

Dear Till,

Thank you for taking the time to read our manuscript and for your useful and constructive comments and suggestions. We are pleased with your positive review, and have incorporated many of your suggestions, which have helped to improve the manuscript.

Our responses to your comments are included below in blue, together with details of how we have incorporated these into the revised manuscript

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

Interactive comment on “An inter-comparison of the mass budget of the Arctic sea ice in CMIP6 models” by Ann Keen et al.

Till Wagner (Referee)

Received and published: 1 June 2020

In this manuscript, the authors compare the individual terms of the Arctic sea ice mass budget (predominantly surface and bottom melt, basal growth and frazil ice formation, and advection) from 14 CMIP6 models. This type of in-depth analysis had not been possible for previous CMIPs, as individual mass budget terms were not routinely reported.

The paper is very well written and structured, clearly illustrated, and the subject matter is a good fit for The Cryosphere. It presents an interesting result in that ~half of the simulated annual ice loss is due to basal melt, and ~one quarter each due to surface melt and advection out of the Arctic. Another central result is that ice formation occurs predominantly as basal growth, with frazil ice formation playing a substantial role depending on a model’s minimum frazil ice thickness. Finally, it is striking to see how consistent the partitioning of the individual mass budget terms is between models.

In light of this I recommend the paper for publication after minor revisions.

I agree with the other reviewer with regard to two general comments:

1) I found the paper could do with some more focus, and investigate model differences in more detail. In particular highlighting which differences are due to different physics (e.g., meltpond or radiation schemes) and which are due to different parameter values (such as minimum frazil ice thickness) would be of interest.

This brings me to my main comment. Many of the models share the same sea ice model components (CICE, LIM, SIS) and I believe it would be of interest what the differences are between models with the same sea ice component vs between models with different sea ice components. In the case this is not insightful, it would nevertheless be helpful to discuss the role of having shared sea ice components or not (e.g., for a reader like me who is keenly interested but no expert in the differences between sea ice model components).

2) In line with the other reviewer’s comment, I found the section on the forced model runs somewhat vague and only tangentially related to central message of the paper. I would also suggest that this section be either incorporated more carefully or cut (which would further help focusing the main story).

In response to both your general comments (and those of the other reviewer), we have now included a new section 'Understanding differences between the CMIP6 models' (Section 5), where the forced experiments are introduced and used to help understand differences between the model mass budgets during the reference period. We have also looked more closely at the model budget differences and identified links between some of the budget terms and the formulation of the sea ice component.

Specific Comments:

I.65 "uncertainty" (not uncertainly)

I.68 " models' " (not model's)

I.74 maybe "emerging consensus" rather than "increasing appreciation"

These changes have been made.

I.135 what time periods are covered by the 3 observational products?

For the seasonal cycles shown in figure 2 (figure 1 in the revised manuscript), we use the years 1990-2009 for each observational dataset.

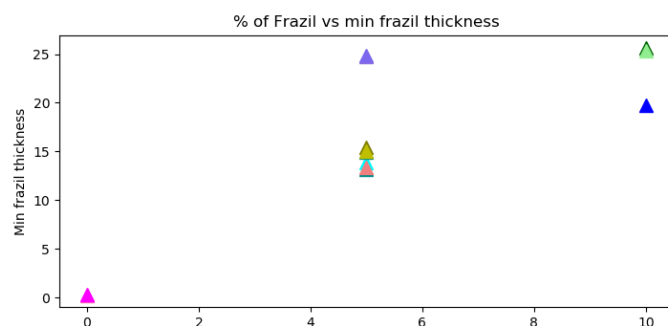
For the trajectories shown in figures 3 and 4 (figures 2 and 3 in the revised manuscript), we now use the following time periods:

- OSI-SAF, HadISST1.2 and HadISST2,2,0,0: 1979-2015
- PIOMAS: 1979-2018
- HadCRUT4: 1960-2019

This is now clarified in the manuscript.

I.263 Is this a linear relationship? Would it be insightful to plot "% of frazil ice formation" vs "min frazil ice thickness"

That is an interesting question. There is some evidence that it may be a linear relationship (see plot below). However, since we submitted this work the term representing frazil ice formation in the MRI-ESM2 model has been added to the basal growth term, which means we no longer have a frazil ice term for a model with a minimum frazil thickness of 20cm. So as there are not many data points we have not focussed on this in the text.



Plot of frazil ice formation (as a percentage of total ice growth) for the reference period 1960 to 89 for each model, plotted against the minimum thickness of frazil ice formation.

I.309 period at end of sentence

I.350 "amount" (not about)

The test in this section has now been updated, and these mistakes corrected.

Fig 4 would it make sense to add the observations?

Yes, we have now added observations to Figure 4a. (figure 3 in the revised manuscript).

Figure 4b has been removed, as we instead reference the recently published SIMIP community paper: SIMIP Community (2020). Arctic seaice in CMIP6. Geophysical Research Letters, 47, e2019GL086749. <https://doi.org/10.1029/2019GL086749>

Fig 5b add legend

A legend has been added. (figure 4b in the revised manuscript).

Fig 6 It is difficult to see the differences between the models. Is there a more concise way to present this data? In line with my major comment above, maybe it would be worth grouping the models by sea ice model component? e.g., instead of the bar plots have one subplot for all CICE models, one for all LIM models, etc, with each model's basal growth value indicated by a marker and the same for the other terms?

Fig 7 As for Fig 6 it is difficult to see differences between models.

Figures 6 and 7 have now been updated (figures 5 and 6 in the revised manuscript), and hopefully are now clearer. Thank you for the suggestion to group the models. A new figure 9 shows the main budget terms grouped according to a number of key model parametrizations and settings, and this has proved very useful in helping to understand some of the budget differences.

Fig 9 and 10 are cropped on the left edge.

Fig 9 doesn't show units on vertical axis, Fig 10 does.

Fig 9b is "surface melt", Fig 10b is "top melt".

These figures have been replaced in the updated manuscript. and Fig. 9 is replaced by the new Fig. 10c, which shows the multi-model mean values per unit area of the ice., and Fig 10 is replaced by the new Fig. 12. We have checked to ensure that the units are noted in each case, that the figures are not cropped, and that the headings are correct.

Fig 9a is "basal growth", Fig 10a is "frazil ice formation". Why are the different quantities shown, why not all 4 main terms for each Figure?

Yes, we realise now that this was rather confusing. The figures for this section have been altered, and the evolution of the budget terms is now shown for all the main terms (new Fig. 12). The values per unit area of the ice are now shown for the multi-model mean (new Fig. 10c), and only for those processes that occur over the ice surface. This is explained in the caption.

Fig 11 are the units on the vertical axis Gt year^{-1} rather than kg year^{-1} ?

Yes, thank you for spotting this. It has now been corrected (new Fig. 14)

Fig 12 maybe leave out the lines for lateral melt, snowice, evapsubl, since they are mostly negligible and make the figure harder to read?

Thank you for this suggestion. We have now removed these smaller terms from all the figures apart from figure 4 (where the budget is first introduced). This is a definite improvement.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-314>, 2020.