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Interactive comment on “Can we trust simple marine DMS parameterisations within complex climate models?” by P. R. Halloran et al.

Anonymous Referee #1

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The paper by Halloran et al. investigates the important question whether we can rely on parameterisations derived from empirical data fitting in future climate predictions. This topic is well studied by applying two empirical DMS parameterisations within the context of an Earth System model (ESM). The performance of these parameterisations in reproducing global mean DMS values as well as seasonal and interannual DMS variability is investigated.

The authors present a well structured and clearly written paper, with strong emphasis on the limitations of their study. Nevertheless that a similar testing of one of the empirical parameterization has already been published by Kloster et al. (BG, 2006) the present study extends the scope to DMS variations on seasonal and interannual time scales.

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From my point of view the authors made one assumption that has to be discussed in greater detail. When applying the DMS parameterisation the authors neglected chlorophyll concentrations of the phytoplankton functional type referring to diatoms. In general, it is certainly correct that diatoms are low DMS producers and could, therefore, be excluded from a first order DMS production formulation. However, this is not what Anderson et al. or Simo and Dachs did in their studies. They used the monthly composites of SeaWIFS chlorophyll concentration without any corrections. Chlorophyll concentration is, to first order, negatively correlated with the local nitrate concentration which is part of the Anderson et al. approach. Thus, to consider only non-diatom phytoplankton in the DMS parameterisation might on one hand conceal the mismatch between simulated and observed chlorophyll but on the other hand might obscure the interpretation of the results. Especially the simulated seasonality of DMS might be corrupted by this assumption. The authors must definitely add more information about the effects of disregarding diatom chlorophyll on their findings.

More information is also need about the setup of the experiment. The title suggests the application of several climate models, which is not the case. The level of complexity of the used ESM is not described in the paper (“HadGEM2-ES, at a development stage”). Why was it necessary to use an ESM for this study? E.g. are atmospheric feedback mechanisms’ via the sulphur cycle included? The author should describe and motivate the experiment setup in part 3. How long was the simulation with HadGEM2-ES? Global averages and monthly mean values should be provided with standard deviations, if possible.

The authors should reconsider the title. I agree that any empirical DMS parameterization is a simplification of our current understanding of the marine DMS cycle and certainly of the reality. As mentioned in the paper, process based DMS models that have been developed in the past and are continuously improved will hopefully enhance the DMS future predictability. To clearly differentiate between the empirical approaches, used in this study, and process based DMS models, I suggest to change the title from

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"Can we trust simple marine DMS parameterisations within complex climate models?"
into: "Can we trust empirical DMS parameterisations?"

Given a careful revision is provided I recommend this paper for publication. There are only a few minor issues in the text (see below) that have to be revised.

Minor corrections:

p1302 line 20: delete extra "of a"

p1302 and 1304: replace "Lana et al, 2010" by "Lana pers. comm." as the paper is only in preparation according to the reference list.

p1306 line 6: delete extra "and the"

Interactive comment on Biogeosciences Discuss., 7, 1295, 2010.

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