AA degradation and production, and AA concentrations in porewaters. AA is a product of DMSP cleavage and potentially an important carbon source, but little is known about global AA dynamics. I commend the authors for their expansive assessment of AA dynamics in the context of DMS(P) cycling. These measurements reflect an important contribution to knowledge about AA's role in the marine microbial ecosystem. However, the current manuscript requires significant improvements for accuracy and presentation clarity.

Specifically: 1. Statistical tests are missing/incomplete throughout the manuscript. Any conclusions deemed significant should be supported by statistics. Overall, results should be made more quantitative.

Reply: According to the reviewer's suggestion, we will add more quantitative descriptions in Results section and statistics to support conclusions deemed significant in the revised manuscript.

2. More biological measurements are necessary to support conclusions. Only Chla concentrations are reported, which is well established to be a poor predictor of DMS(P) concentrations. This is not a focus here as authors have already reported they can add more biological parameters.

Reply: We agree with the reviewer. We will discuss other biological parameters including nutrients, phytoplankton and bacterial data in the revised manuscript.

3. There are a significant number of citation errors in (both missing and incorrect citations) and I highly suggest the authors review their citations fully before resubmitting. Additionally, many conclusions are "overstated", meaning the strength of the wording should be edited.

Reply: Thank you for your suggestion. We will check all citations very carefully and correct the errors. And the strength of the wording will be improved dramatically in the revised manuscript.

4. The clarity of the manuscript would greatly benefit from dividing the Results into Results and Discussion. As it reads now, the results for each section are being explained in pieces but no full story of all the results is tied together.

Reply: According to the reviewer's suggestion, we will divide the previous Results section into Results and Discussion sections in the revised manuscript.

5. Finally, the motivation of the manuscript should be clearer. I fully recognize that these measurements of AA will improve knowledge, but why is it important to fill that

gap? What unknowns do these results answer about AA cycling? Given the expansive AA measurements, this manuscript could test more specific hypotheses. Additionally, for writing clarity, I would recommend focusing the questions towards AA, and using the DMS(P) as supporting evidence. Stating clear hypotheses at the beginning of the manuscript, addressing any significant errors mentioned below and splitting Results and Discussion will make for a very strong manuscript that will significantly improve knowledge about AA cycling.

Reply: Many aspects of DMS and DMSP have been well documented, but the processes affecting AA concentrations in marine waters are poorly known. Furthermore, AA is an important source of carbon to the microbial community. Therefore, it is important to fill the gap. These results indicated other potential sources of AA (e.g. terrestrial inputs from rivers) besides production from DMSPd, determined if temperature was a key controlling factor on AA dynamics through winter and summer comparison, and provided new measurements of AA in porewater. We supposed that changes of phytoplankton and bacteria species and abundance played important roles on AA dynamics and expected these hypotheses could be test in this manuscript. We meant to focus on AA and use the DMS(P) as supporting evidence. We will emphasize this in the revised manuscript. According to the reviewer's suggestion, we will state the above-mentioned hypotheses at the beginning of the manuscript, address significant errors and split into Results and Discussion in the revised manuscript.

Major comments Line 109: Only dissolved AA was measured. Please make this clear and consistent with abbreviations for DMSP.

Reply: Yes, only dissolved AA was measured. We will check the entire manuscript and used the abbreviation AAd for the dissolved AA in the revised manuscript.

Line 161, 209, 234,241: Riverine/terrestrial runoff is argued to be a critical input of AA into the systems studied but are lacking direct evidence. Are there actual measurements of riverine AA concentrations in Liu 2001 that could be reported? How do their

measurements compare to yours?

Reply: Liu (2001) found 90 kinds of organic pollutants including acrylic acid in Yalu River, but the exact concentrations were not reported. We could not compare our results with theirs directly, but we thought it could be a direct evidence for the terrestrial input of AA.

Line 173: I only see that DMSPt and Chla coupled (e.g. lowest DMS corresponds to highest Chla). Please edit so as not to overstate trends, and use qualitative statements and tests for significance.

Reply: We are sorry for confusing you. As horizontal distributions of DMS and DMSP in surface seawater of the BS and the YS has been described by Jin (2016), we did not cite those figures from her MS thesis in our previous manuscript, which made you not see their coupled relationships with Chl a. We will add figures of DMS, DMSPd and DMSPp distributions in surface seawater and describe their relationships using quantitative statements and tests for significance, as indicated below. "DMS and DMSPd presented positive correlations with Chl a (DMS: $r=0.418$, $n=50$, $p<0.01$; DMSPd: $r=0.351$, $n=50$, $p<0.05$)."

Line 174, 198: Correlations are likely impacted by measuring only dissolved AA, as the majority of AA produced from DMSPd degradation would be expected to be stored intracellularly, whereas the majority of DMS produced would be expected to be found in the dissolved phase. As well, DMS is more diffusive and reactive, and therefore inputs of DMS are likely more complicated than dissolved AA (Tyssebotn et al. 2017). Please consider these comments in the Discussion.

Reply: Thanks for your suggestion. We will add these comments in the Discussion section of revised manuscript.

Line 175-177: It is well-established that Chla rarely correlates with sulfur compounds because production is specific to community composition/location (Lana et al. 2011;

Galí et al. 2015; McParland and Levine 2019). I suggest authors review comments about these relationships throughout manuscript. Incorporating new parameters (phytoplankton type abundances and bacterial abundances) will better reflect the role of biology in these dynamics.

Reply: Thanks for your suggestion. We will review comments about these relationships throughout the manuscript and incorporate phytoplankton type abundances and bacterial abundances to better reflect the role of biology in these dynamics in the revised manuscript.

Line 178-192: Such high AA concentrations in porewater is very interesting, and should be better highlighted...these concentrations are ~an order of magnitude greater than in the water column! I suggest making qualitative comparisons with previously measured AA concentrations and highlighting the significance of these new measurements.

Reply: According to the reviewer's suggestion, we will make quantitative comparisons with previously measured AA concentrations in porewater and highlight the significance of these new measurements in the revised manuscript.

Line 182: Why are the concentrations so different? Location/sediment types? This would be an ideal place to discuss bacterial abundances from previous studies if appropriate.

Reply: Yes, location and sediment types could be the reason for different concentrations. According to the reviewer's suggestion, we will discuss bacterial abundances and compare with previous studies in the revised manuscript.

Line 188: If AA in porewater and bottom water are not significantly correlated, what is the supporting evidence for the statement that the source of high AA in bottom water is porewater?

Reply: We are sorry for the inaccurate statement. Nedwell et al. (1994) reported that DMS could emit from sediments to water column, so we speculated AA could also

emit form porewater to bottom seawater. We will sample vertical cores of sediment to measure the variations of AA concentrations in bottom seawater with time using the method referred in Nedwell et al. (1994) in future cruises. At this time, we will revise that statement as "We speculated AA might emit form porewater to bottom seawater".

Line 209-212: Figure 3 as well as associated text are confusing. Are these relationships significant? Are the slopes significantly different than zero? (They do not look so). I'm also confused why AA was normalized to salinity as this is the x-axis? You'll get the same answer. As is, I would remove Figure 3. The relationships do not look significant and do not support conclusions.

Reply: Thank you for your suggestion, we will remove Figure 3.

Line 226: I'm confused by conclusion that this negative correlation is linked to enhanced lyase activity? If low salinity promotes lyase activity, then we would expect a positive correlation of salinity and DMSPt (i.e. low salinity=more lyase activity=less DMSPt/DMSPp due to cleavage).

Reply: We are sorry for confusing you. We agree with the reviewer. We will revise that sentence as below. "DMSP might be expelled extracellularly in order to reestablish cellular osmotic balance as a response to reduced salinity (Deschaseaux et al., 2014), which might have led to the negative correlation between DMSP and salinity."

Line 234: At the surface, where terrestrial runoff is expected to impact concentrations, the excess does not appear to be 'significant'...(AA at 10m ~60nM, DMSPt at 10m ~55nM, difference =5). Please edit this statement for clarity using quantitative statements and/or justify the use of 'significance' when describing the excess difference in AA and DMSPt.

Reply: As Yellow River is the world's largest river in terms of sediment load and flows into the NYS, AA may be absorbed on those sediments and sink to bottom. Therefore, terrestrial runoff may impact AA concentrations along the vertical profiles of transect

B12-17 rather than only at the surface. Along the transect B12-17, the average AA concentration was 34.60 nmol L-1 and more than 2 times of that of DMSPt (15.45 nmol L-1). According to the reviewer's suggestion, we will remove the word 'significant' and state it using quantitative statements, as "the average value of AA was more than 2 times of that of DMSPt".

Line 247: Is there a statistically significant relationship between Chla and DMSPt? Please use qualitative statements, rather than listing the order of concentrations.

Reply: No statistically significant relationship was found between Chl a and DMSPt. We will revise that sentence as below. "This suggests that large amounts of phytoplankton biomass may induce high concentrations of DMSPt."

Line 255: Please revise this statement. . .yes DMSP could be a cryoprotectant, but this is most relevant to ice algae and temperatures in freezing conditions.

Reply: Thank you for your suggestion. We will revise that statement as below. "van Rijssel and Gieskes (2002) also found a negative effect of temperature on the DMSP content per volume."

Line 280: This entire section (3.5) needs statistical tests to support statements. Example: are the rates of DMS production significantly lower than rates of DMSPd degradation in summer? (Remember to report the statistical test and p-values in text/methods).

Reply: Thank you for your suggestion. We will add statistical tests and p-values and edit the strength of the wording for the entire section 3.5.

Line 280: Was Chla measured at beginning of experiments? This could better support statements about biomass productivity altering rate measurements (Cho and Azam 1990).

Reply: We are sorry for not measuring Chl a at beginning of experiments. We have the Chl a data at these stations. It may not have a big difference from the Chl a concentrations at beginning of experiments because the seawater used for experiments were

also sampled from the Niskin bottles. We will discuss the relationships between Chl a and reaction rates in the revised manuscript.

General comment: There is a significant order of magnitude difference between absolute AA concentrations presented here and recently published measurements from the Gulf of Mexico. As well, uptake rates of AA were are an order of magnitude less than the degradation rates of AA measured here (Tyssebotn et al. 2017). Please acknowledge these previous measurements and describe potential reasons for differences. The AA dynamics presented here by Wu et al. are an exciting contribution to our knowledge of AA and should be compared with all previous work.

Reply: Thank you for your suggestion. We will compare the absolute AA concentrations and degradation rates of AA with previous work and explore the reasons for the differences in the revised manuscript.

General comment: I recommend the authors consider how the measurements of AA dynamics here could help inform a better understanding of the bacterial 'switch' hypothesis for which the environmental drivers of are still debated (Kiene et al. 2000; Slezak et al. 2007; Levine et al. 2012).

Reply: Thanks for your suggestion. We will discuss how bacteria species and abundance affect AA dynamics in the revised manuscript.

Minor comments Overall the manuscript should be 'cleaned up' in terms of English but also small text errors. Some errors 'overstate' the significance of the statement, but most do not inhibit reading.

Reply: Thank you for your suggestions. We will polish the entire manuscript and correct text errors and wording errors.

Line 29: Please rephrase statement about acid rain. DMS is correlated with the natural acidity of rain (as stated now, implies that DMS is the cause of acid rain).

Reply: Thanks for your suggestion. We will revise this sentence as "DMS is correlated with the natural acidity of rain."

Line 41: Please rephrase minor producers to 'low producers'.

Reply: Thanks for your suggestion. We will revise 'minor producers' to 'low producers'.

Line 41: I suggest removing the "For example" part of this sentence as you state all of the well-known high producers (i.e. it is not an example). When describing low producers mention other low producer types (McParland and Levine 2019).

Reply: According to the reviewer's suggestion, we will remove the sentence of high producers and describe other low producer types, as indicated below. "DMSP distributions are also controlled by phytoplankton species, among which diatoms, flagellates, Prochlorophytes and cyanobacteria are low minor producers of DMSP (McParland and Levine 2019)."

Line 43: this sentence is repeating line 39, please be more concise and edit accordingly

Reply: According to the reviewer's suggestion, we will delete the sentence in line 43 and rephrase the sentence in line 39, as indicated below. "As an antioxidant, a cryoprotectant, and an osmolyte in marine phytoplankton, the production of DMSP is influenced by environmental parameters such as salinity (Stefels, 2000), temperature (Kirst et al., 1991), and oxidative stress (Sunda et al., 2002)."

Line 47: AA should be defined here (even though it is properly defined in Abstract)

Reply: Yes. We defined AA as the abbreviation of acrylic acid in line 46 when it was first mentioned in text.

Line 54: Kinsey and Kieber 2016 is incorrect citation for this statement

Reply: We are sorry for the incorrect citation. We will cite another reference of Noordkamp et al., 2000 for this statement.

Line 55: The use of 'always' here is too strong for the current state of knowledge

Reply: Thank you for your suggestion, we will remove 'always' in the revised manuscript.

Line 58: Alcolombri et al. 2015 is incorrect citation, this paper does not measure anything in situ. Additionally, I would expand these citations as there are so many more studies that have conducted the work described in this sentence besides the two.

Reply: We are sorry for the incorrect citation. We will remove that citation and add others including "Lana et al., 2011; Levine et al., 2012; Tyssebotn et al., 2017".

Line 80: complicated

Reply: Thanks for your correction. We will revise 'complicate' to 'complicated'.

Line 86: How was surface sediment sampled? And where? What time of day collected?

Reply: Sediments were collected using a stainless-steel box-corer and sub-sampled to a depth of ca. 3 cm. They were sampled at 12 stations shown in Table 1 during summer cruise. We will add the sampling method of surface sediment in the revised manuscript and sampling time in revised Table 1, as indicated below. "Sediments were collected using a stainless-steel box-corer and sub-sampled to a depth of ca. 3 cm at 12 stations shown in Table 1 during summer cruise."

Line 91: How was DMS sampled?

Reply: DMS sampling was conducted as soon as the Niskin bottles were on deck. 250 mL brown glass bottle were rinsed and filled to the top to eliminate any headspace in an effort to minimize partitioning into the gas phase. A 2 mL aliquot of seawater sample was directly extracted from the 250 mL brown glass bottle using a 2 mL glass syringe and injected into a glass bubbling chamber. We will add these sentences in the revised manuscript.

Line 94: Was the pre-filtered DMS sample gravity filtered? Please provide a citation for this method. Also, what size GF/F filter was used?

Reply: We did not filter seawater through GF/F filter before analyzing. We will delete that part from DMS analytical procedures.

Line 101, 117, 120: Were analytical samples run? (in duplicate, triplicate?)

Reply: Analytical samples were run in duplicate. We will add this sentence at the end of Section 2.3 in the revised manuscript.

Line 102: Again, what size GF/F filter was used?

Reply: 47 mm GF/F filter was used here. We will add the size in the revised manuscript.

Line 104: How long were the samples allowed to oxidize for?

Reply: The samples were allowed to oxidize for 2 days. We will add a sentence in the revised manuscript as indicated below. "To fully oxidize pre-existing gaseous DMS, the DMSPt and DMSPd samples were incubated in the dark at room temperature for 2 days."

Line 124: Has this methodology for incubations been performed before? Please cite if so.

Reply: Yes. GBT inhibition method for DMSPd degradation was performed by Kiene and Gerard (1995). Methods for photochemical and microbial degradation of AA were performed by Wu et al. (2015). We will add these citations in the revised manuscript.

Line 126: Why were syringes used for incubation? Were they gas-tight?

Reply: Yes. These syringes were gas-tight. It was convenient to collect samples at 0, 3, and 6 h if using syringes. We could just push the plunger to let seawater flow out. Meanwhile, it kept the rest seawater in syringes isolated from air.

Line 131: I don't believe Kiene et al. or Vila-Costa et al. address preferential GB uptake?

Reply: We are sorry for misunderstanding these articles. We will revise that sentence and cite another reference, as indicated below. "and acts as a competitive inhibitor of DMSP (Kiene et al., 1998)."

Line 132: Please address how rates were calculated? Were regressions/fits statistically significant?

Reply: According to the reviewer's suggestion, we will add description about rates calculation, as indicated below. "Linear regression equations were fit to the DMSPd, DMS and AAd time course data and the apparent rates were estimated as the differences between the slopes of samples with and without GBT." Regressions at most stations were statistically significant.

Line 147: Kiene 1996 is incorrect citation, they did not measure AA in their study?

Reply: No, Kiene (1996) did not measure AA in his study. He determined the kinetics of DMSP(d) degradation by running one with spike additions of DMSP and the other one without additions as control. We thought this method could be applied to AA degradation, so we cited this article. We will remove this citation as the reviewer suggested.

Line 150: Suggestion if you do split into a Results and Discussion section... results for Section 3.1 and 3.2 should be combined for a clearer description of the differences in summer and winter.

Reply: According to the reviewer's suggestion, we will split into Results and Discussion sections and combine results for Section 3.1 and 3.2.

Line 151: How are the contours spaced? Center of sea contour looks like 5 ug/L, not 7.07ug/L?

Reply: Kriging method was used for interpolating contours. These circles inside the contour of 5 μg L-1 were too small to be marked as their real concentrations. As we measured, the center point was station B61 with the Chl a concentration of 7.07 μg L-1.

Line 163, 170: Chengshan Cape and Changjiang Estuary not on maps

Reply: According to the reviewer's suggestion, we will add Chengshan Cape and Changjiang Estuary on maps.

Line 172: The comment about MS thesis belongs in methods

Reply: According to the reviewer's suggestion, we will move the comment about MS thesis to Material and methods section.

Line 178-192: I suggest moving results for porewaters to be part of the depth profile results as it seems more relevant to depth distributions, not surface distributions.

Reply: According to the reviewer's suggestion, we will move results for porewaters to be part of the depth profile results.

Line 185-187: This sentence should be re-written for clarity

Reply: According to the reviewer's suggestion, we will re-write this sentence, as indicated below. "The large amounts of intracellular DMSP could be cleaved to AAd by DddY, which is as the only known periplasmic DMSP lyase and widely present in β-, γ-, δ- and ε-proteobacteria which are the dominant bacteria communities in the surface sediments of the BS and the YS (Li et al., 2017;Liu et al., 2015a;Xie et al., 2017)."

Line 198: 'was not correlated with' (remove the word 'any')

Reply: According to the reviewer's suggestion, we will remove the word 'any'.

Line 203: I think this should read 'main phytoplankton type'? Species likely changed based on the small/big cell statement following

Reply: Yes, we will revise 'main phytoplankton species' to 'main phytoplankton types'.

Line 204: should read 'small diatoms in winter and larger diatoms in summer'

Reply: According to the reviewer's suggestion, we will revise to 'small diatoms in winter and larger diatoms in summer'.

Line 214: As you discuss everything in context of North to South in the preceding text, for clarity I would order these transects in the same way (same for the order in Figures 4,5 and Table 1).

Reply: Thanks for your suggestion, we will order these transects North to South in text, Figures 4, 5 and Table 1.

Line 213: If you split Results section, results of Section 3.3 and 3.4 could be combined for clarity.

Reply: According to the reviewer's suggestion, we will split into Results and Discussion sections and combine results of Section 3.3 and 3.4 in the revised manuscript.

Line 218: "Concentrations of Chla, AA, DMS" remove this sentence, it should be a part of caption.

Reply: According to the reviewer's suggestion, we will remove that sentence in the revised manuscript.

Line 219: 'Both Chla and DMS did not displayed...' this sentence does not make sense to me

Reply: We are sorry for confusing you. We will revise that sentence to 'Neither Chl a nor DMS displayed...'.

Line 230: suggested change "...and highest concentrations were observed in..."

Reply: Thank you for your suggestion. We will change that sentence to "...and highest concentrations were observed in...".

Line 239: Correlations are not causation... using the word 'prove' is an overstatement, please edit.

Reply: Thank you for your suggestion. We will change the word 'prove' to 'indicate'.

Line 241: I'm confused by this statement. What did Asher et al. 2017 find that indicates the order of average concentrations demonstrates that both DMSPd and DMSPp produce DMS? Please edit.

Reply: We are sorry for the incorrect citation and statement. We will revise this sentence, as indicated below. "The higher values of DMS than DMSPd might be produced through the intra-cellular cleavage of phytoplankton DMSPp catalyzed by the enzyme DMSP lyase and the photochemical and biological reduction of DMSO to DMS, while the higher values of AAd than DMSPt indicated that there were terrestrial sources of AAd besides the contribution from in situ DMSP degradation along the three transects."

Line 255-260: DMS(P) correlations with both salinity and temperature may be due to a cocorrelation of these abiotic parameters themselves, please use caution in stating these conclusions and incorporate statistical tests appropriately.

Reply: We agree with the reviewer. We will add this comment in the revised manuscript.

Line 258-259: Kiene and Service 1991 looked at DMS production from dissolved DMSP, not particulate. I believe you are discussing a correlation of total DMSP. Please edit for clarity.

Reply: Thanks for your suggestion. We will revise 'DMSP' to 'DMSPd' for clarity.

Line 269: This should be "Figs. 4 and 5" I believe?

Reply: We are sorry for the typos. We will correct "Figs. 3 and 4" to "Figs. 4 and 5" in the revised manuscript.

Line 294: should be "low bacterial abundance" instead of 'poor'

Reply: Thank you for your suggestion. We will revise it to "low bacterial abundance".

Line 336: what does "and so on" refer to? An unknown source? Please be more specific.

Reply: According to the reviewer's suggestion, we will revise "and so on" to "other

unknown sources".

Figure/Table comments Figure 2: I find the labels of summer/winter and Chla/AA on the plots very helpful but please also label the panels (a,b,c,d) in figure and reference in the caption (consistent with other figures)

Reply: According to the reviewer's suggestion, we will label the panels (a, b, c, d) in figure and refer them in the caption.

Figure 3: As mentioned above, I do not think this figure is necessary and it may be more useful to replace with similar surface plots of DMS and DMSP for summer and winter (like Figure 2).

Reply: According to the reviewer's suggestion, we will remove that figure and replaced with surface plots of DMS and DMSP for summer and winter shown in Jin (2016) and Sun (2017).

Figure 4 and 5: Again, reversing the order that the transects are presented to be North to South will make figure clearer. Also the method for interpolating contours should be reported (either in figure captions or methods), and the black dots (I assume sampling points) should be described. Adding temperature for other transects would make for better consistency.

Reply: Thank you for your suggestions. We will order the transects North to South, report the kriging method for interpolating contours in figure captions, describe the black dots as sampling points, and add temperature for other transects.

Table 1: Again, I recommend ordering table to be North to South. Caption should better define what 'Surface' refers to (all three sampling sites?) and what depth the transect values reported are.

Reply: We will order the transects North to South. 'Surface' refers to "surface seawater of the whole study area (the BS and the YS)". The transect values are the average of the whole vertical profile of each transect. We will define these in Table 1 caption in the revised manuscript.

Table 2: What correlation test was used? Additionally, please address in the methods how temperature and salinity were measured (i.e. CTD profile or was salinity actually measured?).

Reply: Pearson correlation test was used here. We will add this sentence in figure caption and address CTD profiles of temperature and salinity in Material and methods section.

Table 4: Very minor, but the table would be easier to read if the abbreviation of BS and SYS are added above the transect station names. Also, these experiments were reported to be conducted in duplicate so please report biological errors for rate measurements.

Reply: Thank you for your suggestions. We will add the abbreviation of BS and SYS above the transect station names and report biological errors for rate measurements in revised Table 4.

References Cho, B., and F. Azam. 1990. Biogeochemical significance of bacterial biomass in the ocean's euphotic zone. Mar. Ecol. Prog. Ser. 63: 253–259. doi:10.3354/meps063253 Galí, M., E. Devred, M. Levasseur, S. J. Royer, and M. Babin. 2015. A remote sensing algorithm for planktonic dimethylsulfoniopropionate (DMSP) and an analysis of global patterns. Remote Sens. Environ. 171: 171–184. doi:10.1016/j.rse.2015.10.012 Kiene, R. P., L. J. Linn, and J. A. Bruton. 2000. New and important roles for DMSP in marine microbial communities. J. Sea Res. 43: 209–224. Lana, a., T. G. Bell, R. Simó, and others. 2011. An updated climatology of surface dimethlysulfide concentrations and emission fluxes in the global ocean. Global Biogeochem. Cycles 25: n/a-n/a. doi:10.1029/2010GB003850 Levine, N. M., V. a Varaljay, D. a Toole, J. W. H. Dacey, S. C. Doney, and M. A. Moran. 2012. Environmental, biochemical and genetic drivers of DMSP degradation and DMS production in the Sargasso Sea. Environ. Microbiol. 14: 1210–23. doi:10.1111/j.1462- 2920.2012.02700.x

McParland, E. L., and N. M. Levine. 2019. The role of differential DMSP production and community composition in predicting variability of global surface DMSP concentrations. Limnol. Oceanogr. 64: 757–773. doi:10.1002/lno.11076 Slezak, D., R. P. Kiene, D. a. Toole, R. Simó, and D. J. Kieber. 2007. Effects of solar radiation on the fate of dissolved DMSP and conversion to DMS in seawater. Aquat. Sci. 69: 377–393. doi:10.1007/s00027-007-0896-z Tyssebotn, I. M. B., J. D. Kinsey, D. J. Kieber, R. P. Kiene, A. N. Rellinger, and J. Motard-Côté. 2017. Concentrations, biological uptake, and respiration of dissolved acrylate and dimethylsulfoxide in the northern Gulf of Mexico. Limnol. Oceanogr. 62: 1198–1218. doi:10.1002/lno.10495

Reply: Thank you for listing these references. We will add them in the revised manuscript.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-172, 2019.