

Interactive comment on “Microphysical sensitivity of coupled springtime Arctic stratocumulus to modelled primary ice over the ice pack, marginal ice, and ocean” by Gillian Young et al.

Anonymous Referee #3

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The paper "Microphysical sensitivity of coupled springtime Arctic stratocumulus to modelled primary ice pack, marginal ice, and ocean" by Young et al. 2016 investigates the sensitivity of three Arctic mixed-phase clouds (at different surface conditions) to primary ice concentrations for three different types of primary ice parameterisations. The simulated microphysical properties of the three case-clouds are compared to field observations. The setup of the study is interesting and provides some nice insights about the modelling of Arctic mixed-phase clouds. However, some parts of the discussion are quite circuitous and could be a bit better organised to transport the main message of the paper in a more compromised way. In total the study is suitable to be published in ACP.

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General comments:

- Some sentences of the abstract are not very clear for readers who haven't looked at the paper yet. More specifically in line 7 it is not very clear what kind of key sensitivities (of what to what) you are referring to. It also reads as if the key sensitivities emerge from the comparison to the observations, but they already emerge from the sensitivity simulations itself (or from a combination of both).

In line 11 it is not specified what kind of parameterisations you are talking about. You should add "for primary ice nucleation" (or something similar).

In line 16 you could add half a sentence what "cloud break up" is.

- The terming "primary ice crystals/primary ice number concentration" is not always consistent, e.g. on page 5, line 11 it is written "pristine ice crystals". It is also not clear how different the terming is meant in the different parameterisation schemes. In DeMott et al. 2010 the parameterisation scheme refers to INP, but INP translate directly to primary ice crystals (the way they mean it) and therefore the parameterisation schemes should technically be the same. If that is not the case, a better explanation (also of how it is differently implemented in the model) would be needed.

The terming "primary ice nucleation" does sound tautologous (nucleation always leads to primary ice).

- It would also be interesting to add a simulation where the different freezing pathways (immersion freezing and deposition nucleation) can compete and look at the importance of the different pathways acting in different S/T-regimes. I was missing this discussion or the discussion of this aspect in the manuscript. Of course it is interesting to look at the resulting ice crystal concentrations of both pathways separately but in reality both pathways could take place in parallel or rather compete with each other depending on the environmental conditions.

- The result that ACC provides the best agreement with the observations (e.g. page

11, line 3) is trivial since the parameterisation is based on the observations. However, it is interesting to have an empirical parameterisation in the comparison, but maybe a bit more critical discussion on this could be added.

- The first part of the paper is quite lengthy, especially the comparison of every single case. Some plots do not seem very interesting at the first point but are very interesting later in the discussion when you explain some of the details behind some features (e.g. Fig. 8). I also had the feeling that later in the discussion many things are repeated (e.g. in section 5.4.3). The discussion itself was (just looking at the headers of the sections) not very intuitively organised, it is quite difficult to see where this now leads to/what the main points are/will be (before reaching the conclusions). It could help to restructure the paper/think again how to organise it so that the focus is clear and the paper interesting to read without losing interest in the first part.

I had the feeling that most of the description on page 22 and also section 5.4.3 could already be moved to the case description and make this part more interesting to read. Also the comparison of the cases within each other came quite late (before they seemed to be quite isolated in the analysis).

However, this point might be subjective and a matter of taste.

Specific comments:

- Page 2, line 15: Add which kind of parameterisations you are talking about.
- Page 2, line 20: You should add in brackets the name of the four ice nucleation modes you talk about. You could also add a bit more explanation (or more structured explanation) about the different ice nucleation modes. You mention that partly later, but maybe it would help the inexperienced reader if you have a short explanation first before you elaborate the pathways and their representation in models in detail.
- Page 2, line 20: In most models not all four ice nucleation modes are represented. It is a bit miss leading to say "commonly represented". Many models only treat immersion

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freezing, on the contrary contact freezing is very rarely explicitly modelled.

- Page 2, line 26: Your example does not fit to the argumentation before (referring to deposition nucleation instead of immersion freezing).

- Page 2, line 28: You write that deposition nucleation and condensation freezing are experimentally difficult to distinguish but most instrument have rather difficulties to distinguish immersion and condensation freezing since in both cases the liquid phase is involved. Deposition nucleation takes place at a different saturation ratio/temperature regime compared to condensation freezing. Many models use immersion freezing as a surrogate for immersion and condensation freezing. I was surprised to read that deposition nucleation should be often related to condensation-freezing- are you referring to pore-condensation? Maybe this issue is related to the definition used of the nucleation modes and more a matter of phrasing/language but it could be confusing to other readers as well, so it might be better to add more explanation.

- Page 2, line 10: What do you mean when you say "ice number concentrations will be suppressed under this conditions"? So deposition nucleation is also only allowed to take place at water saturation? That does not make sense physically.

- Page 4, line 20: What is a sub-Arctic McClatchy profile? Either explain or generalise?

- Page 4, section 2.2: What is the time step of the simulations? 150 seconds?

- Page 5, line 3-7: You can skip this explanation since you switch of Bigg 1953 and Meyers 1992 in the final simulations. That is confusing (especially when readers watch out for B53 and M92).

- Page 6, line 11: It is good that you point out the limitations of this study. However, it would also be nice to add how realistic this idealistic study is and under which conditions you would have a similar system in reality.

- Why did you only add a ice crystal number concentration sensitivity for DeMott et al. 2010? Is there a reason you picked this parameterisation and did not add it for all of

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them (computational costs?)?

- Fig. 1: You should plot the parameterisation only in the temperature range where they are valid or make them transparent in the temperature regime beyond their validity. Or do you extrapolate the parameterisations schemes over the whole temperature range in your model setup (then I miss interpreted it wrong before)?

- Fig. 1: Instead of having three lines for D10 and the corresponding variations, you could only plot D10 and add a shaded area around the line.

- Fig. 1: The D10 Fit is not needed. You also do not really discuss it in detail later.

- Fig. 1: The D10x0.5 line is not really needed here, you already describe it later (and no visualisation is necessary). However, of course you can keep it, it would just make the figure a bit less busy.

- Fig. 2: You could increase the figure to enable better readability.

- Fig. 2: Remove the doubled red altitude axis on the right (or colour it black), that is misleading.

- Fig. 2: It would be useful to add the cloud extent in the figure.

- Fig. 2: Would you still need the grey boxes when you add the cloud extent to the figure (since the altitudes without sampling seem to be always below cloud)?

- Fig. 2: Would it be possible to use the same scale for all cases?

- Page 10, line 14 and line 17: At which altitudes are the ice crystal concentrations estimated?

- Fig. 5: You could increase the figure to enable better readability.

- Fig. 5 a(ii): The scale is not reasonable (there should not be negative Q_{liq}).

- Page 12, line 6: Can you further explain this?

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- Page 12, line 8/Figure 8: Again is the D10 Fit really needed here?
- Page 12, line 14: Why does IWP decrease subsequently?
 - By how much is $N_{\text{ice} > 100 \text{ }\mu\text{m}}$ still clearly related to the ice nucleation parameterisations in your model? Which other processes might influence this variable? Is it fair to compare this variable among the different parameterisations (since that is not the size of primary ice formation)?
- Fig. 6: Would it be possible to use the same scale for all cases?
- Fig. 7: It is very difficult to compare the single lines. It would help to increase the size of the figure. It would also be possible to cut Fig. 7 a, b, d, e, g, h to the relevant altitudes (leaving away everything above 1000 m).
- Fig. 7: It is unclear over which time span the mean was taken and why. What was the availability of the observations? Did you choose to calculate the mean as described to temporally collocate the data?
- Page 15, line 3: Why do the glaciation events take place every 3h? What is driving that?
- Page 15, line 13 + page 15, line 17: You do not really mention or explain the spikes here which is a bit irritating. You also do not explain (here) why only D10 has these glaciation peaks. It is also unclear (here) why in case of C86 the glaciation leads to a decrease of IWP and not an increase. You could think of reorganising your paper so that you add already part of the discussion here.
- Page 15, line 22: Explain what W is.
- Page 15, line 33: The unit for the precipitation is a bit confusing here (is clear in Fig. 11).
- Fig. 8: Was more interesting later for the discussion but at this part of the paper it does not seem so interesting, you might want to shift either the figure or the discussion

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(see general remarks).

- Fig. 9: The features (peaks) are not really clear until the discussion.
- Fig. 9: You could add the periods at the time scale when the cloud had a mixed-phase structure/when there was a cloud.
- Fig. 10: The differences of the last three lines did not get very clear before the discussion also includes the precipitation (Fig. 11)- it could help to reorganise the discussion here.
- Fig. 11: It would be an interesting information to also add the total amount of precipitation for all the cases (mm/m^2).
- Page 21, line 2: You could elaborate here why it is so different in case 3. What is different in that case?
- Page 21, line 14: It is good that the modelled N_{ice} is in reasonable agreement. However, if that is due to colder temperatures than observed, it would indirectly mean that the temperature dependence of the parameterisation schemes used (or the temperature regime where they are efficient) is not correct. You could add some critical thoughts about this issue.
- Page 23, line 12: I did not understand what you meant by "sweet spot" here when you mentioned it the first time. However, it was clear later on.
- Page 25, line 5: Add the order of magnitude.
- Page 26, line 2-4: Could the less pronounced difference in case 2 (compared to 1 and 3) be a result of the higher cloud top temperature and the onset temperature of freezing? It would be an interesting aspect to add to the discussion.

Technical corrections:

- Page 1, line 12: The Cooper (1986) parameterisation...

- The ice nucleation pathway deposition nucleation is commonly not named deposition freezing, since it does not involve the liquid phase (and freezing refers to the liquid phase).
- Page 3, line 14: Replace guide by guidance.
- Page 3, line 32: You could exchange the second with the first sentence.
- Page 4, line 18: The numbering of the cases is wrong in this case (here it reads as if the ocean case is number 2 and the marginal ice zone case is number 3). You should check if the case-numbering is consistent everywhere.
- Page 5, line 2: You write that you vary the form of the deposition-condensation freezing parameterisation but you only use C86 and compare it to D10, which refers to immersion freezing. You should therefore change this here to prevent confusion.
- The equations have the unit and the dependence in the same bracket, which is a bit strange. It would be more correct to write the unit and the dependence in separate bracket, e.g. $N_{\text{ice}}(T_k) [\text{m}^{-3}]$.
- Page 5, line 19: It would read better if you have the text first and then the formula.
- What does the Index k means for the temperature? Why do you not write T?
- Page 7, line 5: Replace These by The (otherwise the reference is missing).
- Page 7, line 12: Skip "over the".
- Page 9, caption Fig. 4: Replace mixed-phase cloud by mixed-phase clouds.
- The numbering/organisation of the figures is not always consistent (e.g. Fig. 7 does not use i/ii for the columns). It would be great to have the explanation of the numbering of the different columns as in the caption of Fig. 10 in the caption of Fig. 1 (you do not need it then in Fig. 10). In Fig. 12 you use the labels i/ii in a different way then before.
- Page 20, line 11: Replace , before Mason by ;.

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- Page 20, line 30: More accurately would be mixed-phase formation phase instead of formation phase.

- Page 24, line 30: Move also in between are and not.

- Page 26, line 7: Replace microphysical structure by microphysical structure of MPS.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-898, 2016.

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