

## ***Interactive comment on* “Reviews and syntheses: Marine biogenic aerosols and the ecophysiology of coral reefs” by Rebecca Jackson et al.**

### **Anonymous Referee #2**

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#### General comments

Jackson and co-authors have produced a welcome and comprehensive review of an exciting and growing topic that bridges the gap between coral reef ecology and DMS biogeochemistry. They skilfully bring together the current threats faced by the coral reefs with a detailed overview of the role of DMS and other reduced S compounds in coral physiology, whilst placing this in the context of the intricacies of the impacts on atmospheric processes and climate regulation.

To my knowledge, a similar review has not before been published. This topic has been growing in strength over the last 10 years or so, and as such a review of this type is timely and appropriate. The manuscript is written in an accessible and easy-to-read style, with few technical or editorial issues. This paper will be of great interest to the

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DMS community – both those with an interest in ocean biogeochemistry, as well as atmospheric chemists and modellers. It should also bring to prominence this topic amongst the coral scientific community, as until now the role of DMS/P in coral physiology, survival and its influence on the atmosphere/climate has perhaps not received the attention it deserves.

I can recommend this paper for publication provided the authors adequately address the issues which I have outlined below. In particular, I strongly recommend that the authors address my concerns with regards to sections 7 and 8 – both are currently somewhat weak and need a change in emphasis to make coral DMS production the focus of the discussion. Furthermore, both sections would benefit from being more forward-looking and include some specific recommendations for future research in the context of coral DMS production (please see further comments below).

Title: The title in its current form doesn't do a good job of describing the paper and should include some mention of DMS as this is really the main point of the paper (and more likely to get found in a Google Scholar search for 'DMS and corals!'). Perhaps something along the lines of the following: Marine dimethylsulfide (DMS) emissions and the ecophysiology of coral reefs. Dimethylsulfide (DMS) emissions from coral reefs in the face of natural and climate-induced stress.

Section 7 Coral reef monitoring: In its current form, the aim of section 7 is unclear and seems slightly weak. The authors give a reasonable overview of coral reef monitoring programs but there is little contextualisation in terms of DMS production. Some statements are unclear in their meaning e.g. L 20-22 from "Additionally, field surveys..." What is meant by "the substance"? And it is not clear what is true of DMS/P in coral reef waters. The authors should revisit this section and reword to make clearer. At the moment, it feels a little like they are struggling to illustrate the relevance to DMS etc. It's also unclear what "DHW of °C-weeks" means. The final sentence of this section (L14 – 16) hints at where the authors could focus this part of the paper i.e. by providing forward-looking recommendations of future research to improve our understanding

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of “the biogeochemical processes occurring in coral reefs and the ways we can effectively ensure their preservation”. This is the emphasis from which they could begin this section to make it much more relevant to the review as a whole. Therefore, I recommend a re-think and re-write of this section, starting with the aim above, then drawing on the past and ongoing monitoring programs to develop recommendations for future research.

Section 8 Mitigation strategies: Similarly to section 7, the current emphasis does not seem quite relevant enough to the review as a whole. It simply serves to summarise the current literature on geoengineering etc. to protect coral reefs. Again, I believe a restructuring of this section is necessary. The authors end the section by mentioning DMS flux – I would recommend bringing this part of the discussion to the beginning of the section and then develop the more general discussion from the standpoint of DMS. Some specific, forward-looking research recommendations would also be welcomed, and this is necessary to turn what is currently a solid literature review into something more innovative.

#### Specific comments

Pg 4, L2: The authors refer to corals as being ‘amongst the largest sources of natural sulfur’ but this is incorrect. As they later explain (Pg 11) the total sea-air flux of DMS is 17.6 – 34.4 Tg S/y compared to only 0.02 – 0.08 Tg S/y from tropical coral reefs. A rewording of the sentence is required.

Pg 4, L24-28: The sentence beginning ‘Particulate DMSP. . .’ would benefit from some rewording. It is currently a long sentence describing multiple phenomena and reads confusingly. The reference to grazing seems out of context here. I suggest something along these lines: ‘DMSP in the form of intracellular, or particulate, DMSP (DMSPp) may be released to the surrounding reef waters via zooxanthellae expulsion at a rate of 0.2 – 0.4% Symbiodinium cells day<sup>-1</sup>, in response to elevated irradiance or temperature (ref). Furthermore, DMS or DMSO may be released in coral mucous and Symbiodinium

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exudates’.

Pg 6 L23-24: This sentence isn’t clear in its meaning. Please double check. Perhaps ‘...when ROS levels are..’ could be replaced with ‘...by reducing ROS levels to...’.

Pg 7, L9 – 11: This is the first mention of soft corals with no previous context, and currently only offers brief information. Either add more information here, or alternatively omit soft corals from this part of the discussion, because it doesn’t currently serve a great purpose.

Pg 11, 11 – 19: The findings of Six et al. (2013) (Nature CC 3, 975) and Schwinger et al. (2017) (Biogeosciences 14, 3633) should be incorporated into this part of the discussion. Also the final sentence of this paragraph is tantalising but vague and would benefit from being expanded. The relevance of carbonate chemistry and buffering capacity is currently very unclear.

Pg 12: The sub heading 4.3 Complexity of the DMS cycle doesn’t quite fit. Perhaps Complexity of the climate response to DMS, or something similar.

Pg 14, L24: It may be better to say ‘prevailing meteorological conditions’

Pg 16, L2: It is an overstatement to say ‘If coral reefs significantly affect our climate...’ perhaps say ‘If coral reefs significantly affect local atmospheric conditions...’ or similar.

Technical corrections

Pg 2 L10: move comma to come after ‘...emerging topic of research,’

Pg 6, L15: remove comma after ‘approached’

Throughout the paper: The authors refer to “the radiative balance”. It would be better to refer to “the Earth’s radiative balance”.

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