Author comments

On the discretization of the ice thickness distribution in the NEMO3.6-LIM3 global ocean–sea ice model

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Anonymous Referee #1

This paper investigates the sensitivity of the ORCA1-LIM3 model to the choice of ice thickness distribution discretization. It stands to reason that an improved representation of the ice thickness field should also lead to more realistic simulations of the coupled sea ice-ocean system. However, this is not always the case, and there has been only a handful of papers devoted to clarifying why this is so. This manuscript is therefore welcome. It is a worthwhile attempt to shed some light on this important issue by focussing on physical processes that may explain the simulated sea ice response to changes in the formulation of the IDT. I am not sure, though, the authors entirely succeed, especially as regards elucidating the reason for the non-convergence of total ice volume as the number of ice categories increases. We do not learn enough from the paper about the sensitivity of the different physical processes that control ice growth to the choice if ice thickness categories. The authors show the average bottom ice growth for experiments S1 and S3, but there is virtually no discussion in the paper as to the physics that controls the ice growth, notably, air-ice and ice-ocean heat budgets and snow and ice thermal conductivities (others?). There also seems to be a strong nonlinearity in the system's response to the number of thin ice categories, as evinced in Figs. 4 and A2, and this, I believe, should be explained through a more detailed process analysis. In its present form the paper is basically a summary of the experimental results rather than a discussion of the said results. While I understand that the authors might not desire to embark on a major overhaul of the paper, I would certainly advise that, at the very least, they report in greater depth on the mechanisms and non-linearities that control the increase in basal ice growth as the number of thin ice categories is increased.

Reply. We thank the reviewer for the helpful feedback on our manuscript. We take note that the paper deserves more investigations to understand the physical controls on the processes of ice growth and melt when the ITD discretization is changed, a point that was also raised by Reviewer #2. To meet these requests, we have conducted a comprehensive sea ice mass balance analysis to the output of our simulations. Namely, we have split the seasonal changes of ice volume by distinguishing between (1) the source *vs.* sink terms, and (2) the thermodynamic *vs.* dynamic processes:

- Source terms include:
    - (Thermodynamic) basal growth, that was already diagnosed in the first version of the manuscript (Fig. 4 of the original manuscript)
    - (Thermodynamic) snow-ice production;
    - (Thermodynamic) growth in open water;
    - Dynamic production, i.e., ice formed due to refreezing of seawater after entrapment into porous ridges.
- Sink terms include:
    - (Thermodynamic) basal melt;
    - (Thermodynamic) surface melt.

We show in Fig. R1.1 below the contributions of the various terms to the sea ice mass balance in the set of simulations “S1”, whereby the number of categories and their boundaries are changed simultaneously following the default discretization of LIM3.

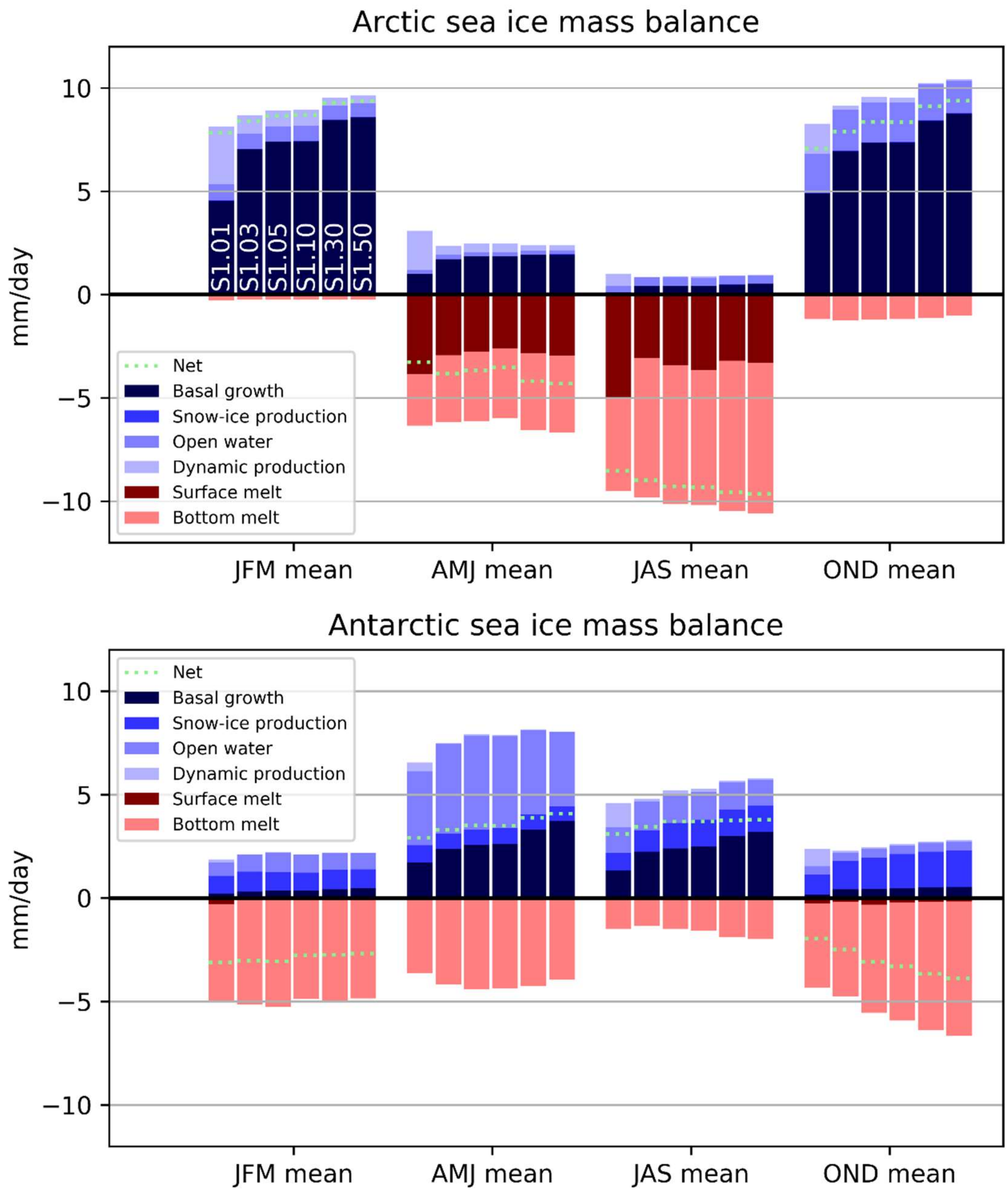

Figure R1.1. Contributions to the seasonal mass balance of Arctic (top) and Antarctic (bottom) sea ice as simulated by “S1” (varying number of categories with the default LIM3 ITD discretization), averaged over the areas depicted in Fig. A1 of the original manuscript. Blue colors refer to processes that contribute to *positive* ice volume changes, while red colors indicate processes implying *negative* ice volume changes. The name of experiments is indicated in the upper panel for the January-February-March season and is not repeated for the sake of clarity.

Thanks to this figure, we now have better evidence for the physical mechanisms at play behind our sensitivity tests. Fig. R.1.1 confirms our initial finding, by showing that basal growth is indeed the first-order factor to explain the increases in winter ice volumes noted in Fig. 2 of the original manuscript. It also reveals interesting findings regarding the simulation with one ice thickness category in the Arctic. Indeed, the decreased thermodynamic basal ice production in S1.01 is balanced by enhanced dynamic growth compared to multi-category experiments. We can understand this finding as follows:

- Decreased basal growth is a direct consequence of the lack of subgrid-scale sea ice thickness variability in the experiment with one category, as explained in the original version of the manuscript (p. 9, lines 5-9);
- Enhanced dynamic production is a consequence of the ice being thinner. Indeed, in the model, the ice strength is parameterized using the classical Hibler 1979 formulation:

  $$P=P^* H e^{-C(1-A)}$$

  where P is the ice strength, P^* and C are empirical constants set to 20 kN/m^2 and 20, respectively, and A and H denote the grid-cell ice concentration and average volume, including open water, respectively. This formulation does not depend directly on the ice thickness distribution but well on the grid-cell average thickness. In winter, A approaches 1 in all our experiments due to the thermal constraint imposed by the atmospheric forcing, so that ice strength is essentially proportional to the thickness. Since sea ice is on average thinner in the 1-category simulation (Fig. 3 of the original manuscript), mechanical redistribution is more intense in thinner 1-category simulation, which fosters dynamic ice production.

Regarding the origins of the lack of convergence in sea ice volumes, we agree with the reviewer that the result is somewhat surprising. To better understand the mechanisms at play, we first computed the theoretical dependence on growth rates on the number of categories. For this, we assumed a grid cell in which the sea ice thickness distribution follows a log-normal law with mean of 3 m and standard deviation 2 m, as depicted in Fig R1.2, left panel. We then discretized this distribution using n = 1, 2, 3, … 100 categories according to the default formulation of LIM3, and computed the expected grid-cell average basal growth rate for an atmosphere-ocean temperature difference of ΔT = 30 K, assuming no snow, a sea ice conductivity of k =2 W/mK, latent heat of fusion of L_f = 334 000 J/kg and sea ice density of ρ_i = 917 kg/m^3:

$$\dot{h} = \frac{1}{\rho_i L_f} . \sum_{i=1}^{n} \frac{k\Delta T}{h_i} g(h_i) dh_i$$

where h_i is the mean thickness of category i, dh_i is the category bin width, and $g(h_i)dh_i$ is the fraction of the grid cell occupied by sea ice in that category. From this theoretical analysis, we find that growth rates reach 95% of the asymptotic value when five categories or more are used (Fig. R1.2, right). Based on these considerations, the increase in basal growth rates noted between 30 and 50 categories in the model (Fig. R1.1) cannot be attributed to the ITD discretization alone. Our

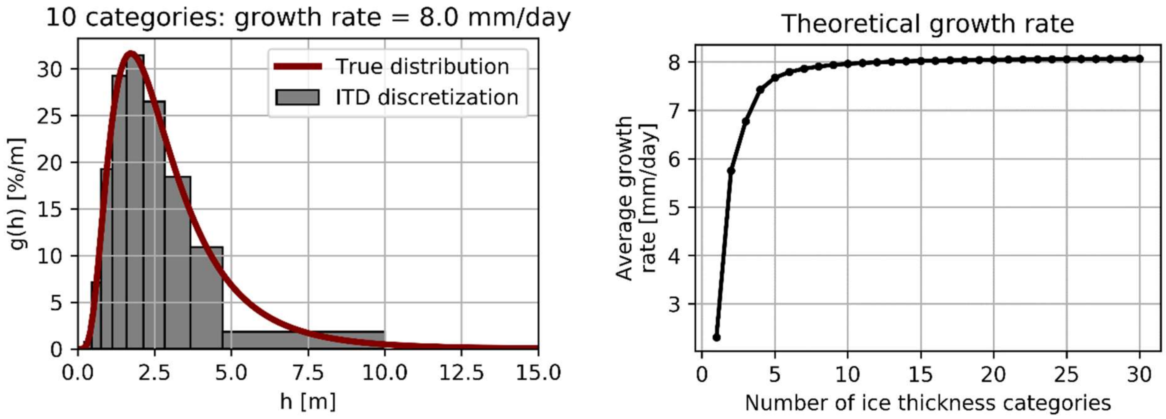

Figure R1.2. (Left) A supposed true ice thickness distribution in a model grid cell (red; log-normal with mean 3 m and standard deviation 2m) and its discretization in 10 categories following the default formulation of LIM3. **(Right)** Average basal growth rates for 1, 2, … 100 categories (only the first 30 are shown): for each category, the basal growth rate was computed assuming sea ice thickness equal to the category mean, assuming no snow, an atmosphere-ocean temperature difference of 30 K, sea ice conductivity of 2 W/mK, latent heat of fusion of 334 000 J/kg and sea ice density of 917 kg/m³. The growth rates were then averaged over categories, taking into account the relative area of each category.

hypothesis is that, in these simulations, basal growth compensates for the removal of thin ice by dynamic processes. However, it is not possible to produce deeper analyses as the ice mass balance terms are only available at the grid-cell level, not at the ice thickness category level (primarily because of storage space constraints).

We find that Fig. R1.1 is a good example of the added value of process-oriented diagnostics compared to simpler diagnostics like integrated sea ice volume. It also illustrates that a reorganization of ice production takes place among the various terms involved when going from one to more categories. We have, therefore, replaced Fig. 4 of the original manuscript by Fig. R1.1, which allows appreciating the sensitivity to selected processes (or the lack thereof) to the ITD discretization.

We finish by clarifying one point about the simulation with 100 categories that we mention in the manuscript. The reason that the simulation was not included in the manuscript figures is that it crashed after a few years and was finally deleted. Initial comparisons had indeed revealed larger volumes compared to 50 categories, as mentioned in the text. However, we propose to remove this sentence since the point of non-convergence can be established with 50 categories.

Action. We have made the following changes to the manuscript:

- Fig. R1.1 now replaces Fig. 4 of the original manuscript;

- We discuss in greater depth the origins of volume increases as the number of categories increases, confirming our initial findings that the chief reason for the increase is the enhanced basal growth rates. We also discuss the origins of increased dynamic production in the run with one category, along the lines of our explanations above.
- We discuss the possible origins of non-convergence of volumes beyond five categories (as predicted by theory, Fig. R1.2).

Minor comments. The article is very well written and very clear. I commend the authors for the care taken in creating the figures. Some other punctual comments and corrections can be found in the attached pdf.

Thank you. We now address these minor comments, which we copy/paste below for the sake of clarity.

p. 3, l. 26. Because of minus sign, this term is a convergence, not a divergence.

Action. "divergence" was changed to "convergence"

p.5, l. 21 "position ," → "position,"

Action. Change accepted.

p. 6, Fig. 1. "Weft"?

Action. "weft" has been changed to "set."

p. 6, Fig. 1. Shouldn't the experiments in this set be presented from lower number of categories (top) to largest (bottom) for consistency with panels S1 and S2?

Reply. Yes, this is a good idea.

Action. The order of experiments has now been inverted in this panel of Fig. 1.

p. 8, l. 6. [on the sentence: "even at 100 categories (not shown), the winter ice volume is significantly higher than with 50 categories"] *Isn't this a major source of concern?*

Reply. Indeed, this result would warrant further investigations. As explained in our answer to the reviewer's major comment, that 100-category run is not available for publication because it crashed before completion and was finally deleted. Still, the non-convergence until 50 categories remains puzzling. We conjecture that this result might be a consequence of the experimental setup, in which the atmospheric forcing is prescribed (offering no possibility for negative feedbacks to operate, or at least as not as strongly as they might do in a coupled model). Shortly, we aim to repeat the sensitivity experiments conducted in this paper with a coupled model (EC-Earth) to establish the robustness of this result in coupled mode. We finally note that we do not know other studies that conducted simulations with such a large number of categories. This non-convergence has, therefore, to be confirmed by subsequent studies using other models.

Action. We have added two sentences postulating that this result might be the consequence of our experimental setup and that it will require confirmation using other models. We have also deleted the reference to the run with 100 categories, as it is not available for further investigations.

p.12, Fig. 6. It's not so easy to distinguish between the OBS/REA and the S2.03 curves because their colours are very similar. Please use a better colour code.

Action. A new color code has been used following the reviewer comment.

p. 13, l. 1. All right. This is suitable for present climatic conditions, but it is probably correct to say that, for past climates (e.g., glacial times) or future warmer ones, these upper boundaries would be different.

Reply. We agree that the optimal ITD discretization is context-dependent, as is any tuning parameter of climate models.

Action. We have added a sentence to reflect this notion better.

p. 16, Fig. A1 (caption). I do not understand.

Reply. We mark a grid cell as part of the mask if its 1995-2014 monthly mean of sea ice concentration for March (Arctic) or September (Antarctic) is above 99%.

Action. We have clarified the caption accordingly.

p. 16, l. 11. Delete

Action. Deleted.

Anonymous Referee #2

The paper examines the impact of the discretization of the subgrid-scale Ice Thickness Distribution (ITD) on the evolution of sea ice in an ocean – sea ice model. Sensitivity experiments are discussed changing the number and the range of ice thickness categories. The authors find that the number of categories and the lower bound of the thickest category have an impact on winter ice volume of up to 30% in the Arctic and 10% in the Southern Ocean. They contribute this change to the larger basal ice growth rate in a better resolved ITD. Altogether, the authors conclude that the default ITD discretization with 5 thickness categories is recommended for large-scale climate application. The ITD is a key part of most sea ice models used for climate application and the best way to apply the ITD is a relevant scientific question. The applied model and the performed sensitivity studies are suitable to address this. With a few exceptions the paper is well structured and clearly written. The information provided allows the community to reproduce the presented experiments. While the impact of the number of ice thickness categories on ice properties has been studied in the past, the amount of sensitivity studies is novel. The key result that the default ITD discretization is sufficient confirms existing studies. The described impact of basal ice melt on ITD is known to sea ice modellers, but this has not been published in such a clear way beforehand. However, there are issues which need to be addressed.

Reply. We thank the reviewer for his/her constructive comments on the manuscript.

Major Issues.

From a principle point of view, the discretization of a distribution will become more realistic by increasing the number of categories. Here, Arctic sea ice extent and volume are most realistic for 1 category only and they become worse by increasing the number of categories. It is stated as a side comment that the total ice volume does not converge (higher for 100 categories when for 50 categories). This is worrying and mentioning that the sensitivity experiments have not been tuned and that there are uncertainties in the observations does not address this issue properly. A comprehensive analysis why the ice becomes thicker is required. It has been shown that the basal ice growth depends on discretization of the thinner categories, but the explanation for the increase from S2.07 to S2.09 (leaving more room for thinner ice) is not convincing and needs further evidence.

Reply. Regarding the statement that "Arctic sea ice extent and volume are most realistic for one category and they become worse by increasing the number of categories", we would like to draw the reviewer's attention on one point. While it is true that the run with one category displays a more realistic seasonality in ice extent and volume against observational/reanalysis references for Arctic sea ice, this is readily not the case for Antarctic sea ice. In Fig. 1 of the original manuscript, the simulation with one category largely overestimates the summertime Antarctic sea ice extent, unlike the simulations using more categories. Also, the Antarctic sea ice volume is more overestimated (underestimated) in summer (winter) in the run with one category than in the other runs. So, concluding that the 1-category run is the most realistic one is, to our view, an overstatement. Our scientific question in this manuscript is primarily about understanding the sensitivity of the model to the ITD discretization, and the purpose of showing observational references or reanalysis in Fig. 1 was to show that the model has a sufficiently decent seasonality to start these investigations.

Still, we agree with the reviewer (and with Reviewer #1, who raised a similar issue) that more in-depth analyses are required to understand what controls the increases in volume with a larger number of categories. As per the suggestion of the reviewer, we have conducted a detailed budget analysis to identify the nature of processes driving the ice volume changes from one simulation to the next. Namely, we have separated the thermodynamic processes from the dynamical ones and produced the seasonal cycles of ice volume changes in all simulations. Fig. R1.1 (above) confirms our initial hypothesis that most of the increases in volume with more categories can be attributed to enhanced basal growth rates. The role of melting processes is less clear.

Such process-oriented analyses also bring further evidence for our explanation that appending thick categories beyond 4 m and 2 m has a negligible impact on ice volumes in the Arctic and Antarctic, respectively. We show in Fig. R2.1 the same mass balance analysis as in Fig. R1.1 but for the experiments “S2”, i.e., those with successive addition of thick categories. Fig. R2.1 illustrates that the net gains in Arctic ice volume in fall (OND) and winter (JFM) level off after experiment S2.09. Breaking down the net gains by the processes involved, it appears more clearly that the reason for the stabilization is the convergence in basal growth rates. This finding confirms a posteriori our hypothesis that experiments S2.09 (S2.07) and beyond use an ITD discretization that allows a clear separation between deformed ice and thermodynamically-grown Arctic (Antarctic) sea ice.

The new version of the manuscript now includes a caution note on the meaning of Fig. 1, in particular regarding the results of experiment S1.01 (1 category). At this occasion, we have stressed that the realism of a simulation cannot be inferred solely from its agreement with observational references or reanalyses. As explained in our answer to Reviewer #1, the experiment with one ice thickness category has reduced basal growth compare to other experiments and has enhanced dynamic production compared to them. From a theoretical point of view, we know that growth rates cannot be realistic in a model without subgrid-scale thickness distribution since the subgrid-scale variability is missed by definition (and fluxes are nonlinear functions of sea ice thickness). The larger production of ice through dynamic processes in the one-category run might compensate for this shortcoming.

Action.

- We have replaced Fig. 4 of the original manuscript by Fig. R1.1. A discussion around that new figure, along the lines of our reply, has been added to the manuscript.

- We have replaced Fig. A2 by Fig. R2.1. This figure gives a better justification regarding the stabilization of volume increases after S2.09 and S2.07 for the Arctic and Antarctic, respectively. We have updated the discussion accordingly.

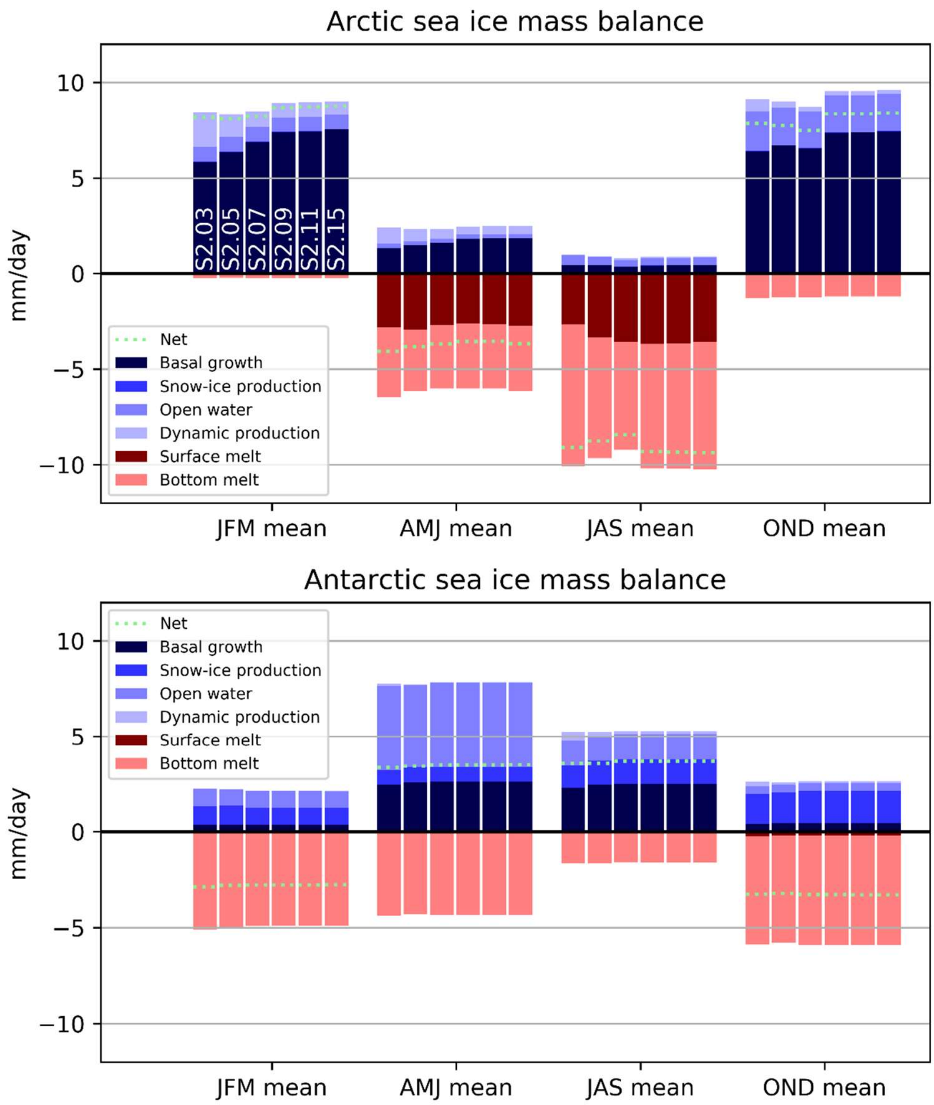

Figure R2.1. Contributions to the seasonal mass balance of Arctic (top) and Antarctic (bottom) sea ice as simulated by "S2" (appending new categories), averaged over the areas depicted in Fig. A1 of the manuscript. Blue colors refer to processes that contribute to *positive* ice volume changes, while red colors indicate processes implying *negative* ice volume changes.

While the Conclusions summarize the paper quite well, the abstract does not. In the abstract the key statement (default ITD discretization with 5 thickness categories is fine for large-scale climate application) is missing and the impact of ITD discretization is overstated.

Reply. This is indeed important information that needs to be stated to those readers who will not go through the paper. Regarding the point that "the impact of ITD discretization is overstated", we are not sure to understand to what sentence(s) the reviewer refers to. Our results indeed show that the ITD discretization has a large influence on the model mean state, perhaps more than we anticipated. Of course, the conclusions hold when all other factors are kept identical, and this needs to be better stated. Another possible source of misunderstanding is that we referred to the *sea ice* mean state. We do not claim that the ITD discretization can influence the climate mean state.

Action. We have re-written the abstract to include the recommendation that five thickness categories are fine for large-scale climate applications. We have made clear that the importance of the ITD discretization has to be appreciated compared to other factors (atmospheric forcing uncertainties, parameter uncertainty) as was done in the conclusion. We have also changed "mean state" by "sea ice mean state".

Minor issues

Page 1, Line 3: "how to implement" the ITD is too general given you only address the discretization.

Action. We have changed the sentence into "how to discretize it remains an open question".

Page 2, Lines 5-6: Melt ponds should be added.

Action. We have changed the relevant sentence into "To quote only three, the ice growth rate critically depends on the local thickness (Maykut, 1982), the albedo of a given region is largely dependent on the presence of open water and thin ice (Maykut and McPhee, 1995; Holland et al., 2006a), and the areal extent of melt ponds depends on the local topography of sea ice (Eicken., 2002)."

Eicken, H. (2002). Tracer studies of pathways and rates of meltwater transport through Arctic summer sea ice. Journal of Geophysical Research, 107(C10). https://doi.org/10.1029/2000jc000583

Figure 1: S3 panel: Typo for experiment name: S3.09 (not S2.09)

Reply. Experiment S3.09 uses an identical discretization as experiment S2.09 (as explained in the figure caption), but we agree that for the sake of clarity, it is better to use S3.09.

Action. All corrections have been done throughout the text.

Figure 7: I do not understand this figure: Why are only grey bars shown for n-1 categories? Why is the fraction per category divided by thickness, so the integral is not 1?

Reply. What we show in Fig. 7 is the sea ice thickness distribution from the model. We recall that, in the model, the upper bound of the last category is forced to be at 99 m. In Fig. 7, the last category contains ice, but the height of the grey bars are small. This is because, by definition of the function *g(h)* (in[%/m]), the product *g(h).dh* (in %) must be equal to the relative area occupied by sea ice with thickness comprised between the category limits (*dh* is the category bin width). Since the last category has a large value for *dh*, the function *g(h)* is relatively small for that category. In several panels, the grey bars are not visible for category *n* because of this small value.

Action. We checked that the sum of the grey bars amounts to 100% for each panel. For the sake of clarity, we have now added the relative areas occupied by each category on top of the grey bars, as shown in this figure:

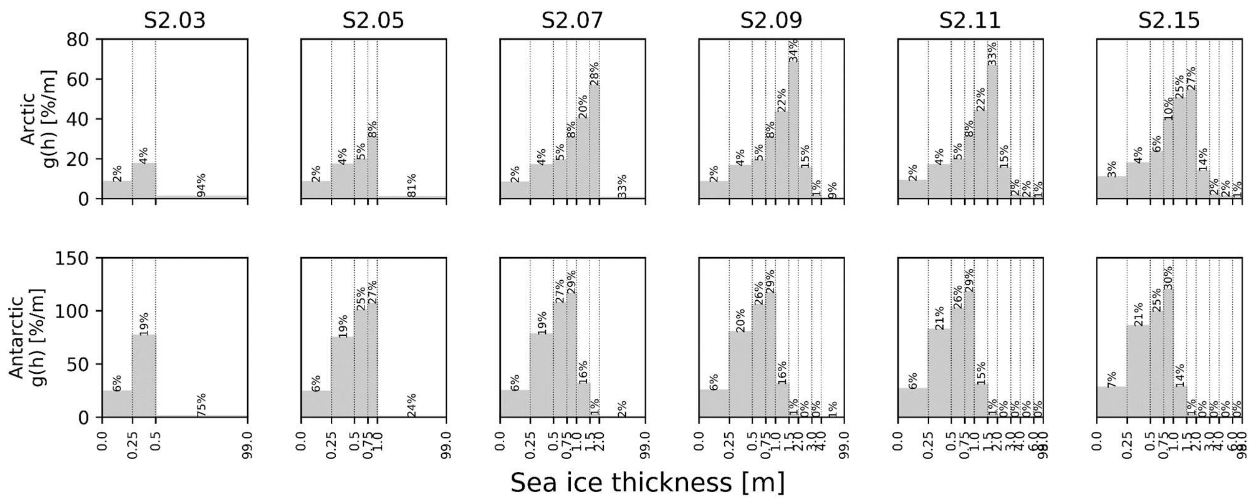

Figure R2.2 New version of Fig. 7 : ice thickness distributions for the experiments S2. The relative areas covered by sea ice in each category are now explicitly displayed.

Conclusions: better inclusion of literature mentioned in Introduction

Action. The Conclusion has been updated to mention the literature cited in the Introduction.

Figure A2: S3.09 (not S2.09)

Action. Changed.