Reviewer 1

REVIEW FOR: 'INTERACTIONS BETWEEN TRADE-WIND CLOUDS AND LOCAL FORCINGS OVER THE GREAT BARRIER REEF: A CASE STUDY USING CONVECTION-PERMITTING SIMULATIONS', ZHAO ET AL.

Firstly, I would like to declare up front that it's Sonya Fiddes here doing this review, which, as you read my comments, will become clear why I have done this. This paper presents a nice exploration of how trade-wind clouds over the Great Barrier Reef are influenced by the local mountain range, sea surface temperatures and aerosol sources. It is also one of very few atmospheric modelling studies focused over the Great Barrier Reef. Its results show that the topographical setting is the most important contributor to shallow cloud and precipitation with local SSTs and aerosol having a lesser impact. As efforts continue to protect the Great Barrier Reef, a better knowledge of how clouds interact with their local environment is essential. For this review, I have a few main comments, and then further general comments on the results. I think if these can be addressed (which I'm certain they can be), then this paper is a valuable contribution to the literature.

SPECIFIC COMMENTS:

1. More information about the WRF model set-up and how these choices may be impacting your results would be beneficial, particularly with respect to the microphysical scheme and how the aerosols are allowed to interact and influence cloud formation/radiation. For example, is the microphysics scheme a double moment scheme (or single moment), and how are aerosol allowed to interact with cloud and radiation (I see that you have mentioned how the aerosol are activated which is part of the indirect effects, but can you be more explicit here? And what about direct effects?)? Why have you decided to use a scheme that only represents 'water friendly' and 'ice friendly' particles, and not a scheme that represents aerosol sources more comprehensively? It would be beneficial to the reader to gain a more comprehensive understanding of the model being used and its limitations.

   As suggested, we have included following information in the revised manuscript (Line 188-219).

   "For the microphysical parameterization, the Thompson Aerosol Aware microphysics scheme (Thompson and Eidhammer, 2014), a bulk scheme that treats five separate water species: cloud water, cloud ice, rain, snow, and a hybrid graupel-hail category, is used. This scheme utilizes double-moment prediction (mass and concentration) of cloud water, cloud ice, and rain mixed with single-moment prediction (mass only) of snow and graupel. Updated from the previous version (Thompson, 2008), this version of microphysics scheme incorporates the activation of aerosols as cloud condensation (CCN) and ice nuclei (IN), and therefore, explicitly predicts the number concentration of two aerosol variables. Rather than assuming all model horizontal grid points have the same vertical profiles of CCN and IN aerosols, this study uses an auxiliary aerosol climatology as the aerosol background condition placed into WRF model for every grid points regardless of cloudiness. The aerosol input data are derived from multiyear (2001-2007) global model simulations (Calarco et al. 2010) in which particles and their precursors are emitted by natural and anthropogenic sources. Multiple species of aerosols, including sulfates, sea salts, organic carbon, dust and black carbon, are explicitly modelled with multiple size bins by the Goddard Chemistry Aerosol Radiation and Transport model with 0.5° longitude by 1.25° latitude spacing. The microphysical scheme then transformed these data into simplified aerosol treatment by accumulating dust mass larger than 0.5 μm into the IN (ice-friendly) mode and combining all other species besides black carbon as an internally mixed CCN (water-friendly) mode. To get the final number concentrations from mass mixing ratio data, it is assuming lognormal distributions with characteristic diameters and geometric standard deviations taken from Chin et al. (2002). Samples of the climatological aerosol dataset can be found in Thompson and Eidhammer (2014, Fig 1). Note that black carbon is ignored for this version but might be incorporated into future versions

(Thompson and Eidhammer, 2014). However, it is not expected that the absence of black carbon aerosol will have a significant effect for pristine maritime trade cumulus clouds. Rather than considering multiple aerosol categories, the Thompson Aerosol Aware microphysics scheme simply refers to the hygroscopic aerosol (a combination of sulfates, sea salts, and organic carbon) as a "water friendly" aerosol and the nonhygroscopic ice-nucleating aerosol (primarily considered to be dust) as "ice friendly". The activation of aerosols as CCN and IN is determined by a lookup table that employs the simulated temperature, vertical velocity, number of available aerosols, and hygroscopicity parameter applied in Köhler activation theory. The activation of aerosols as droplets is done at cloud base as well as anywhere inside a cloud where the lookup table value is greater than the existing droplet number concentration (Thompson and Eidhammer, 2014). Note that the aerosols used by the microphysics scheme to activate water droplets and ice crystals do not scatter or absorb radiation directly. The aerosol's scattering–absorption–emission of direct radiation is only considered within the RRTMG radiation scheme by the typical background amounts of gases and aerosols in this study (Thompson and Eidhammer, 2014)."

We have also added the following relevant discussion to the conclusion (Line 731-747).

"It should be noted that a limitation of this present study is the choice of model set-up in relation to the aerosol representation. In this work, the microphysics scheme simply treats aerosol categories into 'water friendly' and 'ice friendly'. While using a more comprehensive representation of aerosol sources in the model is desirable for a more complete understanding of the complex interactions between aerosols and atmospheric processes (Ghan et al., 2012; Wang et al., 2013), these aerosol-resolving models commonly come at a significant computational cost and are simply unaffordable at a cloud-resolving resolution over a large domain. The primary aim of our study is to better understand the first-order impacts of local forcings on the clouds and precipitation over the GBR, which is the first step towards a more comprehensive investigation of aerosol-cloud-climate interactions. This research requires a large domain at reasonably high resolution to properly capture the complex interactions between the large-scale meteorology and local forcings, which are critical for trade-wind cloud formation (e.g. Vogel et al., 2020; Bretherton and Blossey, 2017). Although far from perfect, the use of the (simplified) aerosol-aware Thompson and Eidhammer (2014) scheme in a convection-permitting configuration is a reasonable middle ground to address these two critical needs. Note that a combination of sulfates, sea salts, and organic matter is found to represent a significant fraction of known CCN and are found in abundance in clouds worldwide (Thompson and Eidhammer, 2014). Therefore, while it would be an interesting (and important) topic for a different project, a precise understanding of aerosol sources, amounts and composition is beyond the scope of the present study."

2. As mentioned, there are very few modelling studies that have the Great Barrier Reefs as their focus, especially with respect to aerosol/cloud modelling. However, you have not mentioned two of the most recent papers on this very topic (see below). I acknowledge that I have some conflict in this given the second paper is my own, however, the similarities between the paper presented for review and this prior paper I think should speak for themselves (eg. use of the WRF model, testing impact of aerosol, etc.). I would request that the authors take some time to consider how the results of their work either supports or disagrees with these prior papers to show how they are advancing a relatively small body of knowledge.

    - Jackson RL, Woodhouse MT, Gabric AJ, Cropp RA, Swan HB, Deschaseaux ESM and Trounce H (2022) Modelling the influence of coral-reef-derived dimethylsulfide on the atmosphere of the Great Barrier Reef, Australia. *Mar. Sci.*9:910423. doi: 10.3389/fmars.2022.910423
    - Fiddes, S. L., Woodhouse, M. T., Utembe, S., Schofield, R., Alexander, S. P., Alroe, J., Chambers, S. D., Chen, Z., Cravigan, L., Dunne, E., Humphries, R. S., Johnson, G., Keywood, M. D., Lane, T. P., Miljevic, B., Omori, Y., Protat, A., Ristovski, Z., Selleck, P., … Williams, A. G. (2022). The contribution of coral-reef-derived dimethyl sulfide to aerosol burden over the Great Barrier Reef: a modelling study. Atmospheric Chemistry and Physics, 22(4), 2419–2445. https://doi.org/10.5194/acp-22-2419-2022

Thank you for these references. We have added relevant discussion and cited them properly in the revised manuscript (Line 63-70).

"While a very small natural contribution to the cloud condensation nuclei (CCN) population from coral derived DMS was noted over the GBR by either global (Fiddes et al., 2021) or regional scale (Fiddes et al., 2022; Jackson et al., 2022) simulation studies, broader impacts of aerosol on cloud and precipitation process over the GBR remains unquantified. For example, anthropogenic emissions are found to be important over the GBR in regard to modulating influence from coral-reef-derived aerosol on local aerosol burdens. In addition, a higher temporal resolution analysis including the diurnal cycle of these low-level clouds may be critical in understanding their effect on the radiation budget (Fiddes et al., 2022, Fiddes, 2020)."

3. I think this paper would benefit from some more quantitative analysis, e.g. some statistics. There are some number values presented but reporting values more consistently (including %) would be good to help the reader gain an idea of how much these experiments are actually changing the fields of interest. I know that presenting significance values for case studies is difficult, so it would also be good to include in some of your discussion how confident you are in these changes given the model's capabilities/limitations. (e.g. can you be sure it is not just 'noise'?).

We have added more statistics in consistency to better support the analysis presented. We have also included a note as follows in the conclusion to comment on the level of confidence in the results/findings presented in this study (Line 761-764).

"In addition, it is acknowledged that presenting statistical significance values is inherently challenging in case studies due to the unique nature of each case and limited samples. However, it is believed that the careful consideration of the model's set-up, combined with a thorough comparative analysis, allows this study to present the findings with a reasoned level of confidence."

4. I think your conclusions need to include a bit more of a rounded discussion rather than just a summary of the results. E.g. what limitations does your model have in how it can represent the processes you are exploring (eg. what resolution are your SSTs/orography – would higher res change your results? Does the very simplified aerosol representation impact your results?). Does this work support or disagree with prior work? What would these results suggest for future work? Does it have implications for the RRAP work that is funding this paper? Can you comment on other times of year, and other cloud regimes? Do you think the results would be similar in these instances?

We agree with the reviewer's suggestion and have expanded conclusions to include more discussions regarding the limitations, implications, and future work.

TECHNICAL COMMENTS

Line 49: The Fiddes et al. (2021) paper actually shows that there is 'no robust evidence that coral-reef-derived DMS influences global and regional climate'. I would suggest re-reading this paper also, as there may be some interesting differences or similarities with respect to cloud response to aerosol.

We intended to convey that this paper represents one of the scholarly works examining the role of coral-reef-derived DMS in contributing to global aerosol levels. The relevant sentence in the manuscript has now been updated for greater clarity and accuracy.

Following your suggestion, we have re-read this paper and added further relevant detail (Line 63-70), please refer to response to main comment 2.

Line 60: Fiddes et al. (2022) did provide a higher temporal resolution study to Fiddes et al. (2021). Analysis of diurnal cycles can also be found here: https://minerva-access.unimelb.edu.au/items/f7b061c0-0bf8-5574-aaa2-e9f83f5fe854

> Thank you for the references. The sentence has now been revised to properly reference these literatures (Line 63-70).

Figure 3: Is there a way you can simplify this plot – it took me a moment to figure out what was going on. E.g. can you combine say a & b and c & d?

> We have now revised Figure 3, with Figure 3c showing both upwind and downwind subdomains.

Line 146: Perhaps you can also introduce the up/down wind domains here as well to help make sense of Figure 3.

> As mentioned above, we have revised Figure 3 to be more concise. We believe that introducing the upwind and downwind subdomains later, alongside the detailed analysis, would be more beneficial for clarity and coherence.

Line 160: What are ‘history intervals’?

> ‘History intervals’ is a WRF setting for controlling the time intervals between each WRF output file. In this study, we set different history intervals for each domain, which are 6 hours for d01, 3 hours for d02, and 1 hour for d03.

Line 166: Can you describe microphysics scheme in more detail (see main comment 1).

> As mentioned earlier, we have added a more detailed description of both microphysics scheme and aerosol climatology used in the study.

Line 168: Do you mean all model grid points with cloud in them?

> The aerosol climatology data is applied for every model grid points regardless of cloudiness. A note has been added for greater clarity.

Line 169: Can you describe the aerosol climatology a bit more? What resolution is it based on, what model derived them? What aerosol does it include (ie. sea salt, sulfates?), how does it include anthropogenic emissions (Fiddes et al. 2022 showed these to be dominant for the GBR region). Do you know if it is realistic for the GBR? Models are becoming increasingly aerosol aware these days, so answering these questions is becoming more and more important, especially if you are considering aerosol-cloud interaction!

> We have now included more information of the aerosol climatology used in this study (Line 197-209) as follows.
>
> “The aerosol input data are derived from multiyear (2001-2007) global model simulations (Calarco et al. 2010) in which particles and their precursors are emitted by natural and anthropogenic sources. Multiple species of aerosols, including sulfates, sea salts, organic carbon, dust and black carbon, are explicitly modelled with multiple size bins by the Goddard Chemistry Aerosol Radiation and Transport model with 0.5° longitude by 1.25° latitude spacing. The microphysical scheme then transformed these data into simplified aerosol treatment by accumulating dust mass larger than 0.5 μm into the IN (ice-friendly) mode and combining all other species besides black carbon as an internally mixed CCN (water-friendly) mode. To get the final number concentrations from mass mixing ratio data, it is assuming lognormal distributions with characteristic diameters and geometric standard deviations taken from Chin et al. (2002). Samples of the climatological aerosol dataset can be found in Thompson and Eidhammer (2014, Fig 1). Note that black carbon

> is ignored for this version but might be incorporated into future versions (Thompson and Eidhammer, 2014). However, it is not expected that the absence of black carbon aerosol will have a significant effect for pristine maritime trade cumulus clouds."

Line 170: What scheme are you referring to? The microphysics?

> Yes, we meant to say microphysics scheme. The sentence has now been revised to be more specific (Line 193).

Line 173: I've never really heard of aerosol being categorised as 'water friendly' and 'ice friendly'. Some further justification/explanation of this might be good, i.e. what types of aerosol do these categories actually represent?

> We have now added more detailed explanation of the microphysical scheme as well as aerosol climatology used in this study, please refer to response to main comment 1.

Line 194: Can you describe the SST data sets more? What are you using for the control? Is it the HadISST data set? What have you used to create the climatology? Why did you choose a 1˚C perturbation and not more or less? Also with your supplementary Figure (S2), I think reversing plot c would make more sense as you mention that the climatology is cooler than the control, but that plot would make you think it was warmer if you didn't read the caption properly (also can you describe plot c a bit more clearly in caption – it's not so much an anomaly distribution as just an anomaly plot).

> The manuscript has now been revised to include the following description of the SST climatology used in the model (Line 251-258).
>
> "The monthly SST climatology applied in the control simulation is derived from ERA5 for the period of 1998-2018. This climatology integrates SST data from both HadISST2 (before September 2007) and OSTIA (September 2007 onwards) datasets. It is important to note that, unlike the SST alteration in the SST-cooler experiment, part of the ocean area in the SST-climatology is warmer than the actual SST (Figure S1). Nevertheless, the sea surface temperature over the majority of the GBR is reduced in the SST-climatology experiment. The selection of a 1 ˚C perturbation is based on the findings presented in Zhao et al. (2021), where a typical 1 ˚C positive SST anomaly is noted during the coral bleaching season over the GBR."
>
> Following your suggestion, Figure S2 (new Figure S1) has now been revised, and the caption has been updated.

Line 208: It's great that you are evaluating the model against obs, but unfortunately, none of the obs you are using to evaluate the model are independent, i.e I am fairly certain that all of the obs you have used would have been ingested into the re-analysis. This is certainly worth mentioning.

> While a great wealth of thermodynamic observations (including radiosonde soundings and wind observations) are being assimilated into the reanalysis, observations of clouds and precipitation are not. We note that some of the observations sensitive to cloud and precipitation (such as microwave radiances) are starting to be assimilated, but direct cloud amount is not, which is why all simulations need a spin up time.
>
> https://journals.ametsoc.org/view/journals/clim/29/6/jcli-d-15-0637.1.xml
>
> https://www.ecmwf.int/sites/default/files/elibrary/2017/17718-assimilating-observations-sensitive-cloud-and-precipitation.pdf.
>
> The manuscript has been revised to include a comment on this issue (Line 281-288) as follows.

> "It should be noting that some of thermodynamic observations (e.g. radiosonde soundings and wind observations) are being assimilated into the reanalysis, however, the observations of cloud and precipitation are not. It is expected that the initial hours will exhibit strong agreement of thermodynamic variables with the ERA5 dataset, but over the course of the 36 hours, the simulations will be sensitive to the parameterisations and settings selected. Therefore, the evaluation of model output is designed to examine the middle and last few hours of the simulation (Figure 4, and 5) against both ERA5 thermodynamics and independent cloud and precipitation observations (e.g. Himawari and weather station, Figure 2 and 6)."

Line 211: I'm not sure that comparing a model that is being driven by ERA5 to ERA5 is worthwhile – a good sanity check yes, but not really necessary to include as a result. If there are BoM pressure/wind obs, that would be better, but that faces the same problem as the other obs in that they are ingested into ERA5, so not really independent either.

> While our simulations are initiated by ERA5, and forced at the boundaries by ERA5, they are freely run for 36 hours. It is expected that the initial hours will exhibit strong agreement of thermodynamic variables with the ERA5 dataset, but over the course of the 36 hours, the simulations will be sensitive to the parameterisations and settings selected. For example, different microphysics schemes can lead to different cloud structures; different boundary layer schemes can lead to different boundary layer heights. As such, it is necessary to evaluate the CONTROL simulation to ensure the simulation configuration reasonably produces the observed meteorology. This is a common practice, even if it is largely a sanity check.
>
> Also, as mentioned earlier, the observations of cloud and precipitation are not directly assimilated into the reanalysis. Indeed, the clouds and precipitation within ERA5 are from numerical simulations, rather than assimilated observations, which is why they are not used for evaluation purposes. As such our evaluation examines the middle and last few hours of the simulation (Figure 4, and 5) against both ERA5 thermodynamics and independent cloud and precipitation observations (e.g. Himawari and weather station, Figure 2 and 6).
>
> A note has now been added to comment this issue as mentioned above (Line 281-288) and as follows (Line 318-321).
>
> "It needs to note that this is largely expected as the simulations are initialized with ERA5 reanalysis in this study. However, a good agreement is also found towards the end of the simulation (1200UTC 30 April), suggesting that the simulation in this study is doing a good job regarding representation of the synoptic condition."

Line 250: I think you mean Figure 6?

> Yes, we meant to say Figure 6. Thank you for pointing this out - it has now been corrected.

Line 259: I think you mean green lines?

> This was a typo. In the revised manuscript, Figure 6 has now been updated with different colormap. The caption has been revised accordingly.

Line 258: I think if you want to show the correlation of precip to altitude, a better plot might be a scatter plot with elevation on y-axis, and precip on x-axis? You could colour it by east/west aspect or something like that to take into account up/down wind orientation.

> We agree that a scatter plot would be able to illustrate the correlation between precipitation and altitude more clearly. However, our comment on the correlation is only intended to highlight the model skill, which helps motivate the subsequent experiments. For this reason, we have decided that presenting the suggested plot is not essential within the scope of this study.

Figure 5: Can you expand the figure caption? Eg. what are the dotted/solid lines?

> The caption for Figure 5 has now been updated with more information added.

Line 284: Can you please explain what you mean here? ‘In this study, CF is defined as the proportion of total grid points in the target domain that are classified as cloudy grids, denoted by the binary number 1, for each model level ‘. Do you mean that the grid point has to have 100% CF to be included?

> In our model output, cloud fraction for each grid point is defined by a binary number: 1 for cloudy and 0 for cloud free. In our study, cloud fraction for the targeted area (e.g. 1°×1° upwind sub-domain) at a given level is defined to be the ratio of the number of cloudy grid points to the number of all grid points at that level.
>
> The sentence in the manuscript has now been revised to be clearer (Line 383-386).
>
> “Note that, in the WRF output, CF for each grid point is given by either binary number 1 or 0 to indicate either cloudy or cloud free pixel. In this study, CF of the target domain (e.g. 1°×1° upwind and downwind sub-domains) is defined as the proportion of total grid points in the domain that are classified as cloudy grids for each model level.”

Figure 9: I know it’s often a personal preference, but I always get confused when precipitation is plotted so that less precip is blue and more is red, I intuitively think of it the other way around.

> The Figure 9 in the manuscript has now been revised with a reversed colormap applied.

Line 380: I would say the spike isn’t that ‘notable’ – it would be good to mention that it is only in WFAx5, and it is preceded by a gradual ramp up? Why would a change in wind direction change the aerosol properties?

> Under the steady easterly wind condition, we would expect a uniform increase in WFA number concentration over the sub-domains throughout the simulations given that only increases in aerosol surface emission over the GBR are considered. However, when the wind condition changes to southeasterly, inflow from the southern part of the GBR with increased aerosols will bring in an additional amount of WFA to the sub-domains.
>
> We observe the gradual increase in WFA in the last day of the simulation (both for Aerosol2 and Aerosol5, but with different magnitude). This is considered to be correlated with gradually changed wind condition from easterly to southeasterly.
>
> The sentence in the manuscript has now been revised for clarity (Line 520-531) as follows.
>
> “The gradually increase of the WFA number concentration during the last day of the simulation is primarily attributed to the strong inflow with an additional significant amount of aerosols from the southern portion of the GBR when the surface wind changes from easterly to southeasterly (Figure 4).”

Line 388: Same results are found (though not shown) in Fiddes et al. 2022 (see last paragraph of results section). You can find plots for this here: https://minerva-access.unimelb.edu.au/items/f7b061c0-0bf8-5574-aaa2-e9f83f5fe854

> Thank you for the reference. We have now revised the sentence to include this information (Line 537-538).

Line 392: The increase in LWP is really only occurring on a few occasions and not uniformly – can you comment on this?

LWP is a measure of the total amount of liquid water in clouds. It is not only correlated with the droplet number concentration, but also droplet sizes (Han et al., 2002). Variations in cloud formation processes, evaporation and precipitation can all lead to fluctuations in LWP. Following the suggestion, we have included a following comment about the occasional increase in LWP observed in our Aerosol sensitivity experiments (Line 570-574).

"Note that fluctuations in LWP response are observed throughout the simulation. Previous studies have shown that multiple processes (e.g. cloud formation processes, evaporation, and precipitation) play a role in determining the LWP response to aerosol perturbations (Han et al., 2002). Also, meteorological conditions (e.g. relative humidity) could strongly modulate the LWP-droplet number concentration relationship (Gryspeerdt et al., 2019)."

Line 395: Due to cloud lifetime effects? Does your model support these indirect effects?

We think this is likely due to the cloud lifetime effects, as noted in the revised manuscript (line 548). The increase in LWP with higher aerosol loading, as seen in Aerosol sensitivity experiments, is primarily due to the rise in CDNC. This increase in CDNC leads to smaller cloud particle sizes, which in turn inhibits rain formation. This is consistent with the cloud lifetime effect (Albrecht 1989; Zhang et al., 2016), where changes in aerosol concentrations alter the microphysical properties of clouds, influencing their duration and characteristics.

That said, in order to confirm this one would need to properly track the cloud targets throughout their lifetime in the simulation and understand how they differ from the control run. Given the natural variability of these cumuli, however, isolating the cloud lifetime effects (if any) from other influencing factors would still be a challenging task. Therefore, we prefer not to be too speculative on the lifetime effects in our discussion.

Line 414: Can you provide some stats to support this 'respond strongly' statement? It would be useful to have a quantitative idea of this.

Thank you for your suggestion. Manuscript has now been revised to include more statistics (line 565-567).

References:

Albrecht, B. A. Aerosols, cloud microphysics, and fractional cloudiness. Science 245, 1227–1230. 1989.

Bretherton, C. S., & Blossey, P. N.: Understanding mesoscale aggregation of shallow cumulus convection using large-eddy simulation. Journal of Advances in Modeling Earth Systems, 9(8), 2798–2821. https://doi.org/10.1002/2017MS000981. 2017.

Chin, M., and Coauthors: Tropospheric aerosol optical thick- ness from the GOCART model and comparisons with satellite and sun photometer measurements. J. Atmos. Sci., 59, 461–483, doi:10.1175/1520-0469(2002)059,0461:TAOTFT.2.0.CO;2. 2002.

Ghan, S. J., X. Liu, R. C. Easter, R. Zaveri, P. J. Rasch, J. Yoon, and B. Eaton: Toward a Minimal Representation of Aerosols in Climate Models: Comparative Decomposition of Aerosol Direct, Semidirect, and Indirect Radiative Forcing. J. Climate, 25, 6461–6476, https://doi.org/10.1175/JCLI-D-11-00650.1. 2012.

Gryspeerdt, E., Goren, T., Sourdeval, O., Quaas, J., Mülmenstädt, J., Dipu, S., Unglaub, C., Gettelman, A., and Christensen, M.: Constraining the aerosol influence on cloud liquid water path, Atmos. Chem. Phys., 19, 5331–5347, https://doi.org/10.5194/acp-19-5331-2019, 2019.

H. Wang, Easter, R. C., Rasch, P. J., M. Wang, X. Liu, Ghan, J., Y. Qian, J.-H. Yoon, Ma, P.-L., & Velu, V.: Sensitivity of remote aerosol distributions to representation of cloud-aerosol interactions in a global climate model. Geoscientific Model Development Discussions, 6(1), 331–378. https://doi.org/10.5194/gmdd-6-331-2013. 2013.

Han, Q., Rossow, W.B., Zeng, J. and Welch, R.: Three different behaviors of liquid water path of water clouds in aerosol–cloud interactions. *Journal of the atmospheric sciences*, *59*(3), pp.726-735. 2002.

Thompson, G., and Eidhammer, T.: A study of aerosol impacts on clouds and precipitation development in a large winter cyclone. Journal of the atmospheric sciences, 71(10), 3636-3658, https://doi.org/10.1175/JAS-D-13-0305.1, 2014.

Vogel, R., Nuijens, L., & Stevens, B.: Influence of deepening and mesoscale organization of shallow convection on stratiform cloudiness in the downstream trades. Quarterly Journal of the Royal Meteorological Society, 146(726), 174–185. https://doi.org/10.1002/qj.3664. 2020.

Zhang, S., Wang, M., Ghan, S.J., Ding, A., Wang, H., Zhang, K., Neubauer, D., Lohmann, U., Ferrachat, S., Takeamura, T. and Gettelman, A.: On the characteristics of aerosol indirect effect based on dynamic regimes in global climate models. Atmospheric Chemistry and Physics, 16(5), pp.2765-2783. 2016.

Zhao, W., Huang, Y., Siems, S., and Manton, M.: The role of clouds in coral bleaching events over the Great Barrier Reef. Geophysical Research Letters, 48, e2021GL093936. https://doi.org/10.1029/2021GL093936, 2021.