The Cryosphere Discuss., 9, C2210–C2220, 2015
www.the-cryosphere-discuss.net/9/C2210/2015/

The Cryosphere
Discussions
Open Access

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

Anonymous Referee #1

Received and published: 13 November 2015

The manuscript of Castro-Morales et al. (tcd-9-5681-2015) aims to analyze Arctic snow-depth-on-sea-ice simulations for the period 2000–2013. Simulations are made with the MITgcm ocean model which includes a sea ice model. Snow is represented as a single layer with limited parameterizations (e.g. for albedo, snow to ice formation). Multiple snow properties (e.g. density) are thus kept constant. During the period of interest, MITgcm is forced by ECMWF ERA-interim reanalysis. First, simulated snow depth is evaluated against NASA's Operation IceBridge (OIB) airborne radar derived snow depth in early spring (March–April). The authors claim a good agreement between simulated and OIB snow depths, as well as a comparable latitudinal variability. However, appreciable snow depth differences (larger than ±20 cm) may exist within 28-km grid cells, and a clear bias as OIB snow depth increases is present (fig. 2) though

not discussed. Second, a brief comparison of simulated and retrieved ratios of snow depth/ice thickness is presented. This comparison points out inconsistencies with the simulated sea ice thicknesses, and weak latitudinal variations compared to OIB data. Third, year-to-year snow depth changes over the 14-year period is presented, and some links with the sea ice extent is made.

Arctic snow on sea ice is a key element of both the Arctic system and ocean-sea ice-atmosphere interface. The topic addressed in this manuscript (with models and remote sensing data) is important, of interest to the community, and suited for The Cryosphere. Moreover, the approach considered by the authors to gain knowledge on snow depth on sea ice is interesting. However, both the analysis and the manuscript require further work before being published in The Cryosphere. Based for instance on inconsistencies between statements and figures, likely erroneous values in table 2, affirmative/conclusive statements written in the manuscript but not demonstrated, and missing reference in the reference list, I cannot encourage publication yet. In my view, this work should be reconsidered after noticeable improvements.

Main comments p. 5682, l. 16-17. There is no element in the manuscript proving that "year-to-year loss of sea ice" is well reproduced by the model. The authors cannot claim good results in the abstract when there is no mention of it in the manuscript. Furthermore, according to fig. 3b, sea ice thickness is not well reproduced by the model. The authors must support their statements with material and clarify whether the model with sea ice thickness latitudinal bias get the annual sea ice extent right for the right reasons (physical processes).

p. 5682, l. 20. "[...] the long-term reduction in the summer sea-ice extent ultimately affects the maximum spring accumulation of snow." I would argue that sea ice formation (freeze-up) date is a more relevant indicator that can be correlated with maximum spring accumulation of snow. A fall with a very low sea ice extent but an early freeze-up date may lead to deeper snow than usual, because snow could accumulate earlier. In the context of this study (and abstract), do the authors mean that the summer sea ice extent matter more that the sea ice formation (freeze-up) date for snow accumulation?

p. 5682, l. 23. "The model results exhibit a last decade thinning of the snowpack that is however one order of magnitude lower than previous estimates based on radar measurements." Were the results truly compared to at least 10 years of radar snow depth on sea ice retrievals? OIB only started in 2009 and the authors stopped their study in 2013. This corresponds to 5 years (half a decade). Studies of changes in snow depth before 2009 rely on drifting buoy data (not radar data; e.g. Webster et al. 2014 cited by the authors). In both the abstract and the manuscript, too often I was challenged to distinguish whether the statements were erroneous because the authors did not pay sufficient attention, or if the statements were purposely written to overstate results.

p. 5688, l. 1-8. This paragraph would benefit from additional information about ice thickness distribution (ITD), ice categories, and their relationships with snow depth statistics. It cannot be easily understood without reading Castro-Morales et al. (2014). Moreover, the simulated snow depth strongly depends on the prescribed ice categories. Therefore, in addition to new text, I also think a table with the possible amount of snow per ice category, and a map of the ice categories in the Arctic basin will be useful.

p. 5688, l. 2. Add the time period covered by these historical ice thickness distributions (ITD). Also, I think it is important to mention the adjustments made to the IDT, if any. For example, it is stated that the same ITDs as in Castro-Morales et al. (2014) are used. Therefore, it should be added that these ITDs were adjusted so that their mean value is 1 m. A comment about the implication of such adjustment may be appropriate as it directly impacts the prescribed thickness.

p. 5688, l. 7. The following statement is very misleading: "[...], the snow then accumulates to its mean thickness in relation to the prescribed ice categories underneath.". Please, clarify it as one may wonder: How is the mean thickness known ahead of time? Is the snow thickness constrained by the ice thickness in the model? If so what is the

benefit of a snow scheme (even simple)?

p. 5688, l. 13. How often does the model require snow to ice transition? Depending on the frequency of occurrence and locations, may this be also a reason for the underestimated snow depth?

p. 5690, experiment 2. Information is needed regarding how the climatology of precipitation phase was derived, applied, and kept consistent with reanalyzed atmospheric temperature.

p. 5691. It is important to inform the reader about uncertainties and limitation in the OIB radar derived snow depth data. These should be stated in this section (about Methods). A mention of Kwok and Haas (2015) is required to help the reader assess the simulation results and the authors statements. Some elements of the discussion may be moved here.

The authors may consider whether the recent publication of King et al. is worth mentioning: King, J., S. Howell, C. Derksen, N. Rutter, P. Toose, J. F. Beckers, C. Haas, N. Kurtz, and J. Richter-Menge (2015), Evaluation of Operation IceBridge quick-look snow depth estimates on sea ice, Geophys. Res. Lett., 42, doi:10.1002/2015GL066389.

p. 5692. A constant precipitation term h_s(f) is used to investigate changes in snow depth. Why was this constant established over the 1979–2013 period for an analysis between 2000 and 2013.

p. 5692, l. 25. Rephrase the following statement: "snow formed at the surface of the sea ice due to rainfall". Throughout the manuscript, I think the authors must distinguish "snow", "snow depth", and "snow water equivalent". These terms cannot be used interchangeably.

p. 5693, l. 17. I would suggest to rephrase at the very least the beginning of the sentence "In contrast and as expected..." because the contrast (though of several orders of magnitude) remains insignificant from a modeling stand point. The difference in snow depth is 96 microns... and the difference in ice thickness is 5 mm. Similar, line 22 the author should reconsider “does not have a strong effect”.

p. 5694. I believe that this first paragraph presenting results is weak, and overlooked.

First, quantify the terms “good agreement” and “capture well the changes”. Note that the term “somewhat in agreement” is later used (p. 5696, l. 9-10) when referring to the same data. Better, in the discussion section (p. 5700, l. 13) the authors write “[...] model snow depth is similar to that obtained from snow radar [...]”. This is barely acceptable.

Second, explain the statement made line 19 “there is no consistent pattern regarding the model values been larger or smaller than the observations” whereas: - there seems to be an obvious simulated snow depth decrease from 70° to ∼72°, in consistent disagreement with the OIB snow depth each of the three years (fig. 1); - a clear bias of the difference simulated – retrieved snow depth as OIB snow depth increases (fig. 2).

Again, reconsideration of statements in the discussion section (p. 5700, l. 15) may be wise.

Figure 1, Panel g. The x-axis starting latitude must be 70 as on the other panels.

p. 5695, line 9. “These results show the consistency on the variability in the model results.” I plotted the variability of the retrieved and modeled snow depths per latitudinal bands as presented in table 1 and found that the model has a lower variability than the observations in 6 out of the 21 latitudinal bands (see figure R1). Clarify the statements according to your results, previous statements, and ensure that it does not lead to confusions.

p. 5695, line 10-17. I did not find this paragraph interesting about investigating possible latitudinal differences. If the objective is solely to analyze the snow depth differences as a function of latitudes, one can see on Figure 2 that regardless of the points' color they cover the h_diff range from -10cm to +20cm. One thing though that may be more

relevant to investigate is the impact of rougher multiyear ice. Figure 2 shows that the extreme values of h_diff (>25 cm, and <40cm) occur at latitudes higher than ∼80°, very likely over over multiyear ice.

Section 3.2. I suggest to replicate figure 2 for ice thickness to give a basin wide view of the simulated ice thickness.

Section 3.2. Also, I encourage the authors to expand their comment on concern about snow depth being dependent on a ice categories (sea ice thickness range).

Section 3.2. By presenting fig. 3 and stating that snow depth is well modeled (section 3.1), the authors inform us that sea ice thickness (ice thick distribution) is not well represented. Based on the relationship between ice thickness and snow depth (not explained), a quick short from wrong ice thickness leads to wrong snow depth can be concluded, limiting the values of their study. More solid presentations of the relationship ice thickness/snow depth and the results may prevent such shortcut.

p. 5697, l. 25+. The entire result description deals with shallower snow than average. May the authors discuss where snow is deeper than average in 2013 (i.e. the yellow and orange areas in figure 5c), and the possible processes (such as atmospheric) explaining it?

Table 2. h_s(sources)=h_s(sf)+h_s(r) and h_s(sf)=9.4. But the values in the column h_s(sources) do not equal 9.4+h_s(r). Why ?

Table 2 shows that simulated loss of snow due to flooding is not weak (almost 5%), and comparable to the loss amount due to heat transfer. Where does flooding occur? Also, can the authors discuss if the occurrence of snow to ice formation may contribute to the lack of similarities between the modeled and retrieved latitudinal evolution of ice thickness seen fig. 3?

p. 5698. To complement table 2, would the addition of a figure presenting the time series (for one season) of the daily values corresponding to each term in eq. 1? This could be done for a few locations over different sea ice categories (or at a regional scale which is in the manuscript title).

p. 5698, l. 15+. "Our results show that the main mechanism responsible for most of the annual loss of snow mass accumulated over Arctic sea ice in the last decade is the heat transfer between the atmosphere and the snow layer (h_s(as))." The data also show a sustained decrease of snow mass loss through this mechanism over time (see figure R2). There is no discussion about this result. How one should interpret this result (is there less snow mass loss over time)?

p. 5700, l. 3-4. Where in the result section was this shown, justified, and discussed?

p. 5701, l. 1. Put in context the value of possible OIB snow depth underestimation with simulated snow depth variability.

p. 5701, l. 3. Based on answer to the previous comment do the authors still affirm that the primary explanation between simulated and retrieved snow depth is a lack of snow redistribution in the model?

p. 5702, l. 23-24. The authors mean here that underestimated snow depth leads to over estimated ice thickness. May the author confirm this? According to figure 2a, one may instead conclude that shallower snow is related to thinner ice.

p. 5703, l. 19. Quantify not considerably.

p. 5703, l. 19. Quantify a decadal decline.

p. 5703, l. 22. Quantify the agreement.

p. 5703, l. 23+. Quantify statement.

p. 5704, l. 21. Refer to which evidences given in this manuscript indicate that the simulations performed allow the statement "In agreement with [...]".

Other comments p. 5682, l. 14. hs should be replaced by hs_mod, or defined.

p. 5683, l. 3. It is not clear to me why do the physical properties mentioned (grain size, texture) contribute effectively in regulating the heat and energy fluxes.

p. 5683, l. 8. The subject of this sentence ("This") is unclear. Also is verb of this sentence ("maintain") best suited?

p. 5683, l. 21. I would recommend the authors to replace the references with the studies of Zygmuntowska et al. (2014) and Kern et al. (2015), both of which focus specifically on the uncertainties related to sea ice thickness retrievals.

Zygmuntowska, M., Rampal, P., Ivanova, N., and Smedsrud, L. H.: Uncertainties in Arctic sea ice thickness and volume: new estimates and implications for trends, The Cryosphere, 8, 705-720, doi:10.5194/tc-8-705-2014, 2014. http://www.the-cryosphere.net/8/705/2014/tc-8-705-2014.html

Kern et al. (2014) is cited elsewhere in the manuscript.

p. 5684, l. 14. OIB airborne measurements are obtained during a few days every year, and rarely survey several time the same area during one campaign. The authors should rephrase their statement that OIB has a larger temporal scale than in situ measurements taking place yearly.

p. 5684, l. 17. ICESat-2 currently planned year of launch is 2017: (http://icesat.gsfc.nasa.gov/icesat2/mission_overview.php). Also, keep consistency in the acronym format (both ICESAT and ICESat are used in the manuscript).

p. 5684, l. 21. May be replacing "on" by "than" will make this sentence clearer. If it changes the meaning, then the authors need to rephrase the last part of the sentence.

p. 5686, l. 2. May the authors add the references to the few studies they are referring?

p. 5686, l. 6. Replace i.e. by e.g.

p. 5686, l. 29. Are only these two snow properties kept constant (which I understand with the use of i.e.), or also other ones like those listed line 18 and page 5688 lines

8-10? It is important to keep consistency throughout the manuscript.

p. 5687, l. 17. Castro-Morales et al. (2014) is not in the references.

p. 5689, l. 5. Can values be added to prove that the model has reached stability and that any artificial trend has been removed?

p. 5689, l. 19. Delete the reference to Lindsay as it already is the subject of the sentence.

p. 5689, l. 28. Should the reference be labeled 2002b?

p. 5690, l. 7. Why was year 2005 used?

p. 5692, l. 11. Check spelling of “ERAInterim”.

p. 5697, l. 19. May the authors justify the selection of years 2000 and 2013?

p. 5699, l. 8. Replace “Up to date” by “To date”.

p. 5699, l. 12. Indicate the corresponding run ID in Castro-Morales et al. (2014).

p. 5699, l. 19. Replace “an” by “and”.

p. 5699, l. 20-24. Is this statement in agreement with Castro-Morales et al. (2014)’s paragraph [13]?

p. 5702, l. 21. Delete coma.

p. 5703, l. 5. Replace “the minimum” by “a minimum”.

p. 5703, l. 13-17. In my view this paragraph can be deleted.

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

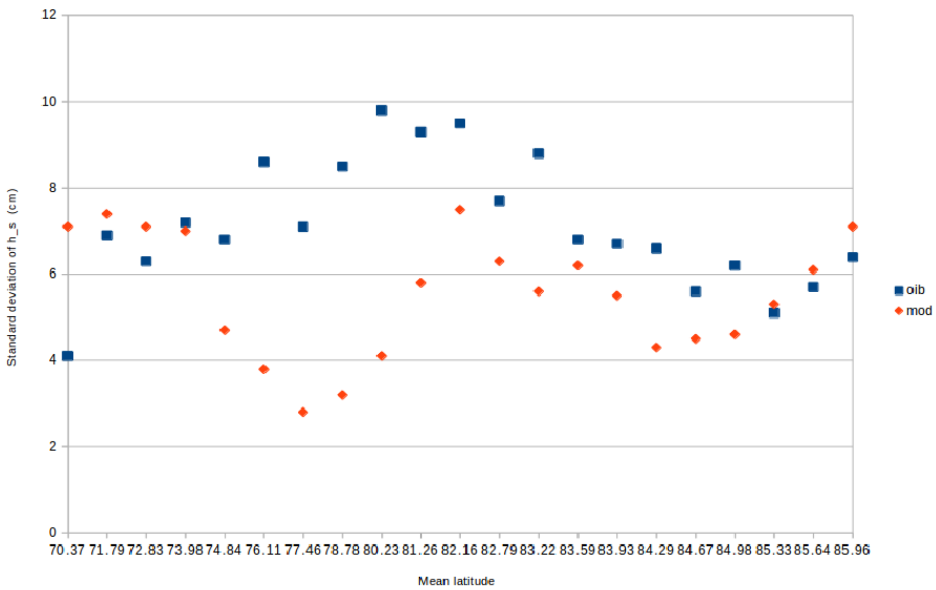

Fig. 1.

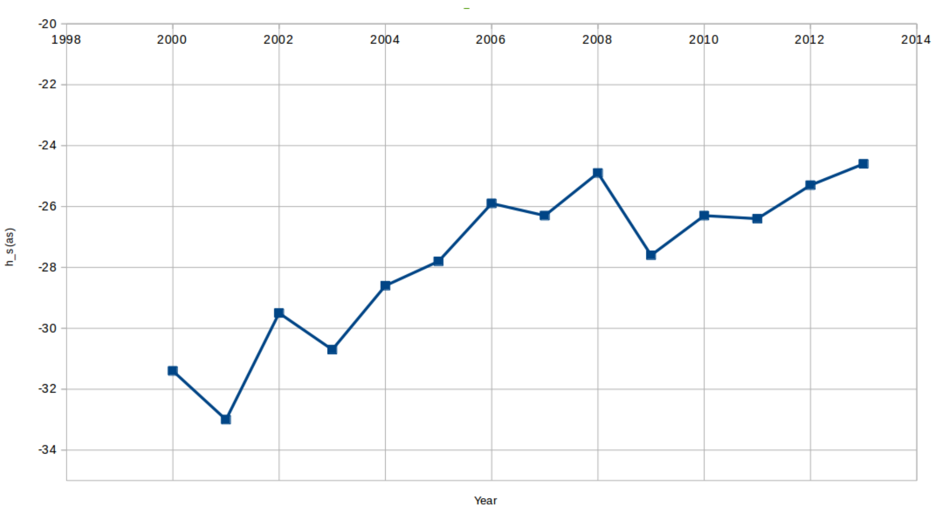

Fig. 2.