

Review of „Reducing the Southern Ocean cloud albedo biases in a general circulation model“ by Varma, V., Morgenstern, O., Field, P., Furtado, K., Williams, J., and Hyder, P.

General comment:

The authors improved the representation of their results and clarified many concerns. However, several issues remain. In particular it is unclear if the new shape parameter is indeed an improvement as the changes in SW fluxes in the SO region in the experiments are accompanied by compensating changes in LW fluxes and undesirable changes in NH SW fluxes. Also the biases need to be quantified globally and regionally. From the figures it is unclear if there is a reduction in the Southern Ocean cloud albedo bias. Since the new shape parameter is applied globally also the global impact needs to be shown. A table with global and regional (50° S to 70° S) mean values should be added to quantify the changes by the experiments. This should at least contain the bias to observations but also the correlation coefficient and root mean square error for SW and LW TOA fluxes (at least). For example abstract and main text suggest that not enough SW radiation is reflected in the SO region (50° S to 70° S) in the ctrl experiment compared to observations, but is this actually the case? This bias needs to be quantified. Furthermore, no clear recommendation is given where the new shape parameter should be applied (globally or only in certain regions). Publication in ACP can only be recommended after these issues are addressed.

Specific comments:

Title: Is the Southern Ocean cloud albedo bias reduced compared to observations? This needs to be quantified, see general comment.

P1L1-10: The SW changes are accompanied by LW changes; this needs to be mentioned in the abstract and main text and also be discussed in the main text.

P2L18: Unless this a named sea surface temperature climatology remove the name here.

P3L1-10: It needs to be discussed here where the new shape parameter is more realistic than the previous one (based on literature), at which latitudes and at which temperature (ranges). In particular it needs to be shown that it's more realistic for the SO region.

P3L18: Do not use contractions in scientific writing.

P4L22: The shear-dominated unstable layer shows a similar change in LWP in the cap experiment as the stratocumulus boundary layer type clouds. This needs to be discussed.

P4L29-31: The change in the shape parameter will slow down the WBF process but the changes to nucleation temperature will determine if there is ice in the first place, which will also impact the WBF process. So why does the change in the shape parameter affect both LWP and IWP while changes to nucleation temperature mainly impact IWP?

P4L31: LWPs in Fig. 1 are for stratocumulus boundary layer type clouds, why are these dominated by fronts?

P5L26: Even in the $c_{\text{tnuc}}=-40$ simulation the SW TOA and surface biases compared to observations remain although there will be no more ice in mixed phase clouds. Therefore also other clouds should also contribute to these biases.

P5L32-33: Is it cloudy and non-cloudy grid boxes or cloudy and non-cloudy parts of grid boxes?

P6L7: control model, control run, ctrl experiment: choose one expression and use it consistently everywhere

P6L7-8: Not for the SO region (50° S to 70° S). What is the net bias over this region?

P6L8-9: It is stated that SW CRE is reduced over SO, but in the title or abstract the opposite is stated. For which experiments is there an improvement? This needs to be quantified (see general comment).

P6L9-10: This is in contradiction to the previous sentence.

P6L11: Do not use contractions in scientific writing.

P6L11-13: Why is the effect of the changed shape parameter in the tropics so much smaller than in the SO region?

P6L12-13: Don't you mean increase in SW CRE here? Fig. 2 suggests an increase in SW CRE in the sensitivity experiments.

P6L16-17: From Fig. 4b it is not clear that this is actually the case.

P6L18: Does the model have convective LWP? Rephrase otherwise

P6L18-19: Rephrase this sentence as it is unclear.

P7L1: Only impacts on mixed-phase clouds are discussed but what is the impact of the new shape parameter on cirrus clouds? This needs to be discussed as well.

P7L2-11: This belongs to the introduction.

P7L4: It is unclear what "It" refers to.

P7L18: "frontal steady states": I'm not familiar with this expression

P7L20-21: As there are undesirable biases in the NH (which need to be discussed more), what is the conclusion for the shape parameter? Should the new shape parameter be used in global climate models or not?

P7L29-32: Which assumptions about the INP concentration are made?

P8L9-10: Has the bias compared to observations in the SO region (50° S to 70° S) indeed been reduced? This needs to be quantified (see specific comments above and general comment).

P15: Fig. 2: The use of upward fluxes for TOA and downward fluxes for the surface should be specified in the title of each panel. For all figures that show fluxes it should be specified in the panel titles if upward or downward fluxes are shown. In the caption of Fig. 2 it is mentioned that upward fluxes are used for TOA and downward fluxes for the surface. This information needs to be added to other figures that show upward or downward fluxes.