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Interactive comment on "Snow on Arctic sea ice: model representation and last decade changes" *by* K. Castro-Morales et al.

Anonymous Referee #2

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The present paper by Castro-Morales et al. provides an analysis of the last decade changes in the Arctic sea ice snow cover through a model simulation. The model used is the MITgcm model including a multi-category sea ice model and a conventional single-layer snow scheme. As in many ocean-sea ice coupled models, the snow scheme is relatively simple (constant thermal and physical properties for snow), but it has the peculiarity of distributing the total snow mass accordingly with the ice thickness distribution (ITD), although this is not specifically described in the paper. The methodology of the authors is to evaluate the snow depth simulated by the model by comparing them with NASA's Operation IceBridge (OIB) data product (radar observations from planes). Then they use the model to simulate and study the changes in snow depth on Arctic sea ice over 2000-2013. The topics and goals addressed in this manuscript

are very relevant to the journal (TC) and very interesting with respect to the current state of snow representation in sea ice models. In particular, tackling this issue with an eye toward the trends in snow accumulation on sea ice is I think something quite new. Thus, I encourage the authors to push the effort forward for publication. This being said, I also believe the paper suffers from a few strong weaknesses and inconstancies that should be addressed before it can be published. I raise below a few major issues and some specific comments in order to revise the paper.

General issues:

- Originality/novelty of the study through its goals. The authors refer to papers like Blazey et al. (2013) and Lecomte et al. (2013) which share common goals with the present manuscript. What is new and original compared to such studies should be made clear. One of the things that should be put forward is that trends in snow are studied. That is a true novelty. However, this must go along with a proper assessment of the trends that is, in my opinion, not performed in the study. For details on this, please read on below.

- Problem of trend analysis. Based on the work that has already been performed by the authors, results could be presented in a far more convincing way. Instead of looking at the differences in the 2000 and 2013 snow accumulation with respect to the 2000-2013 mean, they could more robustly present and analyse maps of snow mass or volume trends. In addition, they show where the changes in snow have been the largest regionally, but they do not explain why. This latter problem is related to the snow mass budget. The relative contribution of each term in the budget is given on average over the period of analysis, but the trends, the changes in each of those terms along the 2000-2013 period are not presented. The yearly time series of these terms are presented in a table but not properly discussed while it seems to be the main focus of the paper. The consequence is that conclusions drawn from the authors on the reasons explaining the past decade changes in snow sometimes seem to come from speculation. Finally, potential trends on snowfall are not discussed, which is strange for a paper on trends on snow accumulation on sea ice.

- Snow thermal conductivity experiment. If the authors are willing to keep the results from this experiment in their manuscript, they have to justify it better and discuss the results more accurately. As it is presented currently, it seems to come from nowhere. Similar experiments have been done in older studies, and I do not understand well why it is done here in a context where trends on snow accumulation are the focus. Even more importantly, the results, i.e. virtually no sensitivity of the model to snow thermal conductivity, is very surprising. ks is known to be a critical parameter for the seasonal to inter-annual evolution of sea ice in the Arctic, both from the observational and modelling points of view. This lack in sensitivity is very unexpected and hence needs to be well explained. Unfortunately this is unpublished data (that I could provide internally if really required) but there has been in the past such sensitivity studies performed with different models (with a single snow layer, like in this study) and they consistently showed a high sensitivity in the Arctic. As I was writing this review, I started thinking about this problem and something came to my mind. As explained below (please see specific comment on this) the competing processes of 1. weaker snow insulation on thin ice (due to snow accumulation limited and prescribed as a function of the ITD) and 2. higher insulation by reducing the value of ks, may compensate each other and be a reason for this. A way to test this would be to run again this sensitivity experiment but without using the snow distribution scheme introduced in Castro-Morales (2014).

- Analysis as a function of latitude. I do not really get the point of doing this. Why not an analysis as a function of the ice "properties", like FYI vs. MYI, ITD [...]?

- Issue with snow redistribution by the wind. Throughout the paper, the authors mention the potential role played by the blowing snow process. However, they refer to this process in an approximate and confusing way. Things like why the redistribution of snow (over sea ice) would impact the snow mass balance at the large scale, or why redistribution would contribute to the sources of snow accumulation should be explained. In my opinion, what matters for the first of those questions is how much snow is lost in

leads (and this is distinct from redistribution on the ice) and, for the second, it is simply wrong.

Specific comments:

Title

In my opinion, the title is broader than the paper content actually is. It makes the reader expect a review paper on the modelling of snow on sea ice while it is not. I think the "model representation" part is the problem. Something like: "Snow on Arctic sea ice: last decade changes from a model simulation" would be more appropriate.

Abstract

- L14: hs should be hs_mod

- L16: "year-to-year loss of sea ice" This is not shown in the paper since only two particular years (2000 and 2013) are compared to the 2000-2013 mean. In general, why was this method used? Why not working with the proper trends from the model. That would provide way more convincing information.

- Last sentence (L23-26): This statement seems stronger than what really stands out from the study. In the discussion, I understand that the redistribution process could play a role in explaining the difference between the observed and simulated thinning of the snow pack, but not a major one. Besides, as explained later in this review, I think it is more specifically the loss-into-leads component of the redistribution process that comes into play here, instead of the redistribution over sea ice itself. Also, for information, the authors refer to Lecomte et al. (2013), but in later studies they actually included a parameterization for blowing snow in their model:

o Olivier Lecomte, Thierry Fichefet, Daniela Flocco, David Schroeder, Martin Vancoppenolle, Interactions between wind-blown snow redistribution and melt ponds in a coupled ocean–sea ice model, Ocean Modelling, Volume 87, March 2015, Pages 67-80, ISSN 1463-5003, http://dx.doi.org/10.1016/j.ocemod.2014.12.003. o Olivier

Lecomte, Thierry Fichefet, François Massonnet, Martin Vancoppenolle, Benefits from representing snow properties and related processes in coupled ocean–sea ice models, Ocean Modelling, Volume 87, March 2015, Pages 81-85, ISSN 1463-5003, http://dx.doi.org/10.1016/j.ocemod.2014.11.005.

Introduction

- P5683; L2: comma after “e.g.” + “metamorphic” is not really appropriate. “Metamorphic processes” would be fine, but not “properties”. Simply use “physical properties”.

- P5683; L3: comma after “e.g.” + reword “thermal conductivity almost an order of magnitude less than the one of the ice”

- P5683; L4: I suggest removing “in high latitudes”

- P5683; L8-9: What do the authors mean? This should be rephrased/extended.

- P5683; L10-11: remove “its” + add comma after “thickness” and “(Stroeve et al., 2012, 2014)”. The paragraph is a bit strange. I do not really see the connection between the first sentence (about the trend in Arctic sea ice extent) and the second one (ice-albedo feedback description). Also, this whole paragraph is related to trends while this is properly addressed only later at the end of P5684.

- P5683; L17: replace “thickness” by “depth”

- P5684; L2: “up to date” is misleading. It can be understood as “accurate” or “most recently updated” and it is not the case

- P5684; L14: remove comma after “scales”

- P5684; paragraph L22-26: All right, but OIB data still suffer from (very) large uncertainties. That should be clearly mentioned.

- P5686; L1: here, and everywhere else in the manuscript, “validated” should be replaced by “evaluated”. Although this word has been used many times in the literature

in the past, we have to stop doing so. A model can never be “validated”, it may only be evaluated as a proper tool for simulating a set of specific physical processes. In addition, observations also have uncertainties that are sometimes just as large as the models’ ones. So “validate” should definitely not be used.

- P5687; L11-13: This does not seem accurate. The authors show on average how much of each term in their mass budget contributes to the yearly accumulation of snow on Arctic sea ice. However, it is not shown how those relative contributions have changed inter-annually. In other words, trends in the terms of the mass budget are not presented, so what is announced here is not really what is provided in the paper.

Methods

- P5688; L6-7: very unclear statement “accumulates to its mean thickness”

- P5688; L9-10: Could you please add references and justification for using those albedo values

- P5688; L10-11: Also unclear statement. Please rephrase or be more explicit.

- P5688; L15: “on” or “for Antarctic sea ice” and “heavier loads of SNOW than ON Arctic…”

- P5688; L17: 15cm seems a very large (too large?) value for this critical depth. May the authors justify or add a reference for choosing it? It requires way less than 15cm of snow to make the surface albedo completely independent from the ice underneath. Is it to account for horizontal heterogeneity?

- P5689; L19: remove Linday et al. (2014) reference, it is already given at the beginning of the sentence.

- P5691; L16: There is a couple of sentences like this in the paper, I suggest it would be better written as “Laser altimetry data from the Airborne Topographic Mapper (ATM) (Krabill et al., 1995) are used to retrieve snow freeboard”.

- P5692; Eq 1: The formalism of this equation should be revised. It is not consistent to have dhs/dt on the one hand, thus a trend or a rate of change, and “hs” on the other hand that is the same thing without dividing by dt.

- P5692; L11: space in “ERA Interim”

- P5692; L15: about the third term on the right hand side of eq. 1. I cannot understand what the authors mean. This, as it is written, simply makes no sense.

- P5692; L25: “snow formed at the surface of the sea ice due to rainfall”?? Same comment. Are the authors referring to the refreezing of liquid water? Rain does certainly not turn into snow once on the ice.

- P5692; L26: Something should be clarified here about the snow redistribution. The loss of snow into leads is clearly a sink term for snow accumulation. Redistribution over the ice however, as it says, is just a redistribution. It may change the snow depth distributions with respect to the ITD (and even this is not clearly established), but on average over a model grid cell I am not convinced it would change the accumulation significantly. It may have an indirect impact, through the interplay between ice thinning and opening but it is then again related to losses in leads. When the snow is redistributed by the wind, it is not removed.

- P5693; L2: Contradiction with what is said later

Results

- First paragraph of the result section: Given the numbers provided L16 and 18, “little difference” (L13) could be replaced by “almost no difference”. At the scale at which the snowpack is considered here in the model, this difference is insignificant, and it actually worries me a lot, given that snow thermal conductivity is usually in the top three parameters ocean-sea ice coupled models are sensitive to (in the Arctic). It is hard to understand how reducing snow thermal conductivity to half its initial value does not change the sea ice mass balance.

- P5694; L20: “been” => “being” + “Despite” => given the rest of the sentence, why state “despite”? Are there reasons to expect a smaller inter-annual variability?

- P5694; L26: “insight in” => “insight on”

- P5694; L27-28: “likely related to the history of sea-ice thickness”: vague statement.

- P5697: Why not just showing the trends instead of comparing two single years to the 2000-2013 mean value? I do not really doubt there is a trend in the model, but one can clearly imagine a time series of snow accumulation having no trend and still the same differences with the mean value as those described by the authors.

- P5698; L10-11: see previous comments above, “formation of snow from liquid precipitation” (this, to my knowledge, does not exist in the real world) and redistribution.

- P5698; L13-14: I do not understand, and it contradicts what was said earlier in the methods...

Discussion

- P5699; L11-14: Although the Castro-Morales et al. (2014) reference is given, the way the snow accumulation is prescribed as a function of the ITD should be described/discussed in more details here.

- P5699; L21: “as a general rule...” No. This can definitely not be said. I get the point made by the authors but it cannot be stated this way because the intimate relationship between snow and sea ice is very complex. See, for instance, Sturm and Massom (2010) cited in the paper. What is said in this second paragraph of the discussions may be true for Antarctic sea ice (because the hs/hi ratio is driven by high precipitations and the hydrostatic balance of the snow/sea ice column, hence larger hs for larger hi) and Arctic FYI (time of accumulation indeed plays a significant role on conditioning the total accumulation). However, besides the exception mentioned by the authors about deformed ice (which is fact not really an exception since sea ice is very often deformed), there is also the problem of MYI. By definition MYI is present at the end of the melting season, hence present for the whole following accumulation period. In the Arctic, the snow cover almost entirely melts away every year, so the age of the ice is not directly related to the age and properties of the snow. In this case, the precipitation rates, onsets of accumulation and melting seasons are more important.

- P5700; L3-4: I do not really see how the presented results support this. First, if the authors want to justify the use of such a scheme, they would specifically have to show the snow depth distributions in relation to the ITD, and show that results are improved with regard to a standard case where the snow is not distributed according to the ITD. This, is not done. Second, there is the above discussion about the fact that snow depth is not, as a general rule, proportional to the ice thickness. And last, I guess that if such a snow distribution scheme is used, it is to better simulate the snow depth distributions, themselves partly driven by blowing snow. In other words, I see the use of such a scheme as an attempt to implicitly account for snow redistribution by the wind by prescribing the snow depth distribution as a function of the ITD. That is ok with me, but then the authors say that the blowing snow process should be included in the model without saying that the initial distributions of snow are wrong. This is a contradiction. If the conclusion is that the redistribution by the wind process is missing in the model, the way the snow is distributed on the ice should be questioned by showing for instance wrong depth distributions (I am not saying it is the case though, because it is not shown). If the authors refer to the loss of snow into leads, by using the general "wind redistribution" term, as a missing sink of snow in the model, then ok but it should be made explicit.

- P5701: Discussion about the snow thermal conductivity experiment. Once again, this looked VERY surprising to me so I thought of it and something that may actually explain this came to my mind. First, I am not really convinced that the reasons provided by the authors really explain this lack of sensitivity. They make the comparison with Lecomte et al. (2013) and mention that the difference in sensitivity may be because they do not have a multi-layer model. Indeed, this may come into play to some extent,

but here their model has simply no sensitivity to snow thermal conductivity, which is really extreme. Basically, the thicker the ice, the smaller the conductive heat fluxes through it and the smaller the ice growth rate. So, given this, the relative importance of snow thermal conductivity on the sea ice growth rate is larger for thinner ice. However, the authors explain that in their snow distribution scheme, snow is prescribed to be thinner on thinner ice, which also has a thermal impact on the ice growth (i.e., thinner snow has a weaker insulating effect). Thus, the effect of reducing the snow thermal conductivity (increasing its insulating power) may be lessened/compensated by the fact that accumulation on thin ice is limited in the model. I am not sure this is the explanation, but it could contribute. If so, it would be good to show it. Ultimately, is this physically realistic?

- P5702; L27: No, the long-term trends are not evaluated. Quantitatively speaking, only 2000 and 2013 are compared to the 2000-2013 mean.

- P5703; L15-19: This again looks a little farfetched. There is no mention at all of snowfall. The regional distribution of snow is not the sole indicator of sea ice conditions prior to the growth season. Precipitations matter a lot. As for saying that there has been no major change in accumulation over the period of interest, it may be true, but it is surprising the snowfall trends are not discussed.

- P5704; L26-27: Again, I do not understand this.

Conclusion

- P5705-5706; L25-4: It may have been shown in the previous paper, but it is not properly done here.

- In general, this conclusion is really short and does not summarize/conclude on the main findings of the paper. Most importantly, it does not answer to the objectives set in the introduction.

References

- The Castro-Morales (2014) reference is missing. Please add it and check that no other references is missing.

Tables and figures

- Table 2: Once again, I do not understand the way hs(sources) is computed. I can only see the snowfall as a source for snow accumulation. Besides, there are rows in the table for which the sixth column + 9.4 is not equal to the last column. It is probably simply a rounding error or something like this but it should be fixed. Lastly, Why not showing those time series in plots, it would be so much easier to visualize, and trends could be added on every time series. . .

- Figure 4. (a) : How come there is negative accumulation at the very beginning and end of the time series??

Interactive comment on The Cryosphere Discuss., 9, 5681, 2015.