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Interactive comment

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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Reviewer 1 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

“But while the manuscript does show that changing the capacitance has an impact on the simulated SW radiation bias in the SH, the impact on longwave (LW) radiation and the Northern Hemisphere (NH) are not sufficiently considered (the latter is only shown in the supplementary material and not discussed), although the agreement with observations decreases. Furthermore, the properties of ice containing clouds will depend on further uncertain processes in a GCM like aggregation (efficiency). Even without cloud ice forming in mixed-phase temperature regime the SW radiation bias in the SH is not fully removed (exp2). It remains therefore unclear whether the claimed improved simulation of clouds over the Southern Ocean still holds when other aspects are considered. Therefore publication in ACP cannot be recommended unless these issues are addressed.”

We have modified the manuscript as per the points below.

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2 Specific Points

1. P1L4: Is it more realistic to use the capacitance of Field et al. (2008) everywhere? In mixed-phase clouds (which occur frequently in the Southern ocean) riming can be important, hence more spherical ice particles can be present in these clouds.

Riming would eventually produce quasi-spherical particles, but in highly supercooled environments at water saturation, the growth due to deposition is likely to be faster. This is not only due to the high ice supersaturation but also because riming with cloud droplets is diminished. Observation by Harimaya (1975) shows that droplets with diameters less than 10 μm are too small to be collected onto ice crystals. Westbrook and Illingworth (2013) show some examples of pristine particles in supercooled layer clouds. Particles like these stellars have a capacitance close to the one we are using. Also the description of their fig5 points out the strong Z_{DR} (differential reflectivity) signal indicating non-spherical oblate particles.

References:

Westbrook, C. and Illingworth, A.: The formation of ice in a longlived supercooled layer cloud, Q. J. Roy. Meteor. Soc, 139, 2209–2221, doi:10.1002/qj.2096, 2013.

Harimaya, T., 1975: The riming properties of snow crystals. J. Meteor. Soc. Japan, 53, 384–392.

2. P1L9: The reduction of the bias of $\sim$4 Wm^{-2} should be put into context of the strength of the bias in the model. Also this reduction in SW bias is accompanied by an

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increase in the LW bias.

The reduction of SW cloud radiative effect of 4 W/m2 that we have shown is for the TOA since changes are more certain compared to the surface flux. As mentioned in the revised 'Observational data' section, the surface changes are prone to more uncertainties. CERES surface data itself depends on the uncertainties in the radiative transfer model.
Page 4; Section 2.2

3. P1L10-11: This is what Vergara-Temprado et al. (2018) have shown. What is your original contribution? In the conclusions it is written that INP's are not represented in the model, this can be mentioned in the abstract as well.

Removed the last sentence of abstract in the new version that mentions INP

4. P1L19: You mean in the Southern Ocean. These studies include for example Williams et al. (2013) and Lohmann and Neubauer (2018).

References added.
Page 1; Section 1

5. P2L20: For which years is the sea-surface temperature climatology computed? Is Schuddeboom et al. (2019) the right citation for AMIP simulations?

Reynolds SST for the years 1981 – 2012 has been used. Gates et al 1999 is the reference for AMIP (reference already included). Schuddeboom et al., 2019 is another study where a version of the control run used in of our study was used, hence cited it. Removed it to avoid any confusion.
Page 2; Section 2.1

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6. P2L21: fig. 1 and further references to figures in the text: follow the manuscript preparation guidelines for authors of ACP

Corrected in the new version

7. P2L32: Why is the ventilation factor not considered in eq. (1)?

The capacitance value of 0.5 that we have used in our model does take into account ventilation factor as well (as per Field et al 2008) although it is challenging to quantify the effects of ventilation factor and deposition rates separately.

8. P3L7: For a sphere the capacitance is 0.5 x (maximum particle dimension). This is mostly difference in the naming convention or defining the 'capacitance'. Wesbrook et al., 2008 or Field et al., 2008 defines C = 0.5 for spheres and the product C*D is the capacitance. It is however conveying the same message. Essentially the default value of 'capacitance' is reduced from 1xd to 0.5xd in our study.

9. P3L9: Morrison and Grabowski (2008) use the capacitance of a sphere for small spherical ice and 0.48 times the capacitance for a sphere for unrimed nonspherical crystals, and a linear interpolation in between for partially rimed crystals. Morrison and Milbrandt (2015) use the same in the predicted particle properties (P3) scheme. This is an even more realistic representation of ice crystal capacitance. Could this be implemented in the Unified Model?

No, this approach cannot be applied in the Unified Model as we don't have a prognostic for riming fraction on ice like in their study.

10. P3L10-17: How is heterogeneous nucleation of ice represented in the model?

Does it depend in ice nucleating particles (INP) concentrations or is it just a function of temperature (and if the latter, which function)? Not enough information is provided how heterogeneous and homogeneous freezing is implemented in the GA7.1*. What is the difference between the start-ice temperature and the all-ice temperature?

The control model does not have any ice-nuclei dependency for the heterogeneous nucleation. The heterogeneous nucleation temperature is simply following the temperature dependent function suggested by Fletcher [1962] (N. H. Fletcher. The physics of rainclouds. Cambridge University Press, London, UK, 1962). This then gets multiplied by a small ‘seed’ ice content for ice free clouds in order that the other micro physical terms can grow it. As far as homogeneous nucleation of liquid water is concerned, all liquid water at temperatures less than $-40°$C is instantaneously frozen to form ice particles according to Rogers and Yau [1989]. (R. R. Rogers and M. K. Yau. A short course in cloud physics. Pergamon Press, Oxford, 3rd edition, 1989). At ’start-ice temperature’, the detraining of liquid condensate as ice begins in the model and by ’all-ice temperature’, all condensate is detrained as ice. These details have been added in the revised version under Model set-up.
Page 3; line 12 - 29

11. P3L20-21: Since this is not a model version that has been already described in another publication, it needs to be mentioned whether the experiment setup is similar to another study, otherwise details need to be given here. Why is 12 hourly output used? This means that the diurnal cycle is not well represented in the simulations. CERES-EBAF provides a diurnally complete representation of Earth’s radiation budget (Loeb et al., 2018).

Appendix modified (Page 8). In the newer version of the manuscript, we now use the daily-mean values for radiative fluxes from model

12. P3L22: ERA5 is a re-analysis dataset not an observational dataset.

We have removed ERA5 data in the new version. a modified Observational data section is included.
Page 4; Section 2.2 lines 7 - 12

13. P3L28-29: What was the reason to choose ERA5 as a reference for IWP given the large differences of IWP between different datasets (Duncan and Eriksson, 2018)? An uncertainty range for IWP should be added.

In the newer version, we are not using this comparison for IWP.

14. P3L31-32: Why are IWP/LWP shown only for these clouds? Shallow cumulus clouds may be interesting as well (Forbes et al., 2016).

The focus of this study is mostly on the stratocumulus boundary layer type clouds. Hence, we chose the corresponding types as mentioned in the main material of the manuscript. We have added analyses for other boundary layer types in the Supplementary material. A brief description of other types has now been included in the main material as well.
Page 4 ; lines 17-23

15. P4L1-2: Why is the analysis split into this boundary layer types? Either provide a motivation and discussion for the different boundary layer types or remove this split.

Similar to previous comment. More details included now.

16. P4L8 and all following occurrences: “w. r. t.”: follow the manuscript preparation

guidelines for authors of ACP.

Corrected

17. P4L8-9: Why? Why does changing nucleation temperature not also impact LWP?

The zonal mean liquid water paths that we have shown here are dominated by the fronts mostly. So, even if ice nucleation temperature shows some sensitivity, they are mostly away from the frontal systems and mostly restricted to the shallow boundary layer types.
Page 4 ; line 30

18. P5L6: What does ". . . at surface well" mean? Rephrase.

Corrected

19. P5L13-14: That's an uncommon definition of SW CRE for model simulations. Typically two calls to the radiation routine are done, one with clouds and one without clouds. From these SW CRE is computed, taking into account cloud cover. How is SW CRE computed in partly cloudy gridboxes?

We have rephrased the sentence. In the model, for each grid box there is a cloudy and non-cloudy flux. From these fluxes, the CRE can be calculated using the amount of cloud fraction.
Page 5 line 32

20. P5L16-18: Why do exp1 and exp3 show a stronger reduction in SW CRE than exp2. In exp2 the least cloud ice should be present in the mixed-phase clouds so why

is the SW CRE larger in exp2?

We do acknowledge some uncertainties in the effect of nucleation temperature on fluxes. There are some detrimental effects due to changes in the nucleation temperature that could be mostly due to the changes in the vertical distribution of the clouds affecting not just the low clouds but also the high clouds. By changing the nucleation temperature, we are essentially modifying the level at which freezing occurs. So, when we don't freeze the water lower down then it can go higher up in the atmosphere probably creating cirrus clouds and thus change the high cloud characteristics (thus affecting both short/long waves). A detailed examination of the effects on fluxes is not intended to be within the scope of this study. However, we do acknowledge the importance of this aspect and have incorporated that to be continued in future work. We have stressed this point and made more clarity in the discussion and conclusion sections.
Page 6 ; lines 20-25 and Page 7 ; lines 23-25

21. P5L31-32: Why (see previous comment)?

Reply similar to the previous one

22. P5L33: What are "eastern sects"?

Removed

23. P6L1: Provide references for this statement.

Added ; Page 7 lines 1-4

24. Why are low INP concentrations relevant? Are INP's used in any of the experi-

ments?

Removed the sentence and modified Discussion section; Page 7

25. "temperatures between the homogeneous and the heterogeneous freezing points"; rewrite, it's unclear what is meant

Removed the sentence

26. P6L17: 0.5 x d is used above

Similar to comment 8

27. P6L19-20: Why are these then shown?

We have modified the Results and Discussion sections

28. P6L22: This is not discussed anywhere. Either a discussion is added or the respective experiment and its results should be removed.

Added details in Page 3 lines 12-33

29. P6L29-30: Is there an explanation why the capacitance change has no significant impact in the tropics?

In an earlier study by Furtado et al., 2016 (using the NWP model), it has been shown that for tropics and subtropics there is a general tendency by the model to overpredict the LWP in response to microphysics modifications. Increasing the stratiform cloud LWP will cause more SW radiation to be reflected back to space. But over the South-

ern Ocean, this effect is beneficial because the Unified Model has a large negative bias in outgoing SW radiation in that region. Some possible reasons mentioned are that of flaws in parametrizations, uncertainities in the estimation of LWP in the convection scheme etc. Basically, in a frontal steady state, the capacitance doesn't have much of an impact compared to more dynamic sites like that of super cooled liquid water clouds. Further details can be found in Furtado et al., 2016.
Page 6 lines 11-19

30. P6L29-30: Why does the capacitance change not significantly change or even decrease SW CRE in the tropics? Is this model dependent?

For the first part of the comment, response similar to the previous point. For the second part, mostly it is not model dependent. Changes in capacitance can be translated to any model that uses that factor. Basically any model that is using capacitance change is a sink of water vapour. The impact of capacitance predominantly depends on the amount liquid water already available in the model. So, if some models have very less liquid water, then the impact of capacitance might not be much.

31. P7L9: There's no discussion why these temperature thresholds have been chosen for the sensitivity experiments. Are this thresholds considered to be realistic?

We have added more details in the text; Page 3 lines 20-22

32. P7L20-21: As these changes are not described in the literature or publicly accessible, they need to be described here.

Appendix modified

33. P7L25: The link is not publicly accessible.

As it is not a published version for the control, we have added some additions to its predecessor that are relevant to this study in the Appendix.

34. Table 1: The experiments could have more meaningful names which indicate what has been changed. Why is the all-ice temperature 1 deg C larger than the start-ice temperature?

We have renamed the experiment names for clarity. The 1 deg C change is merely technical to avoid division by zero in the code.

35. Fig. 1: the sensitivity experiments should be added to this figure. ERA5 is a re-analysis dataset not an observational dataset.

We have removed this figure in the modified version

36. Fig. 3 and all similar figures: a vertical line at 0 Wm-2 is missing. Also these figures make it hard to compare different experiments. One panel should rather show anomalies for one variable but for all experiments and observations. Where do the sensible and latent heat observations come from?

We have included modified figures. Observational Data section 2.2 has also been modified to accommodate the changes.

37. Fig. S8b shows that exp2 still has a SW top-of-the-atmosphere (TOA) bias although no more ice is present in the mixed-phase temperature range. This indicates that the SW TOA bias is not only due to the wrong phase of mixed-phase clouds in

GA7.1* but that there are biases also in other clouds.

We have removed this figure. New figures using observational data is used in the main text.

38. Fig. S8 shows that the SW TOA bias in the NH increases in exp1 compared to ctrl. Also from 50S to 60S the SW TOA bias increases in exp1 compared to ctrl. Is changing the capacitance really improving the agreement with observations? The rootmean-square error and correlation coefficient with respect to CERES would show if the experiments are an improvement globally.

We have now removed this figure. As we have now noted in the revised main text, change in capacitance is mostly favorable for the dynamic regions like super cooled liquid clouds (SO for instance). Since, the focus of our study is mainly SO, we have included fluxes and SW CRE plots (zonal) only for the SH. The global spatial plot is shown mostly for completeness and also as a motivation for the importance of INP and we have emphasized this is in the newer version.

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