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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 #1

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Review of "The microphysics of clouds over the Antarctic Peninsula – Part 2: modeling aspects within Polar WRF" by Listowski and Lachlan-Cope

Recommendation: Requires revision before publication

This paper presents comparisons of simulations of clouds over the Antarctic Peninsula using different cloud microphysical schemes in WRF, and includes comparisons against observations described in Part 1 of the manuscript. The main finding of Part 2 is that, regardless of which scheme is used for the simulations, a large misrepresentation of the cloud thermodynamic phase explains a lot of the large radiative biases derived at the poles continent wide (i.e., all schemes fail at predicting as much supercooled liquid mass as seen in observations). The authors conclude that a parameterization scheme

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Discussion paper

for ice nucleation that depends on both temperature and aerosol content should be implemented in WRF to get a more realistic representation of primary ice production, and hence a better representation of supercooled liquid phase in Antarctic clouds.

The study should be published because unique modeling simulations are compared with a unique set of data over the Antarctic Peninsula. However, the paper can be improved in a number of ways. It is lacking in that it only focus on sensitivities to cloud microphysics, ignoring impacts from other parameterization schemes that could also be affecting the simulations and comparison with observed quantities. The uncertainties associated with the representation of the microphysics needs to be better placed in context of uncertainties associated with the representation of other processes such as boundary layer parameterization schemes. Otherwise, it is possible that the sought after agreement between models and observations may occur due to reasons not associated with the representation of microphysics (i.e., are the right answers being obtained for the wrong reasons). I'm not convinced that simulations that do not consider uncertainties due to other processes can truly assess the ability of the microphysics schemes to model realistic clouds across the Antarctic Peninsula (page 5, line 10) in the absence of these other sensitivities. This is especially true given the statement that none of the microphysics schemes adequately predict the supercooled water: maybe some other process other than microphysics is causing this problem. For example, I think that the amount of fetch off the ocean and its representation would be very important.

The sensitivity studies in the paper emphasize how different schemes affect the modeled cloud parameters. But, in addition to sensitivity in choice of scheme, there can also be sensitivity to some of the parameters (i.e., constants) that are assumed within an individual scheme. Was any effort made to look at the sensitivity to constants within a scheme?

Another weakness of the paper is that the paper does a reasonable job in describing the differences between the simulations and the simulations that are most consistent with the observations (note, I think most consistent with the observations is a better way of wording it rather than saying the best simulation). However, I a paper in ACP should do a better job in showing why these differences between parameterization schemes are occurring. This can be done by looking at various prognostic terms in the model (i.e., production rates due to various microphysical processes). This could explain why different schemes produce different modeled fields rather than just stating that different schemes produce different cloud fields; the result that different microphysical parameterization schemes gives different modeled fields is not an incredibly new or exciting result in that it has been previously demonstrated in a wide number of other meteorological conditions.

The paper attributes a lot of the disagreement between the modeled and observed fields to the performance of the ice nucleation parameterization: namely, it is state that the ice nucleation parameterization is a major reason why supercooled water is underestimated. For example, it is stated that forcing a dependence on aerosol amount and temperature, rather than just temperature, could give better agreement. Can more be done with adjusting constants in the ice nucleation schemes within the different cloud parameterizations to better demonstrate this? This way it would be easier to define how much of the difference is due to the different microphysical schemes, and how much is due to the microphysical parameterization schemes (rather than just stating that different parameterization schemes use different representations of INPs that may be causing some of the differences).

The authors conclude that the Morrison scheme is the best performing scheme. It would have been beneficial to show some production and depletion rates of processes producing various hydrometeor categories (especially supercooled water) so that there could be a focus on specific terms and processes that are well represented in that scheme, and hence to better determine where the supercooled water is coming from. That might also help explain why the Morrison scheme is the better performing scheme.

The authors use the Brown and Francis (1995) mass-dimension parameterization to derive the ice water content. However, this is a very crude parameterization whose use may be inappropriate depending on the mixtures of the shapes and sizes of crystals present. What types of habits were noted in the CIP images? Are they consistent with those assumed in the Brown and Francis parameterization? How sensitive are the calculated IWCs to the assumed parameterization? Does this assumption affect any of the qualitative conclusions?

The smallest domain used in the model simulations is 5 km. This seems quite coarse for a study looking at the impacts of microphysical processes. What vertical resolution is used? Does the choice of horizontal and vertical resolution have any effect on the most important findings of this study? In addition, how sensitive were the simulated fields to the choice of initial conditions (e.g., either the product used or the time at which the simulations were initialized)?

I am also worried about some of the inconsistencies between the microphysics and radiative parameterization schemes that seem to exist in the paper. For example, radiative schemes may make specific assumptions about the shape distributions of various hydrometeor categories. Are these assumptions consistent with what is assumed in the microphysical schemes themselves? Otherwise, an inconsistency between the radiative and microphysical parameterizations could be responsible for some of the discrepancies in the radiative parameterization schemes. Further, the authors themselves seem to state that the assumption of a constant effective radius of 10 micrometers in the radiative scheme may not be consistent with what the microphysical parameterization scheme is assuming.

SPECIFIC COMMENTS

Page 4, line 13: What diameter is the distinct peak located at? Also, recommend using size distribution instead of spectra. Spectra typically refers to radiative quantities.

Page 5, line 14, recommend “graupel is” rather than “graupel are”

Page 8, line 9-10: Degree of agreement is always a very relative term. I think when talking about whether simulations agree or disagree, the writing should be more quantitative stating what the degree of agreement is.

Page 8, line 19: How much graupel was in the observations? Can you give some indication of how much agreement there is between modeled/observed graupel?

Page 9, line 16: Is the radiative scheme consistent with each of the microphysical parameterization schemes?

Page 10, line 4: Shouldn't the radiative schemes be consistent with each of the microphysical parameterization schemes rather than just the K15 scheme?

Page 12, line 8: It would be nice to know the specific terms that are producing the supercooled liquid mass content in the model.

Line 12, line 22: If two schemes use the INP parameterizations, is it fair to say that the microphysical parameterizations are causing the differences, or should it just be attributed to the INP parameterization. This sentence is clear, but I am wondering how this affects some of the preceding discussions in the paper.

Page 13, line 19: If the effects of the mountains are variable/valuable, what are the prognosed values that are affecting the generation of the LWC? This might be especially good to look at given that quantities east and west of the mountain range are being compared.

Page 14, line 7: the statement that the schemes perform worst on the West compared to the East is interesting. But, the paper would be much more insightful if it could identify the processes that are at work so that the schemes are performing better on the West compared to on the East.

Page 15, line 4: There is an overemphasis on what is the best scheme. You might be getting the right answer for the wrong reasons. You should also look at what are some of the prognosed terms allowing this scheme to perform better, and how the sensitivity to microphysical parameterizations is affected by sensitivity to other parameterization schemes.

Page 17, line 9: How much of the missing mass might not be detected? I think a simple estimate of this can be made if you extend the mass distributions to larger sizes (simple exponential or lognormal fit) to estimate how much of the mass that you are missing.

Page 19, line 1: How much of a difference is important? What difference is acceptable in terms of being a good match with observations?

Page 19, line 7: This paragraph has a very good description of errors associated with these measurements. It would be nice if a similar comprehensive discussion of the uncertainties and errors associated with the microphysical measurements were included, especially given that microphysics is the focus of this study.

Page 21, line 12: Should you state that WSM5 and WDM6 should not be used for these studies when the other boundary layer/other parameterization schemes are being used? This conclusion might not apply if some other boundary layer parameterization schemes are being used.

Page 21, line 17: Is the assumption of a constant 10 micrometer effective radius consistent with the assumptions that are made in the microphysical parameterization schemes? See earlier comment about consistency between microphysical and radiative schemes.

Page 21, lines 29-30: If the INP parameterization is so important, why diagnose it only from temperature if an alternate parameterization is available? It would seem that this could be a greater focus of the study if the INP parameterization is so important.

Page 22, paragraph beginning line 22: Can you show how much specific terms are contributing to the ice production processes rather than having generic descriptions of these processes?

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