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Interactive comment on "Microphysics of Summer Clouds in Central West Antarctica Simulated by Polar WRF and AMPS" *by* Keith M. Hines et al.

Anonymous Referee #1

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Summary

This is a study of Antarctic clouds, simulated in various versions of WRF, using the operational version (AMPS) with using GFS boundary conditions, and four versions of WRF v3.9.1 using ERA-Interim as boundary conditions, but with different moist physics schemes; some which are referred to as "more advanced". The model results are compared to observations from the ARM Mobile Facility deployment at the West-Antarctic Ice Sheet (WAIS) Divide. The authors suggest that biases in AMPS are related to a lack of clouds, and especially of liquid clouds, in AMPS and that the "more advanced" moist physics schemes to a large part alleviates this problem.

I must confess up front that I am quite skeptic to this type of study, where one plugs in different physics schemes without making sure that the whole model is well calibrated

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– or tuned – for each scheme. We do not learn much from such an exercise beyond the pure technical lessons and in this case I remain unconvinced; my main impression is that the lions-hare of the improvements in fact comes from changing lateral boundary conditions, from GFS to ERA-Interim. Moreover, the manu-script is not particularly well organized or well written, and the methods used to evaluate the results are not particularly exiting. Therefore, I recommend that this manuscript is rejected at this time, but encourage the authors to come back with a better prepared and more solid study.

General comments

The authors motivate this study by the need to improve models, for operational forecasting in Antarctica and for modeling the mass and energy budgets of the West-Antarctic Ice Sheet (WAIS), and putting the unique data from the AMF deployment AWARE to use, here especially at the WAIS Divide. However, these arguments pop up here and there in small pieces and it is unclear what the main underlying motivation is. For example, as far into the manuscript as in Section 3, we are told that the "primary concern" are cloud forecasts in AMPS. It shouldn't be too difficult to in a few sentences describing the background motivation and the particulars and leave it at that, without having to come back to the arguments again and again. There are also other parts of the paper that gives the impression that all the thoughts and ideas here were collected in a pile on the table and were not put together in a concise fashion.

The totality of all physics schemes in a model is a very complicated issue likely containing a multitude of compensating errors, and for an optimal model all schemes need to be tuned to each other. Replacing one scheme with a completely different one without a proper retuning will of course produce a difference, but this may be due to the creation of new differently compensating errors, or even improving results for reasons that may remain obscure. Hence, any conclusions that one scheme is better than another will be very difficult to draw from a study like this, and what might be a beneficial result might emanate from somewhere else in the model.

In the present case I'm actually unconvinced that the more modern schemes in fact does so much better than the default scheme in AMPS. AMPS is clearly an outlier in of these results, but replacing GFS with ERA-Interim and imposing the latter for a much smaller domain, as in WSM5C-run, the results become quite close to the other WRF v3.9.1 runs in everything but possibly the distribution between cloud liquid and ice. For all other parameters, I submit that the differences between the WRF v3.9.1 cloud have been removed if the different model version had been optimally tuned.

The authors make numerous statements about what biases are statistically significant or not but I wonder if the differences between the different versions of WRF v3.9.1 are in fact statistically significantly different from each other. Over-all, the error analysis is rather run-off-the-mill and unimaginative. Instead of endless tables with biases and correlation coefficients, I would have liked to see the full probability distributions of the errors and also some more imaginative error metrics. I would urge the authors to present a more process-related error analysis. It is not sufficient to plot different time series on top of each other, or even to plot time series of the errors. Instead, think about what processes are driving results for a certain variable, for example in an energy flux, and come up with ways to compare observations and the model for those parameters or sets of parameters. For example, the surface sensible heat flux is due to the surface and air temperatures (or their difference), the wind speed, and the eddy-exchange coefficient, all of which can be in error. In short, be a little imaginative. Below I will list numerous detailed comments that also need addressing.

Finally, the whole section on comparisons of cloud fractions, especially their profiles, should be revisited. This part of the study compares apples to pears; the model data should be run through a simulator that provides what the remote sensing instruments would have seen had the atmosphere looked like the model output. Time periods where the lidar was likely attenuated should be excluded completely. The model provides an instantaneous cloud fraction based on cloud condensate, while the remote sensing observations have to be averaged somehow over time. The model will give the same cloud fraction for cases with the same LWP but a certain value of LWP can come from low cloud fraction and dense clouds as well as complete cover of less dense clouds.

Detailed comments

Page 2, line 19-28: This is a very broad background, even bringing in paleoclimatology. I wonder how the present study helps solve these issues? Is WRF used for climate scenarios and sea-level rise estimates?

P3, l5-6: Here argues that Antarctic clouds are different from Arctic, and low Antarctic IN concentrations is given as an example. But isn't that true also in the Arctic?

P3, l8: Arctic cloudiness peaks in boreal summer, but does cloud thickness? It seems to me that the thickest clouds would be frontal clouds and those do not dominate the Arctic summer cloudiness; the dominating clouds in the Arctic are low mixed-phase clouds that are not very thick. Maybe a ref-erence here would be appropriate.

P3, l10-11: The anthropogenic on aerosols in the Arctic is not particularly large in summer when clouds are at maximum, at least not over the Arctic Ocean.

P4, l3: I would have thought that improvement of atmospheric models was at the fore-front of the AWARE deployment, not ice-sheet models. Did the measurements include any ice-sheet relevant parameters?

P4, 6-9: This, at the end, seems like the relevant motivation here, so it should come first.

P4, l11: However valuable the AWARE deployment is, and it was indeed, it is not "climatological"; for that the AMF should have been left there at least 10 years; more actually.

P4, l12-14: How about marking the locations on the maps in Figure 1b.

P4, l6-7: This is probably similar to what the model is also doing, so it is not a mea-surement; its comparing one model to another. Was there no direct measurement of

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sensible heat flux?

P4, l29-31: Was this done assuming there is no storage term? The surface energy does not need to be in balance; that term is misleading, and one should look at the energy budget, not the balance.

P5, l1: Do we need to know where this calculation was done?

P5, l1-5: Maybe some of this is unnecessary detail for this paper?

P5, l9-12: Here we need more detail, since this data is later used for observed cloud fraction pro-files, which are concluded to be significantly different from the model at higher altitude. What liquid water path does it take to extinguish the signal? Maybe time periods when LWP is larger than that should be excluded from the cloud analysis.

P6, l31: Maybe a reference for ASCOS?

P6, l4-5: Maybe "acute" is an overstatement?

P6, l11: 12 layers below 1km, is the kind of rather blunt information often given by modelers who do not want to disclose poor resolution close to the surface. Instead tell what the resolution is close to the surface and at what height the first model level is. That gives the reader a chance to determine if (s)he things the resolution is high enough.

P6, 19-22: Are these observations also assimilated in GFS? If so, are they effectively given double weight?

P6, l22: Reading this, what it says is that there are four forecast each day, two at 00UTC and two 12UTC.

P6, l25: Please use another word; fluctuations occur all the time, and here I assume you refer to steps that can occur every 12 hours going from one forecast to the next.

P6, l28-29: "Flights" of what?

P6, l1-2: If this is the primary goal, and I can believe it is, maybe this should have come in the introduction rather than improving ice-sheet modeling.

P6, l6-13: Here is central information about the model design that should have come up front, and not as an afterthought.

P6, l14: Using what at the lateral boundaries?

P6, l19: One golden rule in model testing is to change one thing at a time; here you use a different PBL scheme compared to AMPS. Why?

P6, l22: And what convection scheme is used in AMPS, not that I think convection is very important in Antarctica.

P6, l24-26: Is the ice fraction also different from AMPS?

P8, l14: And what is a Cooper curve? At least provide a reference.

P8, l18: As far as I understand, the ASCOS experiment was carried out in the Atlantic sector of the central Arctic; not in the eastern Arctic.

P8, l26: Awkward; cut this sentence after"... can vary."

P8, l30-31: "water-friendly and ice-friendly" is hardly the appropriate terminology.

P8, l8 - P9, l6: In this section it would be useful to have a more in-depth discussion of what physics makes these schemes different, not only what variables they carry.

P9, l14-21: This is another example where central information is buried long into the text. First discuss all the aspects that is the same for the WRF v3.9.1 simulations and then discuss the differ-ences.

P9, l21: Is nudging done in AMPS?

P9, l23-26: Actually this far in I ask myself, why include the AMPS simulations at all? The first WRF v3.9.1 simulation is done as a baseline comparison to AMPS, with similar physics and the same moist physics scheme, but different lateral boundary forcing. But there is an even larger difference here. Not only are different large scale fields used; the size of the "outer" domain is dramatically different. To really know what is what, there should have been an additional run exactly like WSM5C but with GFS imposed directly on smaller domain.

P10, l5: Actually not; there is no onset of anything demonstrated in this plot; just one static field. To show an onset requires a time line. Moreover, it is impossible to see the wind barbs in this plot.

P10, l11: The warming occurs "at" 12UTC; not after.

P10, l13: Pretty weak; "can be inferred". From what? Are there observations of some kind?

P10, l19-25: Actually the different microphysics tests are reasonably similar; the only big change is to AMPS.

P11, l1-6: There is a really strange behavior in the very lowest layer of the profiles, indicating a problem with the boundary conditions in the model. This raises the question of an analysis of e.g. 2-meter temperatures is at all meaningful. Also, here and elsewhere, there are problems with sign convention. I suggest assigning cold bias-es (e.g. line 6) negative values, not to confuse with positive biases (line 3).

P11, l19: Pretty bold statement. While I can agree that a bias on longwave radiation contributes to a bias in temperature, there are also other factors.

P11, l30-21: Sorry, but I don't understand this explanation.

P12, l14-15, There are differences, but to say that this the choice of microphysics "strongly impacts" temperature bias is an overstatement.

P12, l19-20 & l22-24: First the bias is not statistically significant and then ". . . all of these biases are statistically significant . . .". Sound like a contradiction?

P13, l29 – P14, l14: First there is a discussion on satellite data, then a discussion

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about the figure and then more discussions on satellite comparison (lines 8-14). Please organize the text better.

P14, l19-20: Is that small diurnal variation significantly different from a constant value?

P14, l26: Again, the community is moving away from the concept of a surface energy balance, because mostly there is no balance. It's a budget, and sometimes the sum of the fluxes are larger than zero and sometimes smaller. That is what makes the temperature change.

P15, l1: Is it a "bias" when you compare to a residual estimate? Maybe use "difference" instead.

P15, l19: Move ". . ., respectively" to the end of the sentence.

P16, l11: Fig. 9a?

P17, l3-4: This is not meaningful, since it is obvious the lidar do not capture any higher clouds; hence these are not "observed" by the observations.

P17, l21-22: This statement is largly unsupported.

P17, l24-33: This is just a long list of what the reader can see him/her self on the plots. What we need here is a synthesis.

P17, l29: What do you mean by "reflection"? There is no mirroring here what I can see.

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