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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 #1

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Hayashida et al propose a model study of the sulphur cycle in the Arctic landfast ice zone.

The quality of the text and figures is quite good. The authors have the interesting conclusion that sea ice sulphur cycle and ecosystems have considerable impacts on DMS production under the ice and should be considered in estimates of ocean DMS fluxes.

Whereas I believe this conclusion is potentially supported by the scientific elements of the paper, the current presentation did not convince me.

1) I'm not sure that sulphur is conserved in the model. This question is central: if

the authors suggest an enhancement of sulphur fluxes to the atmosphere, they must explain where this extra sulphur comes from. I don't see in the text or in the figures which reservoir is loosing sulphur in your model.

2) I cannot understand the mechanisms in the sensitivity experiments from the text. Section 3.2.1, they authors explain they turn the sulfur cycle off. Which terms of the equations does that represent ? What happens with DMS in these experiments ? What is the chain of mechanisms leading to the decrease in under-ice DMSPd and DMS and decreasing the air-sea fluxes ?

This is my key criticism. If the authors can at least highlight how sulphur is conserved among the different sulphur forms and, more importantly, explain more in depth the mechanisms in their sensitivity experiments, this can make a good paper.

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A few more detailed comments

- Model description is not convincing. This could probably be fixed by better explanations (not more of them).

1) Physical and ice algal components models come from a paper under review. Next time the authors should consider to attach the companion paper.

2) I doubt of sulphur conservation. I don't clearly see how sulphur can be conserved now. It should be visible from the equations. For instance, I don't see where the losses of DMSPp in sea ice go (eq A1). Make sure sulphur conservation is obvious from the few evolution equations.

3) Some physical terms (notably sea ice growth and melt for the first ocean layer) are completely absent from the equations, which is surprising, because these are leading-order terms for most other biogeochemical compounds.

4) Is there any good reason not to use a standard formulation of the DMS air-sea flux

? In your section 3.2.3, they are just proportional to DMS_water. As far as I know, this is not in line with classical air-sea flux formulations. It would not be hard to introduce solubility and pDMS in the atmosphere.

- Sensitivity experiments are difficult to understand. I have trouble to distinguish between "sea ice ecosystem" and "sea ice sulphur cycle" sensitivity experiments, because the sulphur cycle is partly controlled by sea ice algae. So the authors should clearly tell which terms are involved and, most importantly explain the mechanisms involved.

- In figures, the authors often compare model concentrations in μmol/L to observed ones. This could introduce a source of bias is the depth of the extracted core section does not match the 3 cm of the model. I would suggest to rescale observations to 3 cm if possible.

- Some aspects of the intro (links between DMS and cloud nucleation) may not be in phase with literature. I felt the role of DMS was a little bit overstated. Line 10 of page 2, the authors point DMS as the driver of arctic clouds backed with one unique citation. I was surprised by this statement.

I had a little trip in the literature, and discovered that this should probably be nuanced. Tjernström et al (ACP 2014) mention page 5 that they are indeed looking for a missing source of aerosols in the Arctic. Yet they did not found H2SO4 but rather organic molecules polymer saccharide molecules. Later on, page 2828, they explain that "This suggests a stronger possible link between marine biology, cloud prop- erties and climate than provided by DMS alone (Leck and Bigg, 2007)".

Tjernström et al. Atmospheric Chemistry and Physics, 14, 2823-2869, 2014.

I'm sure the authors are aware of these works, and I would here just suggest to better explain the the links between DMS and Arctic clouds, even if it takes a few sentences.

- The introduction felt generally a little bit "inbred", with lots of references coming from

the own group of the authors. If there is no other choice, skip this comment.

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