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Interactive comment

Interactive comment on "Implications of sea-ice biogeochemistry for oceanic production and emissions of dimethylsulfide in the Arctic" *by* Hakase Hayashida et al.

Anonymous Referee #2

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General comments: The study of Hayashida et al. introduces a new coupled sea ice-ocean ecosystem-sulfur cycle model. The purpose of the model is to determine the DMS,P production of Arctic sea ice microbial communities and its contribution to oceanic emissions of DMS in the Arctic. This contribution is important to assess since: -Oceanic DMS emissions could play a paramount role in aerosols formation in the relative pristine atmosphere of the Arctic Ocean. Hence, DMS could be a key compound regulating the regional climate. -Sea ice microbial communities generally show very high production of DMS,P, with concentrations several orders of magnitude higher than typical oceanic concentrations. -The sea ice cover in the Arctic is changing rapidly in response to regional warming, generating potential important feedback loops. In my

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Discussion paper

opinion, the general context of the work of Hayashida et al. is therefore highly relevant.

As correctly introduced by the authors, both the DMS,P production of sympagic communities and its contribution to oceanic emissions of DMS are both unfortunately currently poorly constrained. Only a handful of studies have determined DMS,P (DMSPd, and DMSPp mainly, only one or two studies have reported DMS concentrations) concentrations in Arctic sea ice, and only a couple have tackled production and removal processes and rates. Close to nothing is known about the transfer of DMS,P at the ice/ocean/atmosphere interfaces. In addition, sea ice typically shows a very high temporal and spatial variability making harsh any extrapolation of field studies that are limited in space and time. In this context, I am really happy to see the modelling effort developed by Hayashida et al. As detailed in my minor comments, it brings both interesting answers and questions to the sea ice DMS community and is therefore worth publishing.

As it is the first time such a perilous (given the complexity of the sea ice DMS cycle) enterprise is undertaken, there are however a lot of caveats and limitations that I think the authors could do a better job at presenting in the paper. This is in fact my major comment or request to the authors. The caveats and limitations are only partially tackled in the discussion and in the conclusion. I think they deserve their own section in the discussion part of the manuscript. I would like this section to: 1) Present the caveats and limitations (see my minor comments) of the model itself, in particular the fact that brine dynamics are neglected. 2) Present recommendations for observational studies, something like “what modellers need from field studies”.

This section could also tackle the second important weakness of the paper, the fact that the model is validated by only one time series study that does not even cover all the outputs of the model. Is there really not any other data available in the literature that you could have use to validate the model or at least to give an idea to the reader on the applicability of your model to other locations/ice conditions in the Arctic? The limited time frame of the Galindo et al data set which partially miss two important phases of the cycle presented by the model does not help to build confidence. I think that throughout the manuscript you could better put your results and parameters in perspective with the literature, even if you have to use the more abundant Antarctic sea ice DMS literature. I know that most of the drivers are significantly different in the Antarctic, but some basic physical and biological concepts are the same, especially if you look at fast ice studies with no surface flooding. It is also really important in this section, or in other sections of the paper, to better convince the reader that your model is applicable to other parts of the Arctic and not only to the study site of Galindo et al. A way to do this would be to better describe the study site and conditions of Galindo et al and to show that they correspond to other sites/and conditions in the Arctic.

One final major concern that I have reading the paper is the use of the term flushing throughout the manuscript. It is never described in the manuscript and I am having trouble knowing what the authors really refer to. Flooding is a very specific process defined as: “Flushing refers to the washing out of salty brine by relatively fresh surface melt water that percolates into the pore space during summer”. I have the feeling that flushing is used as another term for “release” by the authors and could in fact include other form of material release from sea ice (e.g. brine drainage, melting of bottom ice). Could you please better define this in the manuscript and replace by another term if necessary? In addition to these general comments, I also have additional minor comments and suggestions listed here after:

Specific comments: âĂć Abstract Page 1: Line 5: Please use the plural form: The result”s” of the 1-D model “were” compared. Also consider using plural for DMS emission”s”. Line 6: Please use the plural form: our result”s” reproduced. Line 7: Flushing is a very distinct process in sea ice thermodynamics. Please consider using the word “release” more generic than flushing, unless you are exclusively talking about flushing. This comment actually applies to the whole document. Line 9: “Processes that dominated the budgets of bottom-and under-ice. . .” This sentence is a little bit heavy, consider rephrasing. Line 15: “would be better constrained by new observations”.

The whole sentence sounds like a recommendation, consider using “should” instead of “would”.

âĂć Introduction Page 1: Line 18: “is a volatile biogenic trace gas” A gas is always volatile, please use either volatile or gas in the sentence. Line 19: “planktonic and microbial” Microbe relates to the size of organisms, and so includes some planktonic organisms. The use of the two words is a little bit confusing here. Line 20: “Oceanic DMS emissions also play…” I would say “can play” especially considering your next paragraph about the controversy on the global scale effect of DMS.

Page 2: Line 7,8: This sentence is very long, consider splitting it in two. Line 8: “increase and peak” I would say: “were observed to increase and peak”. Line 16: “”high concentrations at the bottom” I would say “in the bottom”. Line 19: I would stick only to Arctic studies here, so please remove Kirst et al., 1991 from the reference list, or clearly indicate that the study of Kirst et al. 1991 covers the Antarctic. Line 26: I would like to see one or two sentences saying what the outcome of the model study of Elliott et al. was and how your study differs from their approach. I think it is important to frame your study. Line 27: Please consider starting the paragraph with “In this study, we test the hypothesis that… by developing a sulfur cycle…”. Line 30: “The rest of this study…” I don’t think this part is necessary unless required by the journal.

âĂć Model description and experimental design Page 3: Line 14: “diffusive exchange of nutrients at the ice-water interface”. Does (and if yes how) the model consider the role of brine convection in the exchange of nutrients at the ice-water interface? Several authors and Antarctic based models have highlighted the importance of this process, see Vancoppenolle et al. 2010 for instance (“Modeling brine and nutrient dynamics in Antarctic sea ice: the case of dissolved silica”). Line 23: Please add “emphasized that “the” sulfur cycle represented…”. Line 32: It should be clearly stated here that cell lysis and exudation rates are taken from limited water column measurements and not sea ice/brine measurements.

Page 4: Line 4 to 7: This sentence is hard to understand, please rephrase or make 2 separated sentences. Same comment as in the abstract regarding the term “Flushing”. Line 10: Good, I like to read this, but not only for S but also for nutrients, see my previous comment. Line 14, 15: Ignoring sloppy feeding is unfortunately a weakness of the paper given the relative importance of grazing on bottom ice communities. Same for the DMSO reduction to DMS. Model setup: perhaps some general information about the Arctic-ICE Resolute passage sampling area could be provided to the reader here. Typical ice thickness developing, type of ice (thickness, texture), typical date of formation and retreat, ice deformation, typical ice and weather regimes. . . I think that somewhere you need to convince the reader that your site is somewhat representative of a more general situation e.g. Arctic coastal fast ice. Line 27: What is the total water depth at the Arctic-ICE sampling site? Line 31: Was a met tower deployed at the sampling site? If yes, why not using data from the tower? Or at least compare the two data sets. Line 32: I would like to see in addition to Dukhovskoy et al. 2016 a more general reference for the NEMO-LIM2 model. Line 34: Where the initial ice thickness, snow thickness, and melt-pond depths set based on field observations? This is not clear.

Page 5: Line 1: “to match with observations” To match with observations of the real depth of the skeletal layer, or to match with the vertical sampling resolution used by Galindo et al.? I think this should be clarified. Line 2: It is not clear from your writing if the initial biomass of ice algae was set based on the measurements of Mundy et al. (2014) as for the nutrients. Same comment for ammonium and particulate silica. Line 7: This is a strong assumption given that your initial chl a is set to 3.5 μg.L-1. In oceanic waters, for such chla concentrations you already have non negligible DMS and DMSPd concentrations. And it is definitely the case for sea ice as well, even in diatom dominated communities (see several Antarctic studies for instance, e.g. Carnat et al., 2014 “Physical and biological controls on DMS,P dynamics in ice-shelf influenced fast ice during a winter-spring and a spring-summer transitions” JGR.). How do you justify this? Line 14: Are these “field measurements in Arctic waters” referenced somewhere in the manuscript? Line 15: “Emissions”s”.

âĂć Results and discussions Page 5: Line 30: How did you deal with melting and refreezing of the snow pack (typical in early summer in the Arctic) and the formation of superimposed ice? Was superimposed ice detected in the field samples? Was superimposed ice considered as snow or ice in the obs?

Page 6: Line 9: Yes, and this is why it is important to give general information about the sampling site to the reader. Is this assumption based on observations during the 2010 campaign or during other years? Do you have a reference for this? Line 24: What about the increase in observed under-ice chla in mid-May and the localized increase in early-June? Could they be the sign of brine drainage not simulated by the model? I think the hypothesis has to be written somewhere. You see the same trend in DMSPp and to a lesser extent in DMSPd. Line 27: It would be nice to indicate here the fixed DMSP:chla ratio used and not only refer to the equations. Line 28: How strong was the relationship? Please indicate this. Line 32: How do you explain the spatial variability? Spacing of brine channels? Patchiness of ice algae in bottom ice? Hints should be given here.

Page 7: Line 9 and 10: “To the release of ice algae from bottom ice”. Through which process? Bottom sea ice melting? Hanging algae that just drop in the water? Brine drainage? Flushing? Line 13 and 14: The DMSPp sinking part does not convince me at all. What is the argument to support this? Just that the DMSPp peak rapidly disappears during the next sampling event? Do you have DMSPp measurements in deeper ocean layers during the same sampling event? It could be also that most of the DMSPp is rapidly converted to DMSPd and DMS. You do not have observations of DMS, so you cannot rule that second option out. I think that you should mention both options and not only focus on the sinking of DMPSp. Line 23 to 25: The explanation given for the mismatch between the three first observations and the model simulation in DMSPd does not convince me at all for the moment. We need to know at which snow depth day 1 was sampled and what the effect could be on DMSPd. Why would spatial variability be higher during day 2 than during any other sampling day? Is the fact that all the three observed variables (DMSPp, DMSPd, chla) were significantly higher than model predictions during day 3 not at sign that the model is missing something? What about other ancillary parameters observed during the 3 first sampling days? (community composition for instance?, stress on the algae?). Line 31: “Degradation of DMSPp to DMSPd” that you disregarded before, saying that DMSPp was sinking…see line 13 and 14.

Page 8: Line 3: “upper 10 m” perhaps add “of the water column”. Line 7: Considering the comment you make Line 28-30 of page 11 on the cell lysis and artificial DMSP release (and hence DMSP conversion to DMS) on melting of ice samples, how can we trust the 2000 nmol.L-1 value given in Levasseur 2013. How was the sea ice DMS measured in that study? Section 3.1.4.: I know there is no sea ice field observations to validate these various production and removal rates but it would be nice to at least compare them to oceanic field observations so that the reader can evaluate if the numbers make sense.

Page 9: Line 1: “due to low zooplankton biomass during the melt period” Is this supported by any kind of observation? Line 21-22: “In mid-June, the simulated DMS…” This sentence is hard to understand, please rephrase or cut in two separated sentences. Line 25-30: This whole part is a little bit hard to follow. It is hard to know when you are talking about pre bloom conditions, early bloom conditions, peak bloom conditions…I would suggest rewriting like this (line 25): “On the other hand, during the initiation of the under-ice bloom, the simulated DMS production rates by flushing were comparable to the rates of other simulated processes associated with the bloom, reaching a maximum value (>5 nmolL.d) in early June. “

Page 10: Line 1-2: What about the upward vertical migration of DMS within the brine channels? It is very likely that in an open system such as warm interconnected brine channels, gaseous compounds can migrate upwards due to buoyancy if present under the form of bubbles. Would DMS form bubbles at some point? See Zhou et al., 2013 “Physical and biogeochemical properties in landfast sea ice (Barrow, Alaska): Insights on brine and gas dynamics across seasons", or Moreau et al. 2014 "Modelling argon dynamics in first-year sea ice". Did you consider this? If not, why? I think this needs some clarification. Line 23: The use of the word "significantly" suggests that statistical tests were conducted. Is it the case? And if yes, please indicate the level of significance. If no, please use another term. Line 24-25: Please rephrase: "In comparison to the NoIceSul run, the NoIceBgc run simulated under-ice DMSPd and DMS concentrations that were higher in late June onward." Line 28: "Heightened"... "higher"? Line 27-28: This needs further explanation. How does the absence of ice algae increase the nutrients available to under-ice algae? It is not clear from you text how they get access to the same nutrient pool? Is it the fact that the nutrients not consumed in bottom ice are "flushed" to the under-ice water ? Or the fact that the under-ice nutrient pool is somehow consumed by bottom ice algae? If yes, how? Also this whole section lacks comparisons with observations of nutrient/algae dynamics from the literature. It would also be nice here to remind here that the effect of brine convection on nutrient dynamics (see Vancoppenolle work) was not taken into account in the model. Line 34: This sentence is too long and complex, please split in two or simplify.

Page 11: Line 4: of "the" sea ice ecosystem. Line 10: Remove one of the "the". Again, you need to explain to the reader how the dynamics of the bloom were precisely affected. Whole section: What about the self-shading effect? Removing bottom ice algae should increase the amount of light available to under-ice algae. Did you consider this? Line 10: Influencing the dynamic of the bloom...through the control exerted on nutrients? How? Line 22-25: This sentence is too long, please split in two. Line 25-30 and line 28-30: You should clearly state here which study were conducted in the Arctic and which were conducted in Antarctica. Also, you need to discuss major algal groups in the assemblages sampled by the different studies. Line 28-30: Right, I agree with this. Cell rupture on melting of the samples will cause artificial release of DMSPp and transformation into DMSPd and DMS in contact with free or cell bound enzymes. But then, how can you trust the DMSPp and DMSPd measurements of Galindo et al that you use to validate the model?? Furthermore, how can you trust the DMS measurement from

Levasseur 2013? All these measurements were made by melting sea ice samples. There is at least one study in the literature that reports DMSP:chla ratios using other techniques than melting, you should refer to it. For example, Carnat et al. 2014 "Physical and biological controls on DMS,P dynamics in ice-shelf influenced fast ice during a winter-spring and a spring-summer transitions" JGR.) gives DMSPt:chla ratios for bottom ice diatom dominated communities. I think the best way to present this whole DMSP:chla issue is to draw a quick table with a few references from the literature indicating DMSP(p or t):chla ratios measured, community compositions, arctic/Antarctic, melting or other extraction technique. This table could go in the supplementary but would be a very nice addition to the paper.

Page 12: Line 5-6: It would be nice here to restate quickly which parameters then control the temporal patterns of DMS concentrations. Line 14-15-16: This is very nice to read. Line 23-24: I am not sure to understand this correctly. Is this because DMSPd removed by bacterial consumption is not available anymore to free DMSPd lyase? Is there because part of the DMSPd pool used by bacteria is converted partially in other compounds than DMS? Line 25: Ok, but which other parameters would you recommend observational studies to target?

Page 13: Line 15: How did you deal with fetch of small leads and even large leads compare to open ocean parametrizations that you use? Also, how did you deal with shear-driven and convection-driven turbulence in the sea ice zone that have been shown to have a huge impact on k (see Loose et al., 2014; A parameter model of gas exchange for the seasonal sea ice zone.) I do not especially ask you to redo all the math with a new or corrected parameterization, but at least the fact that these important parameters were neglected should be mentioned. Line 16: Again, was any met tower deployed on site? Line 29-30: Pandis and Russel 1994 are old papers, is there not more recent work on particle formation associated with DMS flux? Line 30-31: "saturation" is an odd term to use here. I think the concept could be better explained with one or two additional sentences.

Page 14: Line 16: Occurrence instead of occurrance.

Page 15: Line 1: A reference for the summertime flux for ice free waters in the Arctic is missing here. I don't know however if the journal allows ref in the conclusion. Line 2 to 4: I totally agree with this part but I believe it should be a distinct discussion section of the paper rather than a few lines in the conclusion as mentioned in the general comments. Line 13: I am a little bit surprised that nowhere in your recommendations/conclusions you are talking about the need for additional DMS,DMSP, DMSO data to validate models. I am also surprised that you do not say a word about brine drainage, although I believe it is a key missing item in your paper.

Figure 1: Same comment as in the abstract regarding the term "Flushing". Figure 3: The use of the different vertical scales for DMSPp, DMSPd, and DMS is slightly confusing. Is there a way to use similar vertical scales for the three compounds (by adding breaks or log scale?) so that they could be better compared? Figure 4: Please check the legend. In the version that I have, the dashed lines do not show up. Also, in all graphs, please use parenthesis instead of the concentration symbol in the axis titles. Figure 5: Same comment as for Figure 5, dashed lines do not show up in the legend box. Figure 8: Perhaps indicate in the figure caption the distance between the sampling site and the Resolute Airport. Other figures are fine.

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