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.
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.
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.
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.
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.

<u>Major comments</u>

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

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?

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.

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.

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.

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.

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.

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?

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.

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).

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.

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

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.

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).

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

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.

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).

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.

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).

Line 41: Please rephrase 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).

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

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

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

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

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.

Line 80: complicate**d**

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

Line 91: How was DMS sampled?

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?

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

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

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

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

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

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

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

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

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.

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

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

Line 172: The comment about MS thesis belongs in methods

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.

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

Line 198: 'was not correlated with' (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

Line 204: should read '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).

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

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

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

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

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

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.

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

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.

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

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

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

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)

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).

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.

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.

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?).

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.

References

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