Dear Chris,

Thank you for your quick response to our revised manuscript, and we are pleased that you are happy with our responses to the reviews. Our responses to your comments are included below in blue, and the majority of these have now been incorporated into the revised manuscript.

In addition, we have altered the larger tables so that all the manuscript pages are now in portrait orientation, as requested at the file verification stage. I think the tables are rather harder to read as a result, but hopefully the final 'typeset' version will look better.

Thank you for your work as editor of our paper. We have appreciated your quick responses and your understanding of the delays that have occurred during this year.

Yours sincerely, on behalf of all the authors,
Ann Keen.

Dear Ann – the concerns of the reviewers are clearly addressed in the revised manuscript. The study provides a comprehensive assessment of CMIP6 model behaviour with respect to sea ice, and is a highly relevant contribution to The Cryosphere. I really like your additions to the manuscript, which now more clearly identify relationships between model formulation and ice budget terms. I have identified just a few minor remaining issues to address (note that line numbers refer to the clean version of the revised manuscript with no tracked changes). Thanks,
Chris Derksen

The level of detail with respect to model descriptions is inconsistent across the Appendix (e.g. very little information on CanESM). Can this be harmonized somewhat?

We have now added a little more information about CanESM, and shortened some of longer model sections so that they are of a more consistent length and level of detail.

Line 124: consider changing to: "Snow ice: ice formation due to the transformation of snow to sea ice due to surface flooding"

This has been changed as suggested.

Line 287: "If we consider the total amount of winter ice growth (here taken as the sum of the frazil and basal growth terms), the spread in modelled values is 3.9x103Gt, compared to the larger range of 5.9x103 Gt for the basal growth alone." Can you provide a brief explanation for the apparent compensatory behavior for frazil formation versus basal ice growth?

The following text has been added: The lower the value of the minimum frazil ice thickness, the more quickly the frazil ice growth can transition to basal growth.

Line 309: can you remove the phrase "likely to be"?

This has been removed.

Line 341: “a simple scheme to account for the loss of drifting snow”. I think a reference should be added here to Lecomte, O., Fichefet, T., Flocco, D., Schröder, D., and Vancoppenolle, M.: Impacts of wind-blown snow redistribution on melt ponds in a coupled ocean – sea ice model, Ocean Model, 87, 67–80, https://doi.org/10.1016/j.ocemod.2014.12.003, 2014.

Actually, it not the Lecomte et al. 2014 snow redistribution scheme, but the one described in Schroeder et al. (2019), which is referenced at the end of the sentence.

Line 350: I don’t see a description anywhere of DFS versus CORE forcing?

We now include the following text: Replacing the CORE forcing which is based on NCEP reanalysis (Large and Yeager, 2009), with DFS forcing based on ERA-interim reanalysis (Dussin et al, 2016), increases the top melt (Fig. 8c) and decreases the basal melt (Fig. 8a), resulting in higher sea ice area and mass.

Line 396: specify that “The remaining models have a minimum frazil ice thickness…”

This has been changed as suggested.

Line 449: consider changing the header “Dynamics” to “Ice Advection” for consistency.

This has been changed as suggested.

Line 556: perhaps add a statement that insight on ice advection from the projections is limited because analysis of winds and large-scale atmospheric circulation (and hence simulated ice motion) were out of scope for this study.

We have added the following sentence: Further insight on the changes in ice advection would require an analysis of winds and large-scale atmospheric circulation, and the associated ice motion, which is outside the scope of this study.

Line 591: “Overall, models with the largest decline in basal ice growth by the end of the 21st century tend to be those with the larger decline in winter ice cover.” In Section 6.1 and Figure 10c, you discuss and show the changes also as a function of ice unit area. Is there value in including this perspective also in Figure 12 (as noted on line 621)?

We have added a reference to Figs. 2a and 12a at the end of the sentence mentioned. When revising the manuscript, we did consider retaining the ‘per ice area’ plots for all the models – essentially figure 9 in the original version of the manuscript, but for decadal rather than annual mean values, but in the end we did not feel that this figure added significantly to the discussion. Once we had generated the new fig 10c using the multi-model mean, this seemed to more clearly demonstrate the points we wanted to make.

Non-public comments to the Author:
Thanks for the revised manuscript, Ann. This is a very comprehensive study and an important analysis of CMIP6 models. I identified just some minor things to clean up…
Thanks for your contribution to The Cryosphere.
Chris