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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Improved simulation of clouds over the Southern Ocean in a General Circulation Model" *by* Vidya Varma et al.

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Discussion paper

Reviewer 2 comments

We would like to thank the anonymous reviewer for his/her comments. Below is the detailed point-by-point reply to the comments.

1 Summary

"However, the study has some issues involving justification of the experimental design, discussion of the simulations, and clarity of the figures and writing. If these issues are addressed, then the manuscript might be acceptable for publication. I therefore recommend major revision."

Manuscript modified as per comments below

2 Specific Points

1.Title : I think “improved” is not appropriate to use in the title since the authors did not improve the theory on which the cloud parameterizations are based. Changing

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Discussion paper

the tuning parameters in a model, as the authors have done in this study, is not the same thing as improving the model. I suggest that the title be changed to something like “Bias of Southern Ocean cloud albedo in a general circulation model linked to ice-crystal shape.”

Title modified

2.Abstract : All of the abstract is fine except for the last sentence. The last sentence should be removed because the authors did not do any new work to justify this statement (“We hypothesize that such abundant supercooled liquid cloud is the result of a paucity of ice nucleating particles in this part of the atmosphere.”). It is unethical to make this statement in the abstract because the statement is based entirely on the work of others. It would be fine to include this statement in the discussion section with proper references, of course.

Modified

3. Data and Experimental Set-up: The experimental design needs to be explained and justified in more detail. For instance, the authors perform a sensitivity study in which the ice-crystal shape is modified. This is done by multiplying the “capacitance” (C) value by a factor of 0.5, which effectively changes the ice-crystal shape from spheres to ellipsoids. However, the authors do not cite any theoretical or observational work to justify their choice of 0.5 until the Discussion section (pg. 6 line 8), and even there it is simply stated that the choice of C is reasonable without any explanation. More background information justifying the choice of C=0.5 is needed in Section 2.1. It would also be nice if the authors provided some justification for their choice that is based on in situ observations over the Southern Ocean, perhaps from the recent SOCRATES field campaign.

We have added some more information in Section 2.1 (Page 3 lines 5-8). Regarding in situ observations, we are not aware of any capacitance or aspect ratio of ice crystals related data from SOCRATES.

A second issue is that, as far as I can tell, some of the simulations and discussion are unrelated to the study goals. Simulations exp2 and exp3 use modified temperatures for ice nucleation in the convection and microphysics parameterizations. How do these experiments contribute to the goal of understanding how ice-crystal shape affects Southern Ocean cloud albedo?

We have added more details regarding this in Section 2.1 lines 5-29

Also, the control simulation is compared to older versions of the model with no explanation of how this comparison helps to understand the cause of the cloud albedo bias in the current model (pg. 5 line 20, Figure 6). I do not understand the value of exp2, exp3, or the older versions of the model presented in Figure 6. Please discuss this or remove the content.

We have now removed the comparison with earlier model versions

4. Results and Discussion
The Results section is hard to follow. It would help to organize the figures and text in a consistent way. The text discusses model bias in the TOA and surface energy budget terms one at a time, so it would be helpful if the data presented in Figure 3-5 were also organized based on different energy budget terms. For instance, Figure 3 could have one panel that shows LW TOA in ctrl, exp1, exp2, and exp3; another panel that shows SW TOA in ctrl, exp1, exp2, and exp3; and so on. Since model bias is the quantity of interest, it would also help to show all of the anomalies relative to observed values (e.g. ctrl – obs, exp1 – obs, exp2 – obs, exp3 – obs) rather than anomalies relative

to the ctrl experiment in some of the panels and anomalies relative to observations in other panels.

We have added modified figures

Another issue is that the content of the Discussion section doesn't seem to logically follow from the content of the Results section. The Results section describes how the model biases change as a result of the modifications to the cloud parameterizations, which is fine. But no clear conclusion about what was learned from these simulations is reached in the Discussion section. Should other modeling groups change the ice crystal shape in their models? If so, what range of capacitance values is suggested by observations and theory, and what values do the authors recommend using? How much of the Southern Ocean cloud albedo bias will be fixed by changing the ice-crystal shape? Please make a clear statement about what was learned from your work before starting the discussion about how other studies say that ice-nucleating particles are the critical thing to study (pg. 6 line 31).

Modified the Results, Discussion and Conclusion sections to make our findings more clear

5. Technical Corrections
Figure 1 – Change axis label to "IWP (km/m2)" to match the rest of the text.

Removed the figure

Figure 2 – Why is the range of the x-axis so much larger in 2a-b than in 2c-d? Make the axis range consistent across all panels.

Modified

Figure 2 – I suggest moving all of the information about cloud types from the figure caption to the main text.

Added cloud type details in the main text; Page 4 lines 20-23

Figure 3,4 – Please organize the data so that one panel shows one energy budget term only, and that all anomalies are shown relative to observations, as mentioned in my comments on “Results and Discussion” above.

Modified figures added

Figure 6 – What value does this figure add to the study? I think this figure should be removed

This figure has been removed.

Figure 7 – What does this figure show that isn’t already shown in Figure 5? It shows a big response in the tropical western Pacific to changing the nucleation temperature, but this isn’t relevant to understanding Southern Ocean cloud albedo biases.

We have added more details in the results section explaining this.

Figure 6,7 – The colorbar makes these figures very difficult to read. Please change the colorbar to a two-color scale with white at zero. For example, the colorbar could have red for positive values, white for near-zero values, and blue for negative values.

Modified figure added

Pg. 1 line 19 “observed radiation biases” – delete “observed”

Deleted

Pg. 2 line 6 – specify that “this model problem” means cloud albedo bias over the Southern Ocean

Modified

Pg. 2 line 8 – I recommend moving the sentence “In the present study, we investigate. . .” to the end of the preceding paragraph and moving the sentence “Here, we define a SO. . .” to Section 2 Data and experimental set-up. I think it helps to finish the Introduction with a concise statement of the study goals, which is what the first sentence does.

Modified; Page 2 lines 7-10

Pg. 2 line 12 – Why isn’t this paragraph in section 2.1 Model set-up?

We have added few more background details in the UM model version in the Appendix.

Pg. 2 line 13 – Is it necessary to put the model description in an appendix? Appendix A is only one paragraph long, after all. It improves the clarity of the paper if the reader doesn't have to jump around between different sections.

We have modified Section 2. Appendix is included with more details now as these are not publicly accessible yet due to licensing issue.

Pg. 3 line 14 "parametrised convection scheme" – "parametrised" is redundant and can be deleted.

Modified

Pg. 4 line 8 – Why does modifying the capacitance value affect liquid and ice? Does the capacitance value control the diffusional growth of liquid droplets as well? If so, then I don't think that C=0.5 is realistic for liquid droplets. Also, why does changing the ice nucleation temperature predominantly affect IWP? I think other studies suggest that it should affect both LWP and IWP [e.g. Kay et al., 2016].

The effect of capacitance on liquid is mostly indirect. When ice grows slower, it leaves more water vapor around to condense to liquid drops. And if the capacitance is high, then ice crystals grow faster and there is less liquid. So, by making the capacitance value to 0.5 from the default value of 1.0, we are in a way reducing the depositional

growth of ice crystals, leaving more room for water vapor to condense (e.g.Wegener, 1911; Bergeron, 1935; Findeisen : same also provided in the main text reference). Kay et al., 2016 shows the improvement in radiation biases over SO by modifying the shallow convection temperature rather than tuning the cloud microphysics.

Pg. 4 line 11, Pg. 5 line 9, Pg. 5 line 19 – Please don't just state that these figures are included in the supporting information. You need to say what the figures show and how they contribute to the findings of the study.

We have modified the Supplementary section and its reference in the main text. Page 4 lines 22-24

Pg. 4 line 17 – Why is the change in TOA LW flux so large in your simulations? LW radiation was not part of the motivation, yet TOA LW flux is more sensitive than TOA SW flux to the model modifications made in this study. Please explain this.

For the atmosphere only version of the model (i.e. without any interactive sea surface temperature), the LW changes are slightly complicated because any changes in the radiation budget of the SW does not have any impact on the outgoing radiation from the sea-surface. But the changes that we see here in the LW could be mostly due to the changes associated with the amount of cloud cover and cloud height that we observe in the experiments. When there is more horizontal cloud cover then more of the surface is covered and that will have an impact on the LW distribution. Also, when the cloud height changes i.e when it becomes thicker that could also impact the LW. We have now emphasized in the discussion/conclusions sections that the capacitance changes are aimed mostly at the boundary layer clouds and nucleation temperature changes could also influence the high clouds. We have also now made it more clear

that it is the SW flux that is mostly benefiting and also mention about the detrimental effects on other fluxes.
Page 6 lines 20-25

Pg. 4 line 22 – By “show an increase” do you mean an increase relative to the control experiment? Please clarify.

We have modified the Results section.

Pg. 5 line 1 – It would help to discuss the difference between the control simulation and observations first to establish the baseline model bias, then discuss how the bias changes in exp1-3. Please rearrange content accordingly.

Modified

Pg. 6 line 18 “The atmosphere-only model studied here does perform better. . .” – Please use more specific language. For example, “model bias in SW CRE is reduced over the Southern Ocean.”

Modified