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Interactive comment on "DMS cycle in the marine ocean-atmosphere system - a global model study" *by* S. Kloster et al.

Anonymous Referee #2

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General Comments

The manuscript presents the results of a coupled ocean-atmosphere model into which the DMS cycle is incorporated. I am an oceanographer, and so am unable to comment on the atmospheric aspects of the work. I found the approach and results of the ocean-related part of the manuscript interesting and informative. With suitable revision, this manuscript should make a useful contribution to the literature. The manuscript is well-written and easy to read.

Specific Comments

(1) Introduction, p. 1069, line 13: "Fundamental gaps remain in our understanding of key processes that regulate the DMS seawater concentration (Andreae and Crutzen, 1997; Liss et al., 1997)." The DMS-field has expanded rapidly over the last few years. If possible, use more recent citations to back up this statement (which I agree with).

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(2) Introduction, p.1070. A brief description of factors involved in DMSP release into the water is given, before focusing on DMS itself. It would be useful to have a (short) linking paragraph, describing the processes involved in conversion of DMSP to DMS, and commenting on what fraction of DMSP undergoes such a conversion as opposed to other pathways. On p.1075 there is: “Kiene (1992) concluded from estuarine experiments where less than 30% of the DMSP was converted to DMS that DMS is not the major transformation product of DMSP, presumably owing to an alternative demethylation pathway”. The latter part of this sentence is too vague. There are in fact at least two major functional groups of bacteria that consume DMSP - those that cleave it to DMS and acrylic acid, and those that demethylate it.

(3) Introduction, p.1072, line 5: “Up to now, none of the global model studies include a description of the DMS cycle in the ocean. The response of the DMS emission to climate change could therefore only be assessed through changes in the sea-air exchange rate which varies with wind speed and temperature.” Not so. First, there is no reference or citation in the text to important studies in the published literature using global modes: Bopp et al. (2003) Potential impact of climate change on marine dimethyl sulfide emissions, Tellus 55B, 11-22; Gabric et al. (2004) Modeling estimates of the global emission of dimethylsulfide under enhanced greenhouse conditions, GBC 18, art no. 2014. Putting the current study in context of these important previous publications is absolutely essential, not just here, but in the manuscript in general. Secondly, existing climatologies such as Anderson et al. (2001) and Simo and Dachs (2002) include chlorophyll in their formulations for predicting DMS. If included in climate change runs in modelling studies, there would therefore be scope for some response of DMS emissions to changing chlorophyll (due to climate change). In other words, some of the existing DMS climatologies are based on descriptions of the DMS cycle in the ocean, albeit highly empirical descriptions.

(4) Introduction, p.1073. The statement of objectives at the end of the Introduction is weak. It’s not enough to simply say that the first coupled model. State clearly what the

objectives of the study are, in scientific terms.

(5) Model description, p.1075, line 6. Unless absolutely necessary, Six and Maier-Reimer (in prep) should be removed.

(6) Model description, pp.1075-1076. I find the parameterisation of DMS production based on export ratios of silicate and carbonate most interesting, presumably a proxy for production of silicate and carbonate. The description of this is much too brief in the text, considering its importance in the overall scheme of things, and that the relevant material is only available previously in an (albeit readily available) institute report. The authors need to state in the text how export of silicate and carbonate are calculated in the model, and give some detailed justification of their approach to convince the reader of its merits. I very much like what they are doing - it is certainly a novel approach, but just want to see it explained in more detail in the text.

(7) Model description, p.1078. The various scaling factors for parameterising DMS in the model ocean are absolutely critical to the work. We are told (page 1078, line 24) that these are systematically adjusted after every second model year to minimise the global deviation from the Kettle and Andreae (2000) database. Simultaneously fitting five parameters in a global general circulation model is a major undertaking. It is necessary to describe in some detail the fitting procedures used so that, in principle, anyone else wanting to replicate their methods could do so.

(8) Model description, p.1081, line 11. I do not know what is meant by “quasi-synchronously” when referring to the model coupling.

(9) Results, p.1082, introduction to section 3.1. This section inexplicably focuses on iron. Instead, what is need is to focus, albeit briefly, on the general characteristics of the ocean run. Convince the reader that the model does a good job at primary production, export flux, nutrient fields, etc. By all means mention iron, but not to the exclusion of everything else.

(10) Results, p.1083. I was impressed by Figure 1, which does indeed in general show a predicted global DMS distribution in line with expectations. There is (line 16): "The polar oceans (North Pacific, North Atlantic and Southern Ocean) feature high DMS concentration Ě". Similarly on p. 1095 (Summary and conclusions, line 22) there is: "The seasonal variation with its high DMS sea surface concentration in the high latitudes in the summer hemispheres is captured by the model". Careful inspection of Figure 1 shows that predicted DMS concentrations are low in the northern North Atlantic, which is also reflected in low predicted air-sea fluxes for this region (Figure 5). This looks like an important model-data mismatch - high values of DMS for the North Atlantic are seen in the Kettle database, and are to be expected given the prevalence of coccolithophorid blooms in this area. Due attention should be given to this, and its causes explained. Is the model, for example, underestimating carbonate export in the northern North Atlantic?

(11) It is not enough to simply show that annual average DMS concentration is realistic, as in Figure 1. I would like to see the seasonality of DMS as predicted by the model, of particular importance given the short-lived nature of DMS in the atmosphere. An additional Figure should be included, showing the seasonal global DMS distributions in the ocean.

(12) Results, pp.1084-1085. I find Figure 2 and its analysis thoroughly unconvincing. The authors repeatedly make out the validity of model predictions by stating that they are within a factor of two of the observations. Is this really so good? What is more significant to me was that there does not appear to be any trend of increasing DMS in the model predictions with increasing DMS in the data, rather just a scatter of points. I suspect that small-scale variability (patchiness in the ocean) has rendered this model-data intercomparison inconclusive. The text currently makes out that this comparison is much better than appears to be so by inspection of Figure 2.

(13) Results. The authors should be careful not to infer the Kettle and Andreae (2000) or Kettle et al. (1999) climatologies as being the best-estimate of the true DMS distri-

bution in the ocean. These climatologies are based on a series of extrapolations and interpolations and therefore subject to all sorts of error. Any one of the existing climatologies may be considered as valid as the others, each with its pros and cons (see Belviso et al., 2004).

(14) Results, section 3.1.2. I found this section somewhat unnecessary in the overall scheme of things. If any shortening of the manuscript is required, then this section could be cut.

(15) References. It is curious that only one initial is given for all names in the list of citations. Ensure that authors names in the cited list have all their relevant initials against their surnames.

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