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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Coral reef-derived dimethyl sulfide and the climatic impact of the loss of coral reefs" *by* Sonya L. Fiddes et al.

Anonymous Referee #2

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Summary: The paper presents a first assessment to my knowledge of the impact of coral reef-derived DMS (DMS being an important precursor to sulfur dioxide and subsequent sulfate aerosol formation) on regional climate over the Maritime Continent and Australian regions. The authors implement a new source of DMS derived from coral reefs, in addition to the existing, widely-used DMS climatology of Lana et al. (2011), in the ACCESS-UKCA model and attempt to answer the question of will the loss of coral reef ecosystems have a significant impact on regional climate via associated changes in aerosol-radiation-cloud interactions. The authors conduct both nudged and free-running climate simulations (with and without the coral DMS source applied) to investigate this question and present an in-depth evaluation of the impact of removing the coral source on atmospheric DMS, sulfur dioxide, aerosol properties as well as

top-of-atmosphere and surface radiation and cloud properties. Impacts on DMS, sulfur dioxide and aerosol number concentrations in the nucleation and Aitken size modes are found to be small (all <10%) but statistically significant in some seasons. Impacts on aerosol forcing relevant variables such as AOD and cloud droplet number concentrations are found not to be statistically significant in most instances and changes in the TOA and surface radiative fluxes are very small. The authors subsequently conclude that DMS derived from corals has a very small climatic impact. Overall, I find this paper well-written, well-structured and easy to follow and the figures are all of a very high quality and clear. It is a relevant and interesting topic given the high potential for increasing damage and loss of coral ecosystems and this study on the impacts on aerosols and subsequent climate interactions is novel. Despite the likely negligible impact of this source on aerosol-climate interactions in coral reef regions it is still important to publish such results. I would recommend publication in ACP subject to a few clarifications and minor revisions.

General comments:

My main issue with this study is the use of both nudged and free-running simulations. Nudged simulations are most useful when conducting an evaluation of model against observed variables for a given time and place and are also useful to isolate the aerosol forcing signal in shorter runs than would otherwise be possible in free-running experiments. However, in the latter double-call radiation diagnostics are used to determine both the direct and indirect aerosol effects cleanly. Otherwise, the nudging suppresses the rapid adjustments due to the aerosol perturbation – this is clearly seen for instance in Figure 10. I would therefore argue that nudged experiments aren't appropriate for this current study and see a lot more value in the 7-member free-running ensemble. The authors do highlight how the difference in nudged and free-running experiments highlights the dynamical feedbacks evident in the latter but the analysis carried out on the vertical velocities and water vapour responses lead to the same conclusion so I do question the usefulness of the nudged simulations here and would urge the authors to

do the same.

Specific comments:

DMS sources from coral reefs are reported to be taken from the UNEP-WCMC climatology. Given the relevance of this data source for this study a more detailed description/summary of this dataset is required.

I think it could be useful to break down the annual mean flux in DMS from corals into its seasonal contributions, given much of the analysis of the response is broken down into the seasonal response. Is there a correlation between the seasonality in the source and the response or are there other factors involved?

P7 L170-172 Given that the Woodhouse et al. 2019 study is unsubmitted a brief discussion of the physical mechanism behind the increase in SO2 and in general SO2 sensitivity in this region is required (see also response in accumulation and coarse model aerosol on Section 3.3). Also, what is the role of anthropogenic sources in this region? This is briefly alluded to later (P15 L250) but not discussed in any detail.

Similarly, can the authors comment on the uncertainties in the response due to other aerosol sources in the region?

What is the reason behind SON showing a larger response in CCN than other seasons? No physical mechanism or justification is currently given. Table 1 seems to suggest that MAM also has statistically significant changes of a similar magnitude. Please provide justifiable reasoning behind selecting SON over other seasons for the subsequent analysis.

Technical corrections:

P2 L38 has summarized reports of –> reports

P4 L71 is upon –> on

P4 L82 parameterization –> scheme or model

P4 L86 DMSw –> I don't think this has yet been defined

P5 L108 in this thesis –> in this work or study

P5 L110-112 Is all this really just saying that the 50nM perturbation represents a maximum possible contribution from coral reefs?

P5 L116 hear on –> hereafter

P5 L116 refereed –> referred

P7 L145 free –> free-running

P7 L146 there is no Figure 1c?

P11 L191 removed –> remote

P14 L246 An increase in AOD to the west of PNG is referred to in the text, however I can not see any such increase in fig 8b?

P21 L363 How can the change in OSW be attributed to aerosol if the aerosol changes in themselves are not significant?

P22 L397 oversimplification –> it would probably be more accurate to highlight certain missing interactions / processes here, such as aerosols interactions in the convective plume, rather than just a general oversimplification. Aerosol process representation in models are increasingly complex but this work correctly highlights certain shortcomings of relevance to tropical aerosol-climate interactions.

P23 L401 I'm not sure I agree with all the limitations specified here. Of course resolution is always a limiting factor when it comes to resolving sub-grid scale processes and leads to the need for convective parameterization but there are still tools for pulling out the aerosol signal even at these resolutions and timescales to circumvent the averaging and process extraction issues noted by the authors such as using double-call radiation diagnostics to diagnose the direct and indirect radiative effects as well as using cloud

simulators to determine the effects on clouds.

P23 405-408 As stated above in my main remarks I don't agree with this statement and find a very limited utility of the nudged simulations in this study. The direct responses can be separated from indirect dynamical response through use of double call diagnostics.

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