

## **Response to comments from anonymous referee #2**

### **Manuscript tcd-9-5681-2015**

We appreciate the valuable comments from anonymous referee #2 and their support on the publication of this manuscript in *The Cryosphere*. Particularly we appreciate the suggestion made to stand out the main contribution of our work regarding the evaluation of trends in Arctic snow accumulation during the last decade. Thus, we have taken into account the suggestions, which made the manuscript to improve considerably from its discussion form and strengthen the weaknesses pointed out by the referee. Below are one by one the answers to the comments.

However, please note that after adding the valuable recommendations from three anonymous referees, substantial changes to the manuscript have been made, and we assess the main changes within the responses. To ease this, we stated in blue the page and line numbers where the change has been done in the revised manuscript. We also avoid in most cases to transcript here the original comment of the referee due to the lengthy response, but we made reference by the page and lines numbers in the discussions' manuscript.

#### **General issues:**

- We have now made clearer the aims of the manuscript (P6, L196-204) which are here listed as well: 1) evaluate the simulated Arctic snow depths obtained under a simple ice/snow-related model scheme, by comparing to large scale snow-radar measurements from NASA's Operation IceBridge, 2) evaluate the decadal trends of Arctic-wide snow depth using the results simulated by the model, and associate these changes to sea-ice extent and thickness, and 3) evaluate the contributing factors to the snow depth changes using a robust snow mass budget.

As recommended, we have put forward the evaluation of the trends in Arctic snow together with a better assessment according to the suggestions made below.

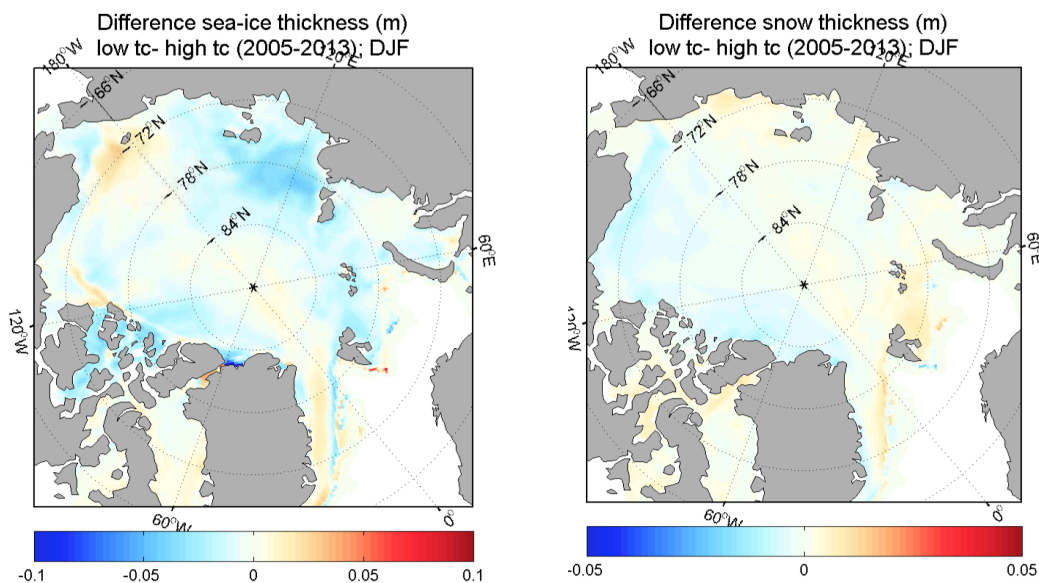
- We improved the presentation of the results regarding the decadal change of snow depths by showing the time series of the April snow depth anomaly (mean April snow depth with respect to the multi-year mean) per year (Fig. 7a) and April mean (Fig. 7b) also by Arctic region after we divided the model domain in six areas (P19, L627-632): Canadian Basin, Baffin Bay, East Siberian and Laptev Seas, Eurasian Basin, Barents Sea and Nordic Seas (Fig. 6a). Instead of showing maps of the differences, we choose to show time series and analyze the variability and trends over time. Our results for this part are shown in P19-20, L632-683 and the discussion of the variability also includes the predominant ice type (first or multiyear ice), sea-ice extent and snowfall trends (P26-28, L868-937).

Regarding the snow mass budget, we emphasize that this is a robust estimate taking into account only the processes that are currently explicit in our model configuration (P26, L938-943). Following the suggestion made by referee #2, we evaluated the trends and changes of the sink and source terms in our snow mass budget as yearly mean (Fig. 8, with sum of sinks and sources listed in Table 5)

and also by monthly means for one year (Fig. 9). To calculate these results we modified our calculation on the basis of a recommendation made by anonymous referee #3, in which we are now accounting the ice concentration in the model grid cell to correctly refer to an equivalent of snow mass (snow depth times concentration). These new results made our discussion and summary to improve and to lessen the tone of speculation.

To evaluate the potential trends on snowfall, we refer to the study by Screen and Simmons (2012) (P28, L944-949) listed in the references, in which they evaluate the snowfall and rainfall trends from ERA-Interim over the period of 1989 to 2009. The authors concluded that over the period of analysis the summer snowfall declined by 40 % over the Arctic Ocean and the Canadian Arctic. This decline is suggested to be mainly due to the warming in the Arctic low-atmosphere, leading to a decrease in the precipitation in the form of snow (we included these lines already in the discussion manuscript, now P4, L108, 114). We linked our results to these previous observations.

- Regarding the thermal conductivity experiment, it is not meant to be a relevant part in our manuscript, since it is a sensitivity experiment that doesn't relate directly to our snow depth analysis. For this reason, and as mentioned by the referee other works have been dedicated exclusively to this topic, we did not include explicit results in figures or tables, but decided only to briefly mention it. This was towards the thought that if we do not mention this experiment at all, the community may expect a discussion on how our results may be affected by changing the fixed snow thermal conductivity. Our results clearly show that the sensitivity of snow depths to low and high-end values of snow thermal conductivity is low (difference of 0.0096 cm), and slightly higher in sea-ice thickness (0.48 cm) as mentioned already in the manuscript. This is in agreement to other works such as in Lecomte et al., 2013 (as mentioned in our discussion, P24, L810-813). We found these differences only in the winter means of snow and ice, and here we include below the figures of our comparison.



As the referee points out, and despite the low differences found in snow and ice thickness, the change in thermal conductivity shows a higher effect in regions of

thin ice. We are curious about the unpublished results the referee is mentioning regarding similar sensitivity experiment in similar single-layer sea ice models that show high snow depth changes using different thermal conductivities. The reasoning behind the competing processes between increasing the snow insulation by decreasing the thermal conductivity, but also forcing the snow layer to be thin in the presence of a thin sea-ice layer makes sense as to finding little difference in our results. The suggestion made by the referee to test the sensitivity to the snow thermal conductivity by using a different snow distribution scheme (i.e. traditional 7 homogeneous ice classes given in Hibler, 1984) sounds interesting and certainly opens an attractive question for future work. However, it is not the aim of this manuscript to make an extensive sensitive study for thermal conductivity. We invite the referee to contact directly the corresponding author for potential collaboration if interested on further test the competing process of the snow parameterization and the variation of thermal conductivities.

Furthermore, we acknowledge that the results of this experiment do not contribute to our decadal analysis of snow depth; however, because of the statement given above we decided to leave this section as is pointing out the potential competing processes of reducing the thermal conductivity and the snow distribution scheme (P24, L804-810).

- What suggests referee #2 regarding the lack of significance on our analysis by latitude distribution between the model and radar snow depths from the NASA's Operation IceBridge is a shared comment also for referees #1 and #3. Thus, one of the major changes in the revised manuscript is a new analysis of our results on the basis of ice type. For this, we used the ice type flag given in the OIB data set. For details see: P11-12, L363-380. The variability of the model and OIB data are now analyzed and discussed in this context, rather than by latitudinal distribution (P15, L511+).

- As mentioned above, our snow mass budget is quite robust and simplified, based solely on the explicit processes the model is taking into account for the changes in snow depth: heat transfer between the different interfaces, snow-ice formation due to snow flooding, falling snow as given by the forcing data. It is therefore plausible to expect that the physical processes that are missing in the model may explain, at least partially, the snow mass that is missing to close the budget. We agree that the redistribution of snow by wind cannot be considered as a "new source" of snow because it is simply redistributing the mass that is already present. What we rather aimed to suggest is that the changes in snow depth can vary due to horizontal snow redistribution, then in regions where accumulation of snow is larger than sinks, snow redistribution can contribute to "add" snow, however, the referee is right that this is not "adding" snow mass. We appreciate the comments on this regard and improved our discussion based on concept clarification about the redistribution of snow by wind. We clarify this in the manuscript and we improved our discussion by adding the references of: Chung et al., 2011 and Lecomte et al., 2015a and b (P24, L789-794). See also P12, L406-410 and P29, L955-959 on the clarification of the residual term in the mass budget.

## Specific comments:

**Title** – we followed the advice of the referee and modified the title to a more specific text: “Snow on Arctic sea ice: last decade changes from a model simulation”

### Abstract

L14 – modified as suggested (also by referee #1)

L16 - According to this suggestion, we have now modified our results and presented a comparison of the model sea-ice thickness to satellite data from ICESat ([Section 3.1, P13, L435-443](#)). Summary of results are given in [Table 1](#). With regard to year-to-year snow depths, we show now a time series of the April snow depth anomaly (difference with respect to the April multi-year mean from 2000-2013) in [Fig. 7a](#), and also we show the total April mean per region in [Fig. 7b](#) (results in [P19, L627-674](#)). By providing the snow depth anomaly we set a reference to the changes of snow depth, this makes the result to be presented in a clearer way. The total mean per region on each year helps to represent better the large-term trend of the snow depth for each region.

L23-26 –

The referee introduces here a comment that is important and contributes to our considerations in the snow mass budget. Our residual term is a synthetic variable meant to account for processes in the model that are not explicit ([P29, L955-959](#)). As suggested by the referee, a contributing factor to this residual term is loss of snow mass into leads. However, and based in our previous assumption to consider into this residual term also a “source” of snow due to accumulation in ridges by wind redistribution, the residual term then remains uncategorized and we left it solely as a residual component in the mass budget (e.g. not as part of sources as done previously) without attempting to close it ([P21, L692-701](#)). Due to this, we cannot give an estimate contribution of each process that it is possible to evaluate with the model, however we still present their individual contributions as sink or source to the snow depth per year ([Table 5](#)).

Regarding the recent works by Lecomte et al., 2015 a and b, mentioned by the referee, we were not aware of them at the time of the submission of our manuscript, therefore we did not mention them before. However, we have now included these references and made reference to them correspondingly throughout the revised manuscript.

### Introduction

P5683, L2 and L3– we corrected this expression to: “physical (e.g. grain size, texture and density) and thermal properties (e.g., lower thermal conductivity than the sea ice thermal conductivity)” ([P1, L40-41](#))

P5683, L4– removed as suggested

P5683, L8-9 – we rephrased this statement to “...contributing to the total sea-ice mass conservation and heat budget of the Arctic Ocean.” ([P2, L43-44](#))

P5683, L10-11– we corrected accordingly and re-arranged the paragraph mentioned by the referee, by splitting it ([P2, L45-47](#)) and including the trends

portion to the section where it is properly addressed further down in the introduction (P3, L98-102)

P5683, L17 – changed as suggested, now moved to P4, L127-129

P5684, L2 – the use of up to date here was wrong, we rephrased this sentence to: “The work by Warren et al. (1999) is to date perhaps the most comprehensive study of large-scale Arctic  $h_s$ ” (P2, L54-55)

P5684, L145 – after the suggestion by referee #1 to modify this sentence, now the comma is removed and the new sentence is: “The NASA-Operation IceBridge (OIB) represents a recent effort to measure snow depths in Polar Regions at larger spatial scale than the yearly scarce in situ point measurements.” (P2, L65-66)

P5684, L22-26- in the revised manuscript we have now addressed further the uncertainties of the OIB data, inherent particularly to the radar technique and limitations under very thin and very rough ice, and made reference to the recent work of Holt et al., 2015, Kwok and Haas, 2015 and King et al., 2015 (P3, L80-96). These uncertainties are later addressed in the context of our findings (e.g. P23-24, L777-788)

P5686, L1 – we appreciate this comment from the referee, and as suggested we replaced here and everywhere in the manuscript the word “validate” by “evaluate” (e.g. P1, L166-168)

P5687, L11-13 – As suggested by the referee in the general issues above, we have now evaluated the trends and changes of the sink and source terms in our snow mass budget as yearly mean (Fig. 8, with sum of sinks and sources listed in Table 5) and also by monthly means for one year (Fig. 9). Results are presented in P21-22, L716-747.

## **Methods**

P5688, L6-7 – we improved the description of the snow parameterization in our model configuration. Referee #1 also suggested this (P8, L240-254).

P5688, L9-10

In the MITgcm sea-ice model, the ice and snow albedos are considered tuning parameters, and are generally modified, under the range of observational studies, to meet a best fit that agrees with the atmospheric forcing that will be used to drive the model. For example, the ice and snow albedos in a MITgcm configuration driven by ERA-40 atmospheric reanalysis were modified to be unrealistically high to compensate for the low radiation that was well known to be present in ERA-40 (Losch et al., 2010). Another model sensitivity study using MITgcm also experienced with tuning the standard albedo parameters, finding the best combination for a different forcing data (e.g. Nguyen et al., 2011 using JRA-25 to drive the model).

The original values were adopted from Flato and Hibler (1995), with 0.70 for wet snow and 0.80 for dry snow. In the current standard MITgcm configuration, the wet snow albedo remains the same and the dry snow albedo has been

increased slightly to 0.84. For this manuscript, we chose to continue using the standard current values of 0.70 for wet snow and 0.84 for dry snow, since it was not our aim to evaluate the snow distribution as a function of albedo values. We added the source of our albedo values in [P8, L256-261](#).

[P5688, L10-11](#)- we improved this paragraph as suggested ([P8, L240-245](#)).

[P5688, L15](#) – corrected text as suggested ([P8, L265](#)).

[P5688, L17](#) – As suggested by the referee, the 15 cm in snow thickness used as a cut-off to define the selection of the surface albedo is too high. This number is used in the standard MITgcm sea-ice model configuration and we believe it is so large to account for horizontal heterogeneity.

[P5689, L19](#) – citation removed as suggested since it is given at the beginning of the sentence ([P9, L291-296](#)).

[P5691, L16](#) – sentence corrected ([P11, L355-357](#)).

[P5692, Eq. 1](#) – suggestion taken and removed the time derivative on the term of the left hand side in the equation ([P12, L389](#)).

[P5692, L11](#) – As in the rest of the manuscript we used now a hyphen in ERA-Interim

[P5692, L15](#) – as suggested we improved the description of the sink term in the mass budget due to the vertical heat transfer from the surface ocean into the ice/snow layer ([P12, L398-399](#)).

[P5692, L25](#) – we corrected this sentence and included the refreezing of liquid water into the snow layer as a process that is not explicitly included in the model and that can be included as part of the residual term in the mass budget. This can only be said if the criterion by Lecomte et al., 2015a (cited in this way in the manuscript), is considered: liquid water refreezing in the base of the snow layer will contribute to the increase of snow mass instead of the formation of ice to be added into the current ice layer ([P12, L406-410](#)).

[P5692, L26](#) – as suggested by the referee, we modified our statements regarding the snow redistribution by wind. We agree that this process cannot be added to our residual term because it does not contribute as a source of snow in the Arctic, but rather horizontally redistributes what is already accumulated. As mentioned earlier in this response, we modified our statements after concept clarification and the definition of our residual term in [P12, L406-410](#) and in [P29, L955-959](#).

[L5693, L2](#) – we fixed also this sentence and in our mass budget, the resulting snow depth as given by the model ( $h_{s\_mod}$ ) is now the result of the sum of the sources plus sinks and plus the residual term (kept separately) ([P13, L413-415](#)).

This is on the basis of the later to account not only for the inexplicit sources but also sinks (as the loss of snow into leads).

## Results

- first paragraph results section: as discussed earlier in this response, we understand the concerns of the referee regarding the almost negligible differences in snow and ice thickness to the change in snow thermal conductivity. We agree that this is a tuning critical parameter in ocean-sea ice coupled models that will influence the simulation of snow and ice, and that is the reason why, despite out of the scope of our manuscript, we decided to include the results of a side experiment testing two thermal conductivities. The results are also unexpected for us, since we hoped for major effects in our simulations, however the referee makes an interesting point about the possible competing processes of the snow scheme and the thermal conductivity, leading to negligible changes in snow depths. We discussed this earlier in this response and left suggestions for future works.

P5694, L20 – we modified this sentence for the revised manuscript. The use of the word “despite” was with the aim to say that the three OIB transects were measured around the same dates in three consecutive years, and one may expect similar snow depths, however there is a large variability in the snow depths from year to year. To avoid confusion we removed the word “despite” (P15, L492-498).

P5694, L26 – corrected (P15, L497).

P5694, L27-28 – we modified this sentence to: “giving insight on the ability of the model to capture the temporal changes of snow depth.” (P15, L497-498).

P5697 – we followed the suggestion by the referee and added the trends of annual snow depth (per subdivided Arctic region) (see Fig. 7b). We also left the monthly-mean year differences because these set a reference to compare with when assessing the changes over time (Fig. 7a). We responded to this already above in the comments for the Abstract section.

P5698, L10-11 – we fixed this sentence, and now we include only the loss of snow mass into leads and due to sublimation of blowing snow and addition of snow mass due to freezing of liquid water, as the main processes that account for the residual term (P21, L694-696).

P5698, L13-14 – this sentence has been also fixed (P21, L699-701).

## Discussion

P5699, L11-14 – we improved the description of the ice thickness distribution used in our configuration and included the reference to Castro-Morales et al., 2014. We left this description for the introduction section (P6, L174-190), and discuss our findings compared to other studies in the discussion (P23, L763-771).

P5699, L21 – we modified the sentence avoiding the “general rule” wording in the sentence. The description of the simple ice vs. snow relationship used in the snow parameterization of our model configuration and supporting references has been now moved to the introduction section ([P6, L174-190](#)), including the recent results by Lecomte et al., 2015a, based on results from another general circulation model. We continued this in the discussion, including the reference of Holt et al., 2015, Kurtz and Farrell, 2011 and Kwok et al., 2011 ([P23, L763-771](#)).

P5700, L3-4 – We see the point of the referee about justifying the aim of the snow parameterization in the model. However, the results presented are an indirect sensitivity study, based on results of a previous published work. In the preceding work of Castro-Morales et al., 2014, the same model configuration was used and there it is presented the sensitivity of the sea-ice thickness by using a more realistic ice thickness distribution and the current snow parameterization as described in this manuscript. The results of that work proved that the sea-ice thickness in the model is considerably improved using specially this snow scheme when compared to the standard scheme in which the snow is distributed independently of the ice thickness distribution. More details can be found in the cited work. Taking as reference these previous results, our intention is to move forward toward the improvement of the model performance, and for this manuscript, we contribute to the evaluation of the snow depths produced by that previously improved configuration. This current manuscript supports then the use of this scheme by showing that the model can reproduce quite well the snow depths in the Arctic when compared to the radar measurements.

Indeed, by forcing the model to distribute the snow as a function of the ice thickness, we intend to better simulate the snow depths distributions and implicitly account for complex processes that are not explicitly prescribed in the model, such as wind redistribution. Based on the newly presented results, and after revisiting the use of the concept of snow redistribution by wind, we agree that the current representation of snow in the model may not necessarily be benefited of the inclusion of explicit wind redistribution processes, since at least, it replicates well the large-scale distribution of snow depths.

However, if the intention is to improve the model toward better evaluations of future changes in the snow cover, we encourage the inclusion of these processes. In agreement with the recent work by Lecomte et al., 2015b, the explicit parameterizations of blowing snow by wind redistribution in ice – ocean general circulation models will actually contribute largely to better represent the missing losses of snow into leads, and minimally to represent better the snow depth accumulation in rougher MYI. We modified our statements regarding the need of the redistribution processes in the model based on these statements ([P29, L973-977](#)).

P5701 – Regarding the discussion on the thermal conductivity, we already mentioned our point toward these results at the beginning of this response.

P5702, L27 – As per suggestion, we included the long-term trends of the modeled snow depths in the revised version of the manuscript supported by [Fig. 7](#) and related discussion in [P27, L892-919](#).



P5703, L15-19 – We agree that this paragraph can be left out. Regarding the snowfall trends, we increased our discussion on the basis of the work by Screen and Simmonds, 2012 (cited in the manuscript) in relation to the snowfall analysis from ERA Interim and discussed them in the context of our model results ([P28, L933-937](#)).

P5704, L26-27 – we modified the way we describe our snow mass budget, particularly for the residual term ([P12, L406-410](#)), which can now be attributed to both losses and gains of snow in a grid cell (loss of snow due to transport into leads and due to sublimation of blowing snow, as well as refreezing of liquid water into the snow layer that may contribute with addition of snow mass) ([P13, L414](#)). We agree on the limitations of our mass budget ([P29, L955-959](#)) and the discussion has been modified with the improvement of the wind redistribution concept.

## **Conclusion**

P5705-5706, L25-24 – we present a summary in the revised manuscript instead of a conclusion ([P30](#)) and it contains six main points extracted from our work.

## **References**

- The missing reference is now in place and revised the complete list of references ([P31-36](#)).

## **Tables and figures**

Table 2 – now replaced as [table 5](#), revisited and corrected. The referee is right that the only source for snow accumulation as it is right now is the snowfall, and we made that now clear in our manuscript. [Table 5](#) is now showing only the sum of sinks, the residual term and the resulting snow depth as given from the model output. For the individual terms in the budget, we took into account the recommendation of showing in a figure the contributing factors of the mass budget as annual means so the trend is easier to visualize ([Fig. 8](#)), but we also show the time series of the monthly means for each term over the year 2013 ([Fig. 9](#)), to assess the seasonal variability and interaction between the processes.

Figure 4 – In previous [Fig. 4](#) ([now Fig. 5a](#)) we corrected the term that was previously used as “snow accumulation” (i.e. sum of the snow depth). What we plot here it is the “accumulation rate” calculated as the snow depth in a month minus the previous month snow depth. Thus one can expect negative values if the month analyzed has less snow than the previous month, and this is occurring in summer months when the snow is melting away. This was done in this way to observe the inter-annual changes of snow depth and over the period of analysis. We have now re-named the term in the figure, caption and in the text itself to avoid confusion ([P17, L574-576](#)).