Response to comments from anonymous referee #3
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We appreciate the comments from anonymous referee #3. We are disappointed that the referee did not support our work, in contrast to referees 1 and 2, who strongly did it. We are aware of the weaknesses and limitations of our study and the comments made by three anonymous referees have greatly improved the work. We answer here the general statements that referee #3 expressed in the review, and put them also in the context of the new version and on the comments made by referees 1 and 2. We include in blue between parenthesis, the page and line numbers where the change has been done in the revised manuscript, and also to highlight some text in the response. 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.

Response to general statements/concerns:

We agree with the referee that precisely measuring in the field and reanalyzing snowfall in numerical models is complicated. The uncertainties in snowfall measurements by gauges were addressed previously in the discussion manuscript, and now correspond to P9, L284-300 in the revised version. Regarding the analysis in the model, indeed the model will react to the atmospheric forcing that is prescribed. The quality of the last will definitely control the behavior of the simulation. For this reason, we chose to use ERA-Interim reanalysis due to the best comparison to GPCP precipitation fields (even considering the uncertainties) as analyzed by Lindsey et al., 2014. This was also discussed in the discussion manuscript (now in the revised manuscript in P9, L291-300). ERA-Interim reanalysis provides total precipitation fields (snow plus rain), and this is used to drive the model. Thus it is only atmospheric input. On the other hand, rainfall can be calculated from this reanalysis because it also provides snowfall independently. In the study by Screen and Simmonds (2012) the authors analyzed the trends of ERA-Interim snowfall and rainfall separately from 1989 to 2009, and found a decline of about 40 % in the summer snowfall in this reanalysis data. This is assumed to be linked to the increase in atmospheric temperature, which leads to a decrease of precipitation in the form of snow, and increase the precipitation in the liquid form. These results were corroborated and agreed to scarce snowfall observations in the Arctic. We agree that this trend may influence our results. Besides changes in the atmospheric and precipitation patterns, other variables such as sea-ice extent and delay in freeze-up day, will ultimately also affect the snow distribution in the Arctic. Simple or complex models actually do help to understand the interaction between these processes and in the long-term changes of snow depth in the Arctic. Still, even if the model is prescribed by a realistic atmospheric forcing from reanalysis data, this does not necessarily mean that the simulated results will be realistic.
We want to emphasize, and now made it clearer in the revised manuscript that the prime aim of our study is to evaluate the snow depth from our configuration in a sea ice-general circulation model, in the context of a simplified snow

parameterization. Many general circulation models currently use this type of parameterization and try to understand the dynamics of the ice system in the Arctic in current climate conditions. It is well known that these models have a lot of limitations and uncertainties, but precisely studies like ours set a direction on the current status of the model performance and possible directions for their improvement. In our case, we are mainly interested in snow depth and made more emphasis in the analysis of trends and linked them to changes in sea-ice extent and snowfall. More importantly, we have proven that with our current model configuration it is possible to replicate the Arctic snow depth on sea-ice in a realistic way after comparing to the NASA-Operation IceBridge snow radar measurements.

Response to specific issues pointed by the referee:

1) Our conclusion is now organized as a summary of our findings (P30) and a couple of recommendations for future works. We believed that our findings listed in the summary section are well supported by our results. Also, the "unrelated" statements previously given are basis for future works and recommendations based on our findings.
   a) By evaluating the simulated snow depths against radar measurements, we are analyzing how realistic is this layer simulated by the model. The model doesn't simulate snowfall since this is provided by the reanalysis data used to drive the model. We mentioned this in the discussion's manuscript and we made again the emphasis here.
   b) We refer to the improvement of physics in the model to the inclusion of explicit processes that are currently missing in our configuration such as: horizontal redistribution of snow by wind and blowing snow sublimation. We discuss this and mention it again in the revised manuscript.
   c) The mention of data assimilation was simply to put in context our work for future directions based on our contribution, this was clearly stated, and we did not meant to relate it directly to the aim of our work. We left this now out in the future recommendations listed at the end of our summary.
   d) About the comment of measuring snow better, this is obvious but it has to be again stated in a paper related to snow and discussing snow measurements, thus we don't agree that is out of place in our manuscript.

   The four main outcomes from our work are (P30):

   1- We evaluated the simulated the Arctic snow depths on sea ice using an Arctic ice-ocean MITgcm regional configuration characterized by a simple snow parameterization (used also in most ice-ocean general circulation models) that distributes the snow proportional to the ice thickness distribution. At large-scale, the model represents better the spring large-scale snow depths in on MYI. On average, the model overestimates the snow depth by 2.5 cm on FYI and by 0.8 cm on MYI when compared to snow radar measurements from OIB.

2- Despite the simplicity of the snow parameterization (single layer) and limitations (single layer and fixed snow thermal conductivity, density, grain size and albedos) used in our model configuration, as well as the uncertainties in the OIB data in the model, this scheme preserves the simplicity of the complex relationship between ice and snow offering a practical solution to realistically simulate Arctic snow depths on sea ice.

3- Based on the realistic model snow representation, explicit snow processes (e.g. horizontal wind redistribution and sublimation of blowing snow) that are currently missing in the model may be implicitly considered by the prescribed distribution of snow given in our parameterization. However, the current configuration will benefit by the incorporation of these processes to improve the horizontal distribution of snow and, more importantly, the representation of loss of snow mass into leads and by blowing sublimation. This ultimately will help to improve the understanding of the interaction between large-scale sinks and sources of Arctic snow.

4- The model decadal trend of snow shows a decline in both FYI and MYI areas. In FYI, the decline in snow depth is related to the changes in sea-ice extent and delays in freeze-up day that limit the accumulation of snow on seasonal sea ice. In MYI, the decline in snow depth is potentially related also to changes in sea-ice extent and to increase in atmospheric temperature as shown by the loss of snow dominated by the heat transfer with the atmosphere. Previous observations of summer snowfall declining trends might also contribute to this reduction.

2) We re-wrote the "motivation" (aims) of the manuscript, but they do not change the direction of the discussion's manuscript. Referee #2 pointed out, and suggested that more emphasis should be made to the main contribution of our work, which is the evaluation of last-decade trends of Arctic snow depth from the model results. Our aims are now clearly stated (P6, L196-204) and are listed below, with notes in blue regarding the improvements done in the revised manuscript to achieve each aim:

   i) 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,
   The analysis for this part was improved by grouping the model and radar snow measurements by first-year and multiyear ice type; this was a common recommendation between the three anonymous referees. For details on our approach to do this see P11, L363-385.

   ii) 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,
   For this we evaluated the April snow depth anomaly (April snow depth minus the April multi-year mean) for each year from 2000 to 2013 (Fig. 7a) and also analyzed the trends in snow depth only comparing the April mean for each year (Fig. 7b). To clarify the trends we discuss them in regard to the sea-ice extent observations now provided in section 3.1 (Fig. 1). We divided the

Arctic domain in the model into six regions (Fig. 6a): Canadian Basin, Baffin Bay, East Siberian and Laptev Seas, Eurasian Basin, Barents Sea and Nordic Seas, and analyzed the previous trends per region. Our results are also discussed in the context of the snowfall decline as observed by the ERA-Interim analysis done in previous works (e.g. Screen and Simmonds, 2012).

iii) evaluate the contributing factors to the snow depth changes using a robust snow mass budget.
In comment number 4 below, we give further information regarding our snow mass budget.

3) We improved the description of our methods section (P7, L223+)
   - from the two simulations ran in our work both are actually very much used in our analysis. The first one is at monthly and daily resolution and mainly used for snow depth evaluation with radar measurements (daily) and for analysis on trends (monthly). The second uses a climatology for precipitation as forcing data, and it is used for the snow mass budged analysis.
   - although we erroneously missed to include the reference of Castro-Morales et al., 2014 (now included), the reference is easily found in a search platform. The ice thickness distribution (ITD) is a probability density function and it is the basis in sea-ice modeling to prescribe the range of ice thicknesses that the model will use and their probability of occurrence at a sub-grid scale. Sea-ice modeling pioneering works such as Hibler, 1984 (*Climate Processes and Climate Sensitivity*) and also observational works such as Haas et al., 2010 (*Geophys. Res. Lett.*), show examples of probability density functions for Arctic sea-ice.
   - Section 3 of results. We don't understand here what the referee really means with his question. The snow thermal conductivity will affect snow depth because defines the insulation capacity of the snow layer. This physical concept is widely known in snow and ice studies.
   - Probably here the referee mistaken our method for finding the similar geographical position on the model snow depth output to the geographical positions of the observations by radar. There was no interpolation involved, we built a search script in which the known geographical position of the OIB data was used as a base to search the closest similar geographical position in the model grid. This was previously well stated in the discussion manuscript, now in P14-15, L480-484 of the revised version. Because of the nature of the method, we needed a reference point (observations) to search into the model grid, and could not have been possible to be done viceversa.
   - We are not showing anymore the dependence of snow depths from the model and OIB wrt to latitude. As mentioned earlier, we are now basing our analysis in ice type (FYI and MYI). The suggestion of using a PDF of error for different snow depths could be have done, but it will also narrow down the observations to ice type (thin and thick ice grouping). Based also in the suggestions from referee 1 and 2, we left our analysis then to ice type.

- We corrected the term in Fig. 4 (now Fig. 5a) as to accumulation rate as the referee points out this refers to a time derivative of the snow depth (P17, L574-576).

4) We are aware of the limitations of our snow mass budget. Referee #1 also pointed out concerns on this regard. 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.
   i) 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 model grid cell to correctly refer to an equivalent of snow mass (snow depth times concentration) (P13, L417-419). Results are presented in Table 5 to three columns where we show the residual term, sinks and the resulting mean annual snow depth (h_{s_mod}=h_s).
   ii) 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. Pointed out by anonymous referee #2, the residual term that represent the processes that are not explicitly accounted in the balance, such as: 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 (P12, L408-410), thus it represents both sinks and sources unaccounted in the explicit processes and cannot be added exclusively to the last. 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 (P21, L692-696 and 699-701).
   iii) As mentioned in our methods section (P10, L320-324) we built the snowfall climatology from ERA-Interim only for the second experiment, to remove the variability or spurious trends in the data and to use this for the snow mass budget analysis. If the snowfall trend known to be present in this reanalysis data is used, then it will be a bias for accounting independently the contributing processes to the changes in snow depth.
   iv) The vertical transfer of heat from the surface ocean to the snow layer ($h_{s(os)}$ in our mass budget) can occur can reach the snow layer when thin layers of ice are present and processes like flooding (also given in our mass budget) take place. This process is mainly occurring when a portion of the snow layer gets flooded by

water due to mass imbalance (large accumulation of snow on thin ice) or in melting confined surface areas like melt ponds, turning first the flooded portion of snow into snow-ice (P12, L400-403). As we also mentioned in the revised version of the manuscript, a sinking snow layer is in closer contact with the ocean below, and during March the formation of snow-ice takes place with the loss of snow mass due to heat exchange with the ocean been more dominant during this month than in the rest of the year. $h_{s(os)}$ is more significant in regions dominated by seasonal ice and in ice margins with thin ice and snow layers that allow a larger surface ocean heat loss through the snow and ice to the atmosphere (P22, L737-742)

v) We agree with the referee that our phrasing in this sentence was incorrect when we referred to the increase of snow depth due to rain. This is wrong. We have removed this statement.