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Geoscientific Model Development Discussions

Interactive comment on "Snowfall distribution and its response to the Arctic Oscillation: An evaluation of HighResMIP models in the Arctic using CPR/CloudSat observations" *by* Manu Anna Thomas et al.

Anonymous Referee #2

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This paper presents an evaluation of a few climate models within the HiResMIP project in their representation of Arctic snowfall. The authors show that the models show significant biases compared to the CloudSat derived snowfall rates, and that there is no clear improvement in the higher-resolution models compared to low-resolution models. While potentially interesting, this paper lacks a lot of details on the observational uncertainty, model choice, statistical robustness, results analysis, and discussion. The authors are invited to expand on the initial analysis and consider the points below in that process. I focus on the main points first, after which I highlight some more minor

points.

Major comments

1. The observational uncertainty is very poorly quantified. CloudSat suffers from main issues with undersampling light precipitation events (as the authors indicate), but no uncertainty is assigned to that. I wonder why the authors did not consider working with other CloudSat data (including internal quality flags of the CloudSat algorithm, and/or radar-derived reflectivity).

2. Statistical robustness and process analysis. If working with percentiles, while only considering <10 years of data (so only a sample of <30 months for each season), I wonder how significant these percentiles are. In addition, the authors should be extremely careful in translating a low observed monthly snowfall rate to an inadequate treatment of light snowfall by the models; it might very well be that this monthly snowfall rate comes from just one large snowfall event! Although I realize that CloudSat frequency is only monthly for this gridded product, this connection from observation to model physical processes should be much more expanded upon; for example, the authors can look into the distribution of snowfall in the models in those low-snowfall months, to confirm whether or not these are associated with just low-snowfall events or not. Without such a detailed analysis, the reader remains 'in the dark' about the exact defiency in the models. Note that recent literature suggests that models produce 'too little, too frequent' snowfall (e.g. https://journals.ametsoc.org/doi/pdf/10.1175/JCLI-D-16-0666.1); does that apply do these models as well?

3. Choice of models: why were only these models chosen? Why only ocean-forced models and not fully-coupled models? Why is opted to not use a COSP-style radar simulator in the models? How does that impact the results? More details on the PRIMAVERA project are necessary. In any case, if only AMIP-style models are chosen, the authors should use the overlapping period between observations and models only.

4. Methods, results, and discussion are mixed throughout the paper, which is some-

what confusing for the reader. A conclusion section with a long list reads like notes from a presentation, and should be converted a flowing text instead. More discussion should be added; why are these results important? How do these compare to other literature? What can we learn from it? What processes are lacking and/or inadequate in models?

Minor comments (P=page, L=line)

P1, L2: surface radiation budget

P1, L20: How would incoming radiation cool the surface?

P2, L34: irrelevant for this paper; Alpine snowfall is much more related to mid-latitude atmospheric dynamics, as well as orographic snowfall.

P3, L10: What is this project and what is its aim, and how does this paper fit in that?

P3, L15: what is a snowfall flux? Mass or energy?

P3, L21: later, you discuss results from 2006-2015. Try to be clear

P3, L27: possibly?

P4, L19: how do you end up with ten seasons? Clarify.

P4, L20 to P5, L8: these are methods, not results. Consider changing your section titles to improve structure (Section 2: Data and Methods; Section 3: Results; Section 4: Discussion and Conclusions)

P5, L16: larger difference

P5, L29: extent

P5, L31: what does ‘negatively skewed’ mean?

P6, L9 and beyond: This belongs to the discussion section

Figures: too small; add units and indicate seasons in Figure itself. Add sublabels (e.g.

a, b, etc) to refer to in text.

P10, L32: this is the main issue of this paper; it should be added to the discussion section and reflected/expanded upon – see Main Issue 2

P12, L5: this is part of Methods, and does not fit here

P15, L1-13: Methods.

Section 6: This has not been clearly introduced in the intro and feels obsolete. Why would this be relevant to this paper? Why not also look at impact of NAO, Arctic sea ice extent, GBI, and other indices?

P17, L4: I would argue that this is definitely not ‘a wide range of models’

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