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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud, precipitation and radiation responses to large perturbations in global dimethyl sulfide" *by* Sonya L. Fiddes et al.

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Response to Reviewer 1

We would like to thank Reviewer 1 for their comments on our manuscript. We agree that while similar simulations have been conducted, our focus has been on providing a complete end-to-end evaluation of the role of DMS in the climate system, from changes in the chemistry through to effects on clouds, precipitation and radiation. Furthermore, we have highlighted the problems remaining when trying to model and understand this complex system. We have made these discussion points clearer in both the introduction and conclusion, which has strengthened the arguments presented in this work. Furthermore, we have clarified our methods, which hopefully leaves no remaining am-

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biguity. We have addressed the specific comments below, and we hope you find our revisions satisfactory.

General Comments:

In the abstract not all results are summarized, like the comparison of the control simulation with observations, the second experiment with increased DMS emissions, and the estimate of temperature response to changes in DMS TOA radiative effect. The final sentence of the abstract is too general, the authors should try to briefly explain what kind of model study are further needed and why.
We have revised the abstract to include the model evaluation, results of both experiments and the temperature sensitivity to DMS flux. We have also made the abstract more specific.

The explanation given at the end of section 2.1.1 about nudging is not very clear. I understand that doing nudged simulation is better to compare the control model simulation with observation. On the other hand the disadvantage is to not have the full impact on meteorology when comparing the control simulation with the two sensitivity experiments. As far as I understand the model simulation includes both direct and indirect effect of aerosols on radiation, so it is not very clear the sentence at page 4, lines 15-18. Maybe also the paragraph at page 14, lines 3-5 should be included in section 2.1.1.
We have revised our explanation and justification of using nudging in our simulations in the methods section as suggested. The reviewer is correct that the model can simulate direct and indirect aerosol effects. Our use of the word 'direct' in this section may have been misleading and we have rectified this. We have also revised the text on the use of nudging in the discussion section.

The quality of the figures is not always satisfying. In particular the figures which include multiple maps are too small and it is difficult to visualize the fields. Also sometime the colors does not help the data visualization. In particular I would recommend to improve

Figures 2,3,4,5,6,8 and 10.
We have adjusted the colour scales by removing the darkest colours to increase visibility of the figures. We have also removed white space where we can and enlarged the figures. We hope these changes make our figures easier to interpret. We will also ensure the figures are readable after the typesetting process.

The section 5 is too long and somehow difficult to read. I would recommend to split in two or to shorten it by removing some of the details which are repeated from the previous sections. I would try to explain better the last part of the discussion, providing more details on what kind of experiments are needed to better understand the role of DMS future, considering the ocean acidifcation, as mentioned in the last paragraph.
We have significantly shortened this section, cutting out repetitive paragraphs summarising results previously discussed as suggested by the reviewer. We strengthened our discussion involving not only the questions we are addressing but also pointing towards what steps need to be taken for future studies. We are hesitant to include more information about the impacts of ocean acidification as it is beyond the scope of this study (and admittedly, not our area of expertise). However, we do wish to highlight potential implications of ocean acidification on climate that may not have previously been considered.

Minor Comments:

Page 3, line 1: Six et al. (2013)
Typo has been amended

Page 3, Line 21: the control simulation is not explained, only the two DMS perturbation C2 simulations are described
We have included a description of the control simulation

Page 4, Line 1: In this study, . . .
We are unsure what the reviewer meant here. We do not feel starting the paragraph with 'in this study' is appropriate.

Page 4, Line 10: did you forget CO2 from the list of GHG gases?
CO2 has now been explicitly mentioned, it is prescribed to the model as global mean concentration.

Page 6, Line 1: 50-440 hPa
Typo has been amended

Page 6, Line 17-19: WHy these three regions were chosen? A short motivation should be added. I would include the boundaries of these regions in one of the figures.
We have included motivation as to why these regions have been chosen and included the boundaries in Figure 2.

Page 6, Line 18: Pacific
Typo has been amended

Page 6, Line 29: for the first time the CTL name is used, should be introduced before
The Ctl name has been introduced a few paragraphs above.

Page 6, Line31: "without the need for an . . .", I would remove or rephrase as an ensemble experiment of free-running climate simulations is needed to better quantify the impact of DMS forcing on the temperature.
We have rephrased this sentence as suggested

Page 7, Line 9: fraction larger then 0.5 instead of >
Typo has been amended

Page 8, Line 18: outgoing TOA, LW or SW?
We have clarified this (SW)

Page 8, Line 30: (Fig 5c)? positive bias if is the difference between the model and observations, not clear from figure caption.
We have revised the figure caption for clarity

Page 8, Line 30: over regions to the north and south if the equator, .. but only in the

tropics is over 2000 mm/yr
We have revised this sentence for clarity

Page 9, Line 4: the figure with % differences is not shown, but the values of the largest % differences could be inserted in the text.
We have included the % differences in the text as suggested

Page 9, Line 17: The largest absolute differences are in the tropics and mid-latitudes over the Oceans.
Sentence amended as suggested

Page 10, Line 5: Fig6 g-h is not correct
Typo has been amended

Page 10, Line 21: the largest absolute differences are in clean terrestrial regions. C3 Which regions? Not easy to visualize in the figures. Too small.
We have removed reference to these differences due to the difficulty of visualisation.

Page 11, Line 11: the results presented here suggest a lower CCN . . .
Sentence amended as suggested

Page 11,Line 18: would title the section "Clouds and precipitation response" Title amended as suggested

Page 11, Line 21-23: this explanation about nudging should be explained also before when describing the experiments.
We have revised our explanation of nudging in the methods section.

Page 11, Line 32: (see section 3) Page 12, Line 4: remove) after fig 10a-b
Typo has been amended

Page 13, Section 4.3: Is it possible to put the uncertainties of these estimates of temperature changes per Tg of S emitted?
We have performed a moving block bootstrap to determine the uncertainty of the mean

changes in radiation, via the 10th and 90th percentile confidence intervals. This can then be translated into a change in temperature by the FAIR model and applied to the flux sensitivity calculations. Further description of how this was performed is now included in the methods and Tables 3 and 4 have been updated to include these uncertainties.

Page 14, Line 33: put ref of thomas and mahajan in parenthesis
Typo has been amended

Page 16, Line 3: can you better explain the role of coral-reef derived DMS? Why it is important?
We have included a sentence describing why coral reef derived DMS is important (the fact that it is unaccounted for in current global modelling). The role of coral reef DMS is not yet quantified and represents an opportunity for further research. Note that this section has been significantly revised as suggested in the general comments.

Page 16, Line 8: our results imply that a 25% decrease in . . . would result in an increase of 0.1C. Is it possible to put the uncertainty on this estimate?
We have now included confidence intervals for our temperature/flux sensitivity calculations, and the estimates of potential temperature change with decreasing DMS flux.

Caption of figure 3: third column is not absolute differences, as negative numbers are shown.
Caption has been amended

Caption Figure 5: c) not clear if difference model-obs or contrary while reading the description of the figure in the manuscript.
We have revised the caption for clarity

Caption Figure 7: blue lines show the SO (Southern Ocean) mean, red the australian (aus). The short name aus is used only here and not in the manuscript.
We have only shortened Australia to Aus. for clarity in this figure.

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