Response to comments from Anonymous referee #1.
Manuscript tcd-9-5681-2015

We appreciate the comments from anonymous referee #1, particularly the support to our manuscript to be published in The Cryosphere. Below we respond and clarify with detail the comments made by the referee.
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.

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… sea ice thickness is not well reproduced by the model.

We agree that in the manuscript we did not include a specific section where the year-to-year loss of sea ice is analyzed. Regarding the sea-ice thickness simulated by this model configuration (as shown in the now included Castro-Morales et al., 2014 reference) is better represented during spring (February-March) than in Autumn (October-November) when compared to ICESat. In the cited work, the main focus was the evaluation of the simulated model sea-ice thickness, and more details regarding this comparison can be found there. However, to set the model sea-ice extent and sea-ice thickness into the context of our snow depth analysis and evaluate the year-to-year loss of sea ice, we included a new section (now 3.1, pag. 13) in the revised manuscript where we compare the simulated sea-ice thickness against ICESat data (comparison of results shown in Table 1), and the mean monthly sea-ice extent against three satellite products: OSISAF, SSMI/I and AMSR-E (from the NSIDC web portal) (results plotted in Fig. 1). In autumn (October/November) the model underestimates the sea-ice thickness on average by ~1 m, and is better reproduced in spring (March/April).

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… In the context of this study (and abstract), do the authors mean that the summer sea ice extent matter more than sea ice formation (freeze-up) date for snow accumulation?

The referee makes here an interesting point. For the revised version of this manuscript we analyzed the first day of the sea-ice freeze-up (P18, L607-613), and compared it against the summer sea-ice extent and spring snow depth of the following year.
This was done for the years 2009 to 2013 due to the model simulation at daily temporal resolution was limited to this years. The results are shown in Table 4.

Independently of the uncertainties in the model to represent the sea-ice extent (as discussed above and in section 3.1 of results), we suggest that a combination between a summer minimum sea-ice extent and late autumn freeze-up, contribute to less accumulation of snow for the following spring.

p.5682, l.23. Were the results truly compared to at least 10 years of radar snow depth on sea ice retrievals?
Thanks to the referee to pointing out this. We corrected our wording regarding a last decade time frame when referring to the observational snow depth data only to 4 years according to our analysis (P12, L380-382). For the model data analysis, we included the time frame between 2000 and 2013; therefore, it is correctly referred to last decade analysis (P17, L571-574).

p. 5688, l. 1-8
New text has been added that contains more information on the ice thickness distribution (P8, L244-254). However, for more details the reader is referred to Castro-Morales et al., 2014. Indeed, in this particular configuration the simulated snow depth strongly depends on the prescribed ice categories, and that is what has made the model to improve considerably its snow thickness representation. The proportion on each sea ice category depending on its thickness is listed and the snow depth accumulated should do so in relation to this probability. As mentioned in Castro-Morales et al., 2014, the used ice thickness distribution is a generalized one for the entire Arctic, and although this is not ideal due to the heterogeneity of the ice thickness in the Arctic, this ITD reflects more realistic conditions since it is based on historical airborne electromagnetic sea-ice thickness measurements, than in the traditional seven-homogenously distributed ice classes defined in simple sea-ice models.

p. 5688, l.2 Add the time period covered by these historical ice thickness distributions.
The time period of the airborne electromagnetic sea-ice thickness for the generation of the used ITD has been added (P8, L.245-248) Comments on their implication were thoroughly discussed in Castro-Morales et al., 2014, thus, we prefer to continue the focus on the snow evaluation in this manuscript and not in the generation of the ITD.

p.5688, l.7
We believe here that the referee was confused as to how the interaction between the prescribed sea-ice thickness probability density function and the snow depth. This is thoroughly described in Castro-Morales et al., 2014, however to clarify this, we also included more specific text in the current manuscript to avoid confusion (P8, L240-245). In summary: the snow scheme is independent of the sea ice formation, following independent thermodynamic process in which snow will accumulate on sea ice when this is present in a grid cell, and will accumulate according to the precipitation and temperature that the model sees. Then the snow layer is distributed, independently of the sea ice thickness underneath, following the same proportional fraction in the prescribed probability density function for each ice category. i.e. a low probability of occurrence of thinner ice in the ITD (0.066 for ice category 1) will be used as a

coefficient to obtain the snow depth. Of course, this is still subject to thermodynamic changes due to the heat fluxes with the atmosphere and sea ice). Figure 3b in Castro-Morales et al., 2014 show schematically this for a given grid cell with 7 ice categories.

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?

The model doesn't have a specific time set for the snow to ice transition. On each iteration of the model in which the thermodynamic processes are updated, the sea ice routine takes into account the surface sea ice conditions of the previous iteration and with atmospheric described forcing data more snow is accumulated, or melted away. On each iteration, the flooding algorithm is evaluated with regard of the sea ice level and ice surface, and when is met the flooded snow is converted into ice. By the following iteration this is again evaluated and it will continue to convert flooded snow in a fraction of the grid cell until the ice and snow interface is lifted to sea level again. Since this process is updated in each model iteration in agreement with the forcing data variability we don't expect that this ultimately has an effect for the underestimation of snow depth in the model.

p.5690, experiment 2.
We elaborated further on the derivation of the climatology of precipitation and how this was applied in the model (P10, L320-329). Basically, from the six reanalysis variables, only a monthly climatology for the total precipitation was calculated offline for the period from 1979 to 2013. We then obtained a 12-months precipitation that was repeated every year from 1979 to 2013, while the rest of the forcing variables, including atmospheric temperature, were changing according to the original reanalysis data within the same timeframe. We choose to do the climatology from the full reanalysis period (34 years) to keep consistency with the full period for the other forcing variables.

p.5691
Already in section 2.3 of Methods was included the uncertainty of the OIB radar derived snow depth data (p.5691, l.10-12 in discussion manuscript). The works suggested by the referee were available shortly before and after the submission of our manuscript, therefore we did not include them in the discussion's paper. For this revised manuscript, we have now included information regarding the limitations of the data already in the introduction (P3, L74-96) and also in the methods section in the context of the following references: Kurtz et al., 2013; Kwok and Haas (2015), King et al., 2015 and Holt et al., (2015) (P10-11, L342-352).

p.5692
We included the answer to this question two comments above (p.5690, experiment 2.), and it is also stated specifically now in the manuscript (P10, L324-329).

p.5692, l.25

We rephrased the statement mentioned by the referee and it now reads: "loss of snow mass into leads and due to sublimation of blowing snow and addition of snow mass due to freezing of liquid water" (P21, L694-696). We also checked that the terms "snow", "snow depth" and "snow water equivalent" are used correctly throughout the manuscript. We were particularly careful to refer to the snow characteristic when necessary rather than simply state the word snow, e.g. snow depth, snow mass. We refer to snow water equivalent only for units in the snow mass budget to refer to the actual amount of water contained in the snowpack without considering its density so the different contributing processes in the mass balance can be compared numerically to each other. The snow depth term is used to estate the thickness of the snow pack and takes into account the constant snow density given in the model (330 kg m^{-3}).

p.5693, l.17

In the discussion's manuscript (tcd-9-5681-2015) available online, the beginning of this sentence is actually different to what the referee stands, being: "Contrary and as expected … "; the referee is right that the previous sentence was not correct and this sentence was corrected during the online proofs; therefore for this revision we did not change it and kept as is (P13, L428-429).

p.5694

We think here the referee actually points out to paragraph 3 on the same page, rather than the first paragraph. We have now been careful on our statements regarding the presentation of results on the comparison of model to radar snow depth data by introducing quantities. This paragraph was completely re-written as suggestion from the referee (now section 3.2, P15, L486-493).

Figure 1, Panel g.

Corrected x-axis of panel figure (now Fig. 2)

p.5695, l.9

In our analysis for the revised manuscript we have now separated the snow distributed on first-year ice (FYI) and multiyear ice (MYI). This was also a suggestion made by referees 2 and 3. For that, we used the ice type flag given in the OIB data set. For details see: P11-12, 363-380. The variability of the model and OIB data are now analyzed and discussed in this context, rather than by latitudinal distribution (P15, L511+). Therefore the statement that the referee makes reference in this comment has now been entirely modified.

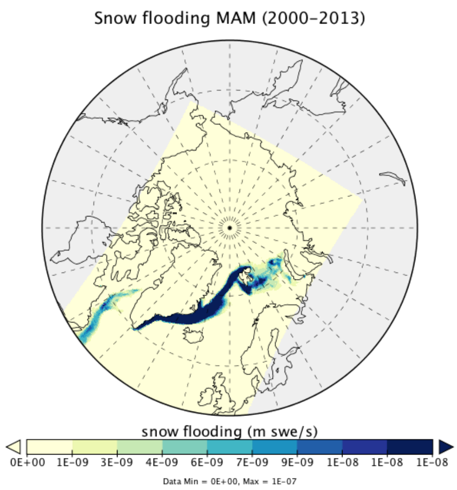
p.5695, l.10-17

As suggested by the anonymous referees #1 and #2, we have now analyzed our results on the basis of FYI and MYI, rather than only by latitudinal distribution. We discuss some of the results still in the context of latitude for the general distribution of snow in the model and the OIB data (P15, L499-509). However, the results are now analyzed in the context of ice type and the results sections have been modified considerably. New tables with results grouped by ice type are added, as well as figures. Our results show that the larger differences between modeled and OIB snow depths (h_{s_diff}) occur on FYI, where the model overestimates snow depths by 2.5 cm on a range of ice thickness from 0.12 to 4.9 m, but the referee is right that there are also some data points located a higher latitudes (80 degN) where the snow model overestimation is of 60 cm against the OIB data and it is for snow on MYI (3.5 m thick) (Fig. 3a and d).

Section 3.2
Suggestion taken. In Fig. 3b now we included the distribution of the sea-ice thickness for the model and OIB data over latitude (P15, L505-509). In Fig. 3, we also included two other panels with the distribution of h_{s_diff} separately for FYI and MYI over sea-ice thickness (Fig. 3c an d).
Our analysis is now based on the snow distribution by sea-ice thickness ranges instead of latitudinal bands (P16, L536-542). With this we sustain the analysis of the snow depth dependence on ice categories similar to the snow parameterization described in the model.
We also modified previous Fig. 3 (now Fig. 4) with again the separated analysis of snow depth, sea-ice thickness and the ice/snow ratio for FYI and MYI (P17, Section 3.3). We combine the analysis made here with the results presented in the new section 3.1 (P13) on the model sea-ice thickness evaluation. As has been demonstrated in our previous work (Castro-Morales et al. 2014), and now here included for the results presented (Table 1 and P13, L436-443), the modeled sea-ice thickness is similar to that retrieved by satellite measurements (ICESat) for February and March. The sea-ice thickness in autumn (October/November) however is overestimated by as much as 1 m. The OIB data is retrieved during spring, thus our analysis of ice thickness and snow ratios for this period of time sets the validity of our analysis. We extensively discuss now these results and also include discussions regarding the ice-thickness retrieval from OIB data.

p.5697, l.25+
To analyze the spatial changes in last decade snow depth (now section 3.4) we divided the Arctic domain in our model into six regions: Canadian basin, Baffin Bay, East Siberian and Laptev Seas, Eurasian Basin, Barents Sea and Nordic Seas (see Fig. 6a). In this context, we analyze the changes in snow depth compared to the multi-year mean from 2000-2013 (i.e. snow depth anomaly). Based on the recommendation made by referee #2, we included time series of the annual snow depth anomaly (Fig. 7a) and also the annual mean per region (Fig. 7b) for better evaluation on trends. These results are presented in P19, L627+. As pointed out by the referee, some areas (and also in some years) show an increase of snow depth compared to the multi-year mean. And we argue that (P20, L675-683): "The snow layer on sea-ice is a dynamic element that varies in relation to the atmospheric conditions (atmospheric temperature, wind and precipitation patterns) and changes in its vehicle, which is the sea-ice itself (drift, divergence and convergence, freeze-up timing). Therefore, accumulation of snow in some regions with a year-to-year variability is expected. The accumulation of snow in regions where FYI dominate, is also reflected by a larger than average sea-ice extent, while a larger accumulation of snow on MYI with respect to the multi-year mean can be due to the drift of sea-ice that enables the accumulation of snow mass by convergence. Despite the snow accumulation periods observed during the last decade, the model results show a mean loss of snow toward year 2013 in both FYI and MYI areas"

Table 2 –
We revisited former Table 2 (now Table 5) and found a typo in the first row of the last column (instead of 58 must be 48). However, we have re-considered the source terms in our mass budget and considered only the snowfall contribution (constant value of 9.73 cm swe/a instead of 9.4 cm swe/a).
Pointed out by anonymous referee #2, the residual term that represent the processes that are not explicitly accounted in the balance, represent both sinks and sources, thus cannot be added exclusively to the last (P21, L692-696). We also 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 grid cell to correctly refer to an equivalent of snow mass (snow depth times concentration) (P13, L417-419).
The resulting snow depth as given by the model is now the result of the sum of the sources, the sinks and the residual term kept separately. We then limited Table 5 to three columns where we show the residual term, the sinks and the resulting mean annual snow depth. The individual terms are plotted in Figure 8 for easier evaluation on the trends and contribution of each term over time. Due to the separation of the residual term from sinks and sources, we therefore are unable to estimate individual contributions in percentage, and we limit our results to contributions or losses in cm swe per year.
We want to emphasize that our snow mass budget is a robust estimate taking only into account the processes that the model is independently quantifying in the current configuration, and it is not aiming to explain all of the processes that may in nature act on the sources and sinks of snow in a yearly basis. Therefore, we do not expect that the mass budget is closed by the still few explicit processes in the model. In order to account for these, we included the residual term that, although synthetic, it gives a picture of the account of the missing explicit processes that act on snow as well as the possible uncertainties in the model. Despite its robustness, we believe that our mass budget provides to the reader a broad picture of the processes and the interaction between them, furthermore, it points out to the relevance on considering the addition of explicit processes in the model to improve the understanding of the sinks and sources of Arctic snow.
Regarding the contribution of the flooding snow term in the mass budget, this process is predominantly occurring at the sea-ice margin with dominance in march each year (see figure below for spring multi-year mean of the flooding term). The extent of the areas affected by flooding vary over time and are related to where the ice is in closer contact with the ocean and dynamically moves due to drift. The area of the Arctic influenced by flooding is not large compared to the area of the entire domain, and this process takes part primarily in East Greenland during March every year in the ice margin of the drifting multi-year where snow ice is formed. Other areas with flooding snow are Barents and Labrador Seas, although not with a predominant contribution on the loss of snow due to this process.



We have included in the main text a brief description as to where flooding of snow mainly takes place and linked it to the observed snow distribution (P21, L707-711). The region east of Greenland is dominated by multi-year ice, and as snow ice is formed contributes to thicker ice. The contribution of this process in the course of a year is limited to one month, and we do not believe this process has a significant contribution to the yearly sea-ice thickness. Furthermore, with our current analysis by categorizing the data into ice type, the latitudinal increase in sea-ice thickness was less clear, particularly on MYI (see Fig. 4d) as seen in the discussion paper. However to evaluate effectively the role of this process on the thickening of sea ice, an evaluation of the rate of change of sea-ice thickness due to flooding must be assessed, and this is out of the scope of this manuscript.

p.5698

We followed the suggestion of the referee for the improvement of the manuscript (P22, L723-748), and added a figure (Fig. 9) that includes the monthly means at regional scale of each term in the presented snow mass budget within one year (2013) to observe the evolution of the processes.

p.5698, l.15+

We think that the consistent decrease in the hs(as) term over time (less negative indicating less contribution to the budget) is directly linked to the loss of snow mass over time (Fig. 8b). As the processes have an effect on the snow layer, with dominance of the heat transfer with the atmosphere, there is also less mass available, translated in less contribution of this factor (P21-22, L717-721).

p.5700, l. 3-4

As we mentioned in the manuscript, one of the aims of the study is to evaluate the performance of the model in the context of the snow depth as a result of the snow parameterization in use in the model. We modified this sentence (P23, L772-774) and are clearer in our goals in the paper (P6, L196-204) to avoid confusion in our discussion.

p.5701, l.1
As suggested by the referee, we included more information regarding the potential underestimation of snow depths in the OIB data and the simulated snow depths (P23-24, L777-788). The snow depth in the model is generally overestimated in both FYI and MYI when compared against the OIB data, however, a larger difference is observed over FYI (by 2.5 cm on a regional average) than over MYI (by 0.8 cm on regional average). The OIB data is known to underestimate the snow layer on very thin ice and in open leads due to the radar failing to retrieve correctly the interfaces. Underestimations may also arise on rough surfaces due to the problems with the radar return in slopes.

p.5701, l.3
Our model results underestimate the snow depths in both FYI and MYI when compared to OIB. However, as mentioned in the comment above, based on the new results provided in the works by Holt et al., 2015, Kwok and Haas, 2015 and King et al., 2015, the OIB snow depths are underestimated on FYI and also on very rough ice, which generally characterizes MYI. Keeping in mind the uncertainties in both the model and the OIB data, at this point we conclude that the model replicates well the Arctic snow depths, and we believe the results are confident enough to carry on analysis on regional trends. Furthermore, based on the analysis of sea-ice thickness in the model and OIB data (P25, L841-855), we added in: P26, L856-859 the following: "Based on the observation that: during spring the model sea-ice thickness is well represented, and that the spring snow OIB is underestimated while the OIB sea-ice thickness is overestimated, the simulated deeper snow layers in the model at least for MYI, might go on the right direction."
We conclude however that the model will certainly benefit using a more comprehensive snow parameterization with explicit redistribution processes, which may affect the horizontal redistribution of snow by accounting by the loss of snow mass into leads and also the convergence of snow in ridges on MYI (P29, L973-977). We included these statements also in the summary section of the revised version (P30).

p.5702, l.23-24
From our improved analysis of the result on the basis of FYI and MYI, our results now show a consistent pattern of finding thin snow layers on thick snow types. Thus, we have now modified the discussion based on this (P23, L763-771).

p.5703, l.19
As required we quantified the statements given in this paragraph (P27, L892-919). The division of the Arctic domain in six regions as mentioned above, now benefits this analysis. According to suggestions from anonymous referee #2, we have included a year-to-year decrease of snow from 2000 to 2013 with respect to the multi-year mean in April (Fig. 7a) and the mean April snow depth per year (Fig. 7b). In this context, we have modified the discussion in this part to fit to the new-presented results.

p.5703, l.22
Modified accordingly to what is mentioned in the response above (P27, L892-919).

p.5703, l.23
Modified accordingly to what is mentioned in the response above (P27, L892-919).

p.5704, l.21,
We modified this sentence to be clearer in our statement on the comparison to the results published by Screen and Simmons (2012) (P28, L933-937). We refer here to the agreement on their results and the one presented here regarding the influence of low-atmosphere warming to the snow depth decrease we observe in our model results.

Other comments:
p.5682, l.14, modified as suggested (P1, L26)

p.5683, l.3, In field-based experiments it has been shown that the metamorphic properties of snow have a strong influence on the heat and energy fluxes trough the snow layer. In Sturm et al., 2002a (reference cited in the manuscript), the authors mentioned that during the SHEBA experiment, the recent snow layer generally has a wide range of grain characteristics and it is the layer that is more susceptible to the influence of variable meteorological conditions. For example, wind slab, fine grained and dense surface snow, results in a well-bonded type of snow that will translate in high thermal conductivity. This type of snow is formed during storms where the wind breaks the large snow grains and the small grains then pack each other. In regions were the snow layer contains mainly large grains, will tend to have low thermal conductivities due to the larger amount of air pockets between the snow grains. Thus, the structure of snow regulates the thermal and mechanical properties of the snow layer. We therefore did not modify this paragraph, but if the referee encounters it misleading we ask for a suggestion.

p.5683, l.8, we rephrased this sentence to make it clearer and it reads now: " … contributing to the total sea-ice mass conservation and heat budget of the Arctic Ocean.." (P2, L43-44).

p.5683, l.21, as suggested we added the two references recommended by the referee, we also decided to keep Kwok et al. 2011 and Kurtz and Farrell, 2011 since they both also discuss the need to improve the radar measurements of depth and snow density for the computation of sea-ice thickness from freeboard measurements with radar and laser altimetry (P4, L127-132).

p.5684, l.14, we corrected this paragraph by removing the larger temporal scale and just leaving the larger spatial scale statement (P2, L65-66).

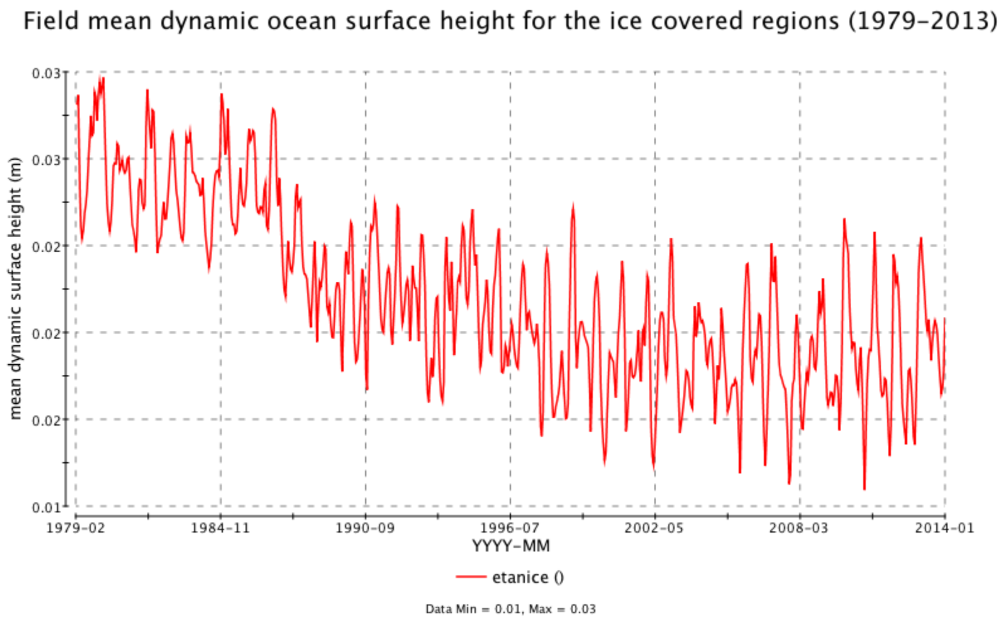
p.5684, l.17, thank you to the referee for pointing out this (P2, L62-69). It also seems that ICESat-2 will actually be launched in 2018 (see https://www.nasa.gov/content/goddard/about-icesat-2) and not 2017 as the referee suggested, there seem to be some inconsistencies in the information provided in the web, however we used now 2018 since this is listed in the official ICESat-2 webpage and we also included this link in the manuscript. We also kept consistency with the acronym throughout the manuscript.

p.5684, l.21, as suggested we corrected the sentence to made it clearer (P2-3, L71-73).
p.5686, l.2 as suggested, we added references to support this statement (P5, L162-165)
p.5686, l.6, replaced as suggested (P4, L136). These lines were moved up to group together the introduction on current models simulating snow depths (P4, L133+).

p.5686, l.29, we corrected the use of "i.e." and replaced it by "e.g." (P6, L174); the referee is correct and to keep consistency, not only density and thermal conductivity are kept constant in the sea ice model, but also snow grain size and albedos as mentioned also in P8, L254-255.

p.5687, l.17, we apologize for this, now the citation is included (see reference section, P31+)

p.5689, l. 5, This model configuration has been previously tested for stability and the years selected are well out of the transition and stability period between spin up with one reanalysis forcing data (in this case COREv2) and the consecutive simulation period for data analysis using a different reanalysis data (in this case ERA-Interim). Here, I show the mean dynamic sea surface height (sea surface height anomaly) from 1979 to 2013 (the period of ERA-Interim) for ice-covered regions for the experiment ID "std" of this manuscript. This variable is a perfect candidate to evaluate the stability in the ocean model and the interaction of the sea ice layer above it, since it acts as a perturbation of the surface in relation to the evolution of the sea ice. A seasonal variability is expected since the surface height will increase with the presence of ice, but the long-term trend should be kept more or less constant. In the following graph it is shown that from years 1979 to 1999 of the simulation, the surface height has a decreasing artificial trend, and by years 2000 to 2013, stability in the signal, with the corresponding seasonal variability is observed. Therefore, we are more confident in the results for the last decade than for previous years in this particular regional configuration. We decided to not show these results as in modeling studies this is generally avoided.



p.5689, l.19, the reference Lindsay et al. 2014, was deleted at the end of the paragraph as suggested (P9, L295).

p.5689, l.28, the reference was modified adding a "b" after 2002 (P9, L305)

p.5690, l.7, there is no fundamental reason to why the choice of years 2005 to 2013 were selected for the simulation comparing two constant snow thermal conductivities, it was only and arbitrary selection of years because this was the timeframe selected initially for the experiment. Due to the low difference between snow depth (spatial mean of 0.0096 cm) and sea-ice thickness (spatial mean of -0.4 cm) results from the low minus high thermal conductivities' experiments, we therefore do not expect this numbers to considerably change if the years 2000 to 2004 are included. However, we added more discussion regarding this experiment, it is now added in P24, L800-809, and it goes along the lines suggested by referee #2, in which the low difference in snow and ice thickness due to the change in thermal conductivity, may be possibly affected by the snow parameterization used as a competing factor. Despite using a constant lower snow thermal conductivity, potential thicker snow layers are forced to become thinner under thin ice conditions. However, this is not a central part in our manuscript and although we regard this result and increased the discussion, we leave going further in this topic for another work.

p.5692, l.11, added a hyphen to ERA-Interim (P12, L395)
p. 5697, l.19, the selection of years 2000 to 2013 was just simply to consider the last decade time frame. No modification on the text was needed as this is stated in the beginning of the sentence.

Discussion,
The suggestions for this section were taken into account and modified, however this section have changed considerably based in our new analysis of data and on

recommendations from three anonymous referees. However, here is the response based on the original discussion's manuscript.

p.5699, l.8, modified as suggested
p.5699, l.12, added simulation ID from Castro-Morales et al., 2014 as suggested, but this was moved to P8, L246 in the Methods section.
p.5699, l.19, modified as suggested

p.5699, l.20-24
This paragraph and the one written in paragraph [13] in Castro-Morales et al., 2014 are in agreement and do not contradict each other: observational studies suggest as a "general statement" that smooth old thick ice sets the right condition for snow to accumulate for longer periods due to its non-perennial condition, hence allowing the accumulation of more snow compared to the snow that is accumulated on perennial thin ice. What was mentioned in Castro-Morales, et al., 2014, is that this is ambiguous since it is just considering a very particular condition for a very heterogeneous system. For modeling purposes, however, simplicity has to be taken into account and the snow scheme in our configuration follows the general rule of more snow accumulated on thicker and more likely older ice. We mention this in P6, L177-195 in the introduction section and in P23, L762-770, and support our results with previous works.

p.5702, l.21, modified as suggested
p.5703, l.5, modified as suggested
p.5703, l.13-17 deleted as suggested