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Comment on acp-2021-507

Anonymous Referee #2

Referee comment on "The contribution of coral-reef-derived dimethyl sulfide to aerosol burden over the Great Barrier Reef: a modelling study" by Sonya L. Fiddes et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-507-RC2, 2021

Review of Fiddes et al, The contribution of coral reef-derived dimethyl sulfide to aerosol burden over the Great Barrier Reef: a modelling study

This study aims to evaluate the effect of the DMS emitted by coral reefs on the local aerosol burden. The WRF-Chem model has been chosen to conduct the experiment, and novel observation data have been introduced to adjust and evaluate the simulated results. Based on the simulated results, the authors conclude that the DMS from the Great Barrier Reef has little impact on the aerosol burden.

Fiddes et al. (2021) found that there is little impact of the coral reef on aerosol burden due to the relatively small size of the coral reef. However, the Great Barrier Reef has been found to contribute to nucleation and Aitken mode aerosol. Therefore, it is reasonable and necessary for this study to narrow down the research domain to the Great Barrier Reef region to investigate the coral reef's impact by choosing a regional climate model with a higher spatial and temporal resolution compared to the GCM used in the previous study. In my opinion, the motivation of this study is intrinsic and meets the ACP criterion.

The authors use the novel measured DMS_w and DMS_f data to adjust WRF-CHEM DMS_w emission is a significant contribution to the modeling community. After the adjustment, the simulated DMS_a from L11S and L11SCR can agree well with RVI and AIRBOX measurements. Also, the method used in this study to introduce the coral reef's perturbation on DMS emission is reasonable. To investigate the impact of the coral reef's emission, the authors subsequently focus on the simulated DMS_a and SO_4 differences between the L11S and L11SCR simulations, as well as the simulated aerosol size difference. In general, the method and the results are clear.

I think an appropriately revised version of the paper could readily be suitable for publication in ACP. I have following suggestions for improvement.

Major comments

1, More model validations are needed in this study. The authors used novel measured DMS_a, SO4, and BC to validate the WRF-CHEM simulations, but all the data points are over the ocean left the Australian continent unaccounted. As mentioned in this study, the Australian coast emissions significantly contribute to the aerosol burden over the sea; I think some comparisons between the simulated and in-situ measured aerosol concentration overland could be helpful to evaluate the overall model performance. As shown in figure 4, the L11S simulation underestimates the BC compared to observation. It could be due to the sea breeze issue suggested by the authors, but it is also possibly caused by inappropriate emissions over the land (anthropogenic and natural sources). As shown in figure 3, the L11S and L11SCR simulations captured the DMSa signal well but relatively performed less well regarding SO_4 concentration. It implies that the DMS_a is not the only contributor to the SO4 over the ocean, and the overland emission could be an interesting one to investigate. Significantly, the background SO4 concentration over the sea could impact the sensitivity of aerosol burden to the coral reef emitted DMS.

2, More sensitivity tests are needed to evaluate the impact of the coral reef-derived emission on aerosols. Fiddes et al. (2021) suggested that the effect of coral reef-derived DMS depends on the background aerosol loading. Therefore, it is interesting to test to what extent the reduction of the anthropogenic/natural (i.e., power plant, biomass burning, and sea salt) emissions could increase the impact of the coral reef-derived DMS? Adding these sensitivity tests enriches the importance of this study in the context of global energy and biomass burning trends.

Minor comments

Line 23, missed a citation?

Line 26 to 29, the authors describe the DMS's in the radiative forcing, including direct and indirect effects. Could the authors give more detailed data two distinguish the two effects?

Line 30, what is the primary sources of DMS in the ocean?

Line 229 to 231, do the authors check the simulated and observed BC concentration over land before concluding?

Line 266 to 267, please be more specified about the "internal model variability"

Line 350, does simulation become better when using hourly nudging? There should be some reanalysis meteorology data available with higher temporal resolution.

Line 384, Could authors specify what size bins in WRF-CHEM refer to the Aitken mode here?