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

Anonymous Referee #1

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In this paper the authors presented the results from model simulations with the aim of quantifying the effects of changing DMS natural emissions on cloud, precipitation and radiation. They compared three global chemistry-climate coupled model simulations, a control run with the most recent climatology of DMS water concentrations, a sensitivity by removing all marine DMS, and a second sensitivity by imposing the monthly zonal maximum DMS water concentrations at each latitude. Despite this study does not present novel concepts, as similar experiments were recently performed by other studies, this paper address relevant scientific questions, which are not yet fully answered. The presentation and the analysis of the results is generally well written and clear, but it can be improved to clarify some of the methods and the analysis. I would suggest acceptance of the paper after taking into consideration the following comments.

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Discussion paper

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.

- 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.

- 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.

- 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.

Minor comments:

Page 3, line 1: Six et al. (2013)

Page 3, Line 21: the control simulation is not explained, only the two DMS perturbation simulations are described.

Page 4, Line 1: In this study, . . .

Page 4, Line 10: did you forget CO2 from the list of GHG gases?

Page 6, Line 1: 50-440 hPa

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.

Page 6, Line 18: Pacific

Page 6, Line 29: for the first time the CTL name is used, should be introduced before

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.

Page 7, Line 9: fraction larger then 0.5 instead of >

PAge 8, Line 18: outgoing TOA, LW or SW?

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

Page 8, Line 30: over regions to the north and south if the equator, .. but only in the tropics is over 2000 mm/yr

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

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

Page 10, Line 5: Fig6 g-h is not correct

Page 10, Line 21: the largest absolute differences are in clean terrestrial regions.

Which regions? Not easy to visualize in the figures. Too small.

Page 11, Line 11: the results presented here suggest a lower CCN . . .

Page 11,Line 18: would title the section “Clouds and precipitation response”

Page 11, Line 21-23: this explanation about nudging should be explained also before when describing the experiments.

Page 11, Line 32: (see section 3)

Page 12, Line 4: remove) after fig 10a-b

Page 13, Section 4.3: Is it possible to put the uncertainties of these estimates of temperature changes per Tg of S emitted?

Page 14, Line 33: put ref of thomas and mahajan in parenthesis

Page 16, Line 3: can you better explain the role of coral-reef derived DMS? Why it is important?

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?

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

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

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.

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