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Interactive comment on "The Microphysics of Clouds over the Antarctic Peninsula — Part 2: modelling aspects within Polar WRF" *by* Constantino Listowski and Tom Lachlan-Cope

Anonymous Referee #2

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The authors present a modeling study of various microphysics schemes and compare model results to observations to aircraft measurements in the Antarctic. They find significant differences in predictions of the cloud thermodynamic phase and conclude that the Morrison scheme is the best scheme as it leads to the best model/observation comparison in terms of clouds and radiation. Fore The number of model studies on cloud in Antarctica are sparse, even though the region is very sensitive to changes in climate. Therefore, the current study might represent an important contribution to the literature if my comments below will be addressed.

General comments

1) I am not convinced that the authors can indeed conclude on 'the best performing'

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Discussion paper

microphysics scheme based on their model studies. They show that the Morrison scheme predicts best the super-cooled liquid in clouds, followed by the Milbrandt and Thompson schemes since all three have a sophisticated description of the various ice categories. Given the large uncertainties that are associated with the representation of clouds in general and of mixed-phase clouds in particular, the comparison to only a few parameters might not be sufficient to identify 'the best' scheme. Uncertainties in the radiation and boundary layer schemes might lead to a prediction bias such as the microphysics scheme may give the right answer for the wrong reason. I suggest a more careful wording throughout the manuscript. At several places in the manuscript, the language is rather colloquial; I listed several instances below but encourage the authors to carefully read and improve language where possible.

2) The authors state that none of the applied microphysics schemes was specifically developed for Antarctic clouds and, therefore, their study is a first step in exploring the skills of the different schemes for such scenarios. However, I am missing a more conclusive statement on what improvements should be done in these schemes to optimize them for Antarctic clouds.

3) It is mentioned that the liquid phase of clouds (drop activation) is either described based on a fixed number of droplets or it is a function of the CCN (p. 7, l. 5). Drop activation is an essential process that determines the microphysical properties of a cloud. How much difference is caused between the different microphysics schemes due to differences in the prescription of the cloud droplet number?

4) The authors describe in length the differences in the results predicted by the microphysics schemes in Section 4.1. However, in order to understand the differences and to assess the skill of the various schemes, a more detailed discussion of the underlying processes is needed. Such analysis will help to identify the 'best scheme' for the right reasons and to improve existing microphysics schemes.

5) Several previous studies have highlighted the importance of ice particle shapes for

ice growth and sedimentation and therefore for the partitioning between ice and liquid phases (e.g., Sulia and Harrington, JGR, 2001). How are ice particle shapes treated here? Are the predicted shapes in general agreement with observations?

6) Only in Section 5, it is mentioned that the radiation scheme assumes a drop radius of cloud droplets. How does this value compare to observations and how does it affect in general the radiation prediction? Are there any studies (not necessarily in Antarctica) that have discussed biases in predicted radiation due to a constant effective radius?

7) Each microphysics scheme includes a different parameterization of ice formation. Different INP parameterizations can lead to significant differences, as it has been shown in several literature studies (e.g. Eidhammer et al., JGR, 2009). How much of the predicted differences in comparison to observations and model/model comparisons can be ascribed to the differences in INP parameterizations? Why not using the DeMott parameterization as the base case as it has been shown to perform better than the temperature-dependent-only parameterization in previous studies?

8) The authors should better justify their choice of a 5 km resolution. At the end of Section 5, they state that convective processes are badly resolved on this resolution; however, they fail to discuss the consequences of this caveat for simulating clouds.

9) Throughout the paper, expression such as 'the xxx scheme forms more liquid' etc should be avoided (e.g. p. 13, l. 15). The schemes themselves do not form or produce any ice or water. Wording such as 'Using the xx scheme, it is predicted . . .' (or similar) should be used instead.

Minor comments

p. 2, l. 34: Not clear what 'the latter case' refers to. – Do you mean all three, i.e. immersion, contact or condensation freezing? If so, it might not be fully correct, since droplets may scavenge INP before ice nucleation and therefore INP and CCN are not the same.

p. 7, l. 18: The beginning of the sentence is not clear. Does the AMPS model predict a liquid phase or not?

p. 8, l. 10: What do you mean exactly with 'twice to four times more liquid clouds'? – Does it refer to the number of clouds or to the liquid water content of the clouds?

Figures 2 and 3: In particular, in the upper panels, the dotted lines are pretty hard to distinguish.

p. 11, l. 6: I think (ice) should be removed – or SWC0 added at the beginning of the sentence (?)

p. 17, l. 5: Can an estimate be given how much the observed mass is biased due to the small and large cut-off diameters? – An extrapolation of the measured size spectrum might be better than the current complete omission.

Technical comments

p. 1, l. 9: 'struggle' is rather colloquial

p.2, l. 10; p. 3, l. 7, and other places: Antarctic

p. 3, l. 6: Southern

p. 4, l. 14: discriminated

p. 4, l. 26: one domain

p. 5, l. 11: developed

p. 6, l. 10: cloud formation

p. 6, l. 25: flight measurements

p. 7, l. 21: observed

p. 8, l. 1: domain

p. 8, l. 27: demonstrate

p. 9, l. 2: tests

p. 11, l. 8: Add unit to LWC (0.02)

p. 12, l. 28: wind regimes

p. 12, l. 33: either 'shows a. . .transect' or 'shows . . .transects

p. 16, l. 19: flights

p. 17, l. 14: equal

p. 19, l. 27: increases

p. 21, l. 9: simulation

p. 21, l. 24: are consistent

p. 23, l. 7: agrees

p. 25, l. 3: crystal

p. 26, l. 15: 'The transects in Figure 5a and b clearly show..'

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