

# ***Interactive comment on “Amplification of global warming through pH-dependence of DMS-production simulated with a fully coupled Earth system model” by Jörg Schwinger et al.***

## **Anonymous Referee #2**

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Schwinger et al. present the results of a high emission scenario with a coupled Earth system model in which marine DMS-production has been linked to ocean acidification. A similar study (Six et al. 2013) has been already performed but without a fully coupled model system (the authors applied a two-step approach where the simulated marine emissions were used as an input for an atmospheric circulation model with aerosol chemistry). The advantage of using a fully coupled system is that feedbacks are enabled and their effects can be studied. Thus, the new work by Schwinger et al. is a necessary extension of the previous work, but unfortunately without exciting results and scientifically thrilling insights. (*But perhaps this cannot be expected...*).

In any case, I expect any scientific article to be self-contained. Unfortunately this is

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not always the case, here. For instance, model assumptions are not clearly stated. In addition, several aspects remain unclear and need to be explained better.

- in several places (page 3, page 9) the authors mention that they use the **same** (page 3, line 1), **similar** (page 3, line 30) or **very similar** (page 9, line 24) biogeochemistry model as in the previous study by Six et al. (2013). These contradictory statements are vague and not very helpful. The differences and the motivation for them need to be spelled out and the consequences for model results need to be explained.
- page 4: as stated on page 2, line 3 “DMS is a by-product of marine primary production” (strictly speaking, it is DMSP). Nevertheless, the authors couple DMS production not to *primary* but to *export* production without further explanation. Primary and export production are uncoupled in most oceanic regions. Thus, the results must be significantly different if the more correct approach (which is relatively straightforward) is applied. Instead of using the exported calcite or opal matter to distinguish high and low DMS-producing phytoplankton, the degree of silicate limitation during phytoplankton growth could be taken. A more detailed explanation for this approach is needed.
- On a more fundamental level, I am puzzled about the idea behind implementing the relationship between DMS-production and pH in the model in the first place. As far as I understand the results of the different mesocosm studies, DMSP-production is even enhanced with decreasing pH but the DMS production decreased, most likely due to bacterial decomposition. I wonder whether the model *without the prescribed relationship* is able to reproduce some of this dynamic as DMS consumption is temperature and concentration-dependent? If so, it might be problematic that certain processes governing sources and sinks of DMS are included in the model but are now “overwritten” through prescribing such a relationship.

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- page 4, line 26: the terms in the equation should be explained.
- page 5, line 21ff: why is pH-dependence only included from 2005 onwards?
- page 5, line 21ff: the experimental set-up must be explained much better. An overview table with details about all simulations would facilitate the understanding of the different model results. Also, what is the purpose of a model experiment from 1850-2200 with constant pre-industrial setting (control run)?
- page 6, line 10ff. and Fig. 1: why are the observations compared to the pre-industrial situation (control run) and not to the climatology from the historical simulation (e.g. 1975-2005)?
- page 7, line 6, line 16, ...: the authors often explain patterns in the DMS concentrations with those from primary production. However, DMS production has been coupled to *export* production in the model, thus DMS concentrations should be compared with patterns of export production. Something is inconsistent, here.
- It is unclear what insights can be deduced from the comparison of the BASE run (without pH-driven DMS changes) and the Control run (with pre-industrial setting).
- a figure showing the projected pH changes in the surface ocean would be helpful. In this context, I am wondering why the effects on DMS-fluxes are much stronger in the Southern Ocean compared to the Arctic Ocean. I would have expected a much stronger effect in the Arctic Ocean due to freshening and related higher drop in pH. Do the authors have an explanation why this is not the case?
- last sentence, page 12, line 31: is there really an agreement about “dangerous climate change”?

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