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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Arctic cloud annual cycle biases in climate models" *by* Patrick C. Taylor et al.

Anonymous Referee #1

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Review of "Arctic cloud annual cycle biases in climate models" by Taylor et al.

This manuscript analyzes climate model simulations of Arctic clouds, to understand the seasonal cycle and what drives it. It provides some interesting analysis, by grouping models by their seasonal cycle. The paper may be suitable for publication in ACP subject to major revisions.

I have a few general concerns:

1. The authors need to show that the results are robust to changes in model groups. Perhaps 1/3 of the models are very close to a 1:1 line they use to select models. What happens if you change the grouping of models? Does it change the results?

2. The authors claim that since their results agree with earlier work, it is fine to use

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monthly data. That is not sufficient. They are averaging over regimes that may yield very different results, and they need to verify with a single model perhaps that monthly data for joint PDFs for example matches high frequency (daily or higher) data.

3. The lack of ice fraction is limiting. Analysis shows ice and liquid, with no sense of what the fraction of ice is. This is related to #2 above.

4. The authors need to document models better. There needs to be a table of models with references.

5. In addition, it would be particularly useful to group those models which have ice supersaturation and look at their results.

6. There is minimal use of observations and comparison with observations in this work. It is hard to tell what is right, would like to see more comparisons against observations, and discussion and conclusions which focus on comparisons with observations. Which group isomer like observations?

Specific comments:

Page 1, L16: model group is a strange term.

Page 1, L18: thermodynamic characteristics is ambiguous.

Page 1, L19: I assume more means Higher fraction?

Page 1, L21: can you separate ice nucleation mechanisms or are you speculating? Most models do not do anything for ice nucleation and just have partition.

Page 1, L23: can you say anything about the bias and what the ice partitioning should be?

Page 2, L57: do you include/account for this bias in your analysis?

Page 2, L72: what does ice formation do to cloud amount? It's implied here but this might need another sentence of explanation.

Page 3, L77: but observations may be uncertain as well. Probably by more than 15%.

Page 3, L103: can you actually determine mechanisms from monthly mean data?

Page 4, L112: use of monthly means does not sound like an improvement.

Page 4, L113: I am skeptical of a monthly mean joint distribution. How do you deal with averaging over different regimes in a month?

Page 4, L119: you have not described the models. Where does that happen?

Page 4, L124: there are maybe 10 models that are very close to the 1:1 line. How do results change if you shift the cloud amount by +/-5% and redistribute models.

Page 4, L125: the C3M Observations are quite close to the 1:1 line. Is there really a defined maximum in winter? You might need to be more careful on the classification here, since the annual cycle may not be so much winter-summer as peaking in fall.

Page 4, L130: I guess one concern I have is that you have not addressed what a cloud is, and what a threshold water content is. How are you sure you are comparing apples with apples between the models and observations?

Page 4, L132: before a results section, you need to describe the models in a paragraph, along with a table listing the models and appropriate reference for each one.

Page 5, L153: why neglect Fall? The peak low cloud amount is in fall, not in winter.

Page 5, L164: I'm not sure I would say that the low cloud differences are spatially uniform. Differences seem lower over open water than sea ice for example, and largest differences are over land.

Page 6, L191: shouldn't you do this by season (winter-summer) or at least comment on differences between winter and summer PDFs. Maybe show a sub set?

Page 6, L198: since these are monthly means, how do you account for averaging across regimes. How do you know you are really seeing cloud influencing factors?

Page 6, L215: why are there vertical stripes here? Is this one model? Does it represent anything physical?

Page 7, L238: what does 'thin' mean?

Page 8, L277: do you think you could do this as well with sub monthly data? That might be a check that you are not a averaging over meteorological regimes (I.e. Morrison et al 2011) within a month.

Page 9, L307: what do you mean by cloud microphysics here? Besides liquid and ice, you don't have any information on microphysics (size, number, precip, optical depth, etc).

Page 9, L313: but the formation of ice would lead to more rapid depletion of cloud condensate than liquid. Rather than monthly amount, you might need fraction of Ice present, but you cannot get that from monthly means I think.

Page 9, L315: which models? Is there something systematic in models that allow supersaturation over ice? This would be an important statement.

Page 9, L318: what are the summer temperatures? Are they greater than freezing? Might just be a Clausius clayperon effect of more water at higher temperatures, not an ice formation issue.

Page 9, L321: I'm not sure you have justified this argument. What shows that? There are also significant shifts in LTS in the models.

Page 10, L346: I think the statement that monthly means are okay needs more analysis. Just because the answer looks similar in a different analysis is not sufficient.

Page 10, L356: I'm not sure that makes sense. The averaging across regimes might be a problem here. Also, amount of ice may not be as important as fraction of ice.

Page 11, L379: what models in your list support ice supersaturation?

Page 12, L435: but you have not commented on liquid production and availability, and ice fraction. That is the key issue.

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