Seasonal Sea Ice Prediction with the CICE Model and Positive Impact of CryoSat-2 Ice Thickness Initialization
Shan Sun and Amy Solomon

In this paper the authors analyse the seasonal prediction skill of a stand-alone CICE model forced/initialised using CFSR. The study is bipolar, although there is a much stronger focus on the Arctic. The authors show that initialising the model with observed sea ice thickness inferred from CryoSat-2 radar altimetry considerably improves the forecast skill, as has been shown previously for other models in other studies (as they correctly point out).

The manuscript is relatively well written and presented and the results will be of interest to the community. Therefore I think it worthy of publication in The Cryosphere.

However, the figures could do with a bit more attention in relation to figure captions and colourmaps. Furthermore, the study could be better motivated, and the discussion of the figures/results is often rather on the shallow side. I therefore recommend that this manuscript requires considerable revision before it is accepted for publication here.

We thank the reviewer for their careful reading of the manuscript and many constructive suggestions, including a detailed edit in the pdf file, which helped improve the quality of the manuscript. We have incorporated them in the revision, see details below (reviewer's comments in black, our replies in blue).

While investigating the origin of the low concentration line north of Franz Josef Land in Fig.4, we found a bug in the namelist used to run CICE. We have rerun all the experiments with the corrected namelist. The revised results don't change the previous conclusions, but do alter figures at different magnitudes.

Particular points

A detailed list of comments can be found in the attached pdf document but I highlight here a few points that will particularly need addressing.

the study needs to be a bit better motivated. The main motivation I can see for the study is lines 36-42 which states that fully coupled (AOIL) models are "considered the ultimate tool" (which incidentally would be considered an insult here!) for sea ice seasonal prediction but here the stand-alone model is used in order to "separate various feedbacks among the components of a fully coupled model". However, this separation of feedbacks is not done in the ensuing manuscript! It is also not mentioned anywhere (albeit a trivial point) that the stand-alone approach is much cheaper.

We have made this clearer in the revised manuscript:

"In order to separate various feedbacks among the components of a fully coupled model, in this study we aim to evaluate the seasonal prediction skill of CICE in a standalone mode with prescribed atmospheric forcings. It is essential to assess the skills of each module separately in uncoupled mode before assembling them in a fully coupled model, as it is easier to reveal the strength and weakness of a module in such a controlled environment. This work is even more relevant as CICE, originally developed for climate research, is to be used in the S2S prediction in operation."

there is no consideration of internal variability, which is a huge factor for sea ice and in polar regions generally, or significance. Many of the figures contain means of multiple years of model runs, which could also include error bars or shading to help understand the impact of internal variability (or at least inter-annual variability over the study period). Likewise hatching could be added to difference plots to try and portray to the reader how significant the changes are in relation to natural/chaotic differences.

We have redone Fig.1 with an all-year mean and standard deviation to show the inter-annual variability over the 7 years. Now it is rewritten as:

"The Arctic SIE forecast matches the NSIDC observations better in the warm season than in the cold season both at 0.5-month and 5.5-month lead times, and the positive bias is biggest in winter. The biggest interannual variabilities in the predicted Arctic SIE occurred in September as shown by the standard deviation."

the CryoSat-2 data, and the way that it is used to initialise the model, are poorly described and so I am left wondering whether things have been done sensibly. There is no mention of what happens with thinner ice (for which CS-2 errors are near-infinite!) and no mention of what is done with the snow on top of the sea ice. Furthermore, it looks like they have not been very careful with their QC because the CryoSat-2 "pole hole" appears as open water in the sea ice concentration for their "alt-init" runs!

We didn't realize these issues in the CryoSat-2 dataset and thus didn't perform QC. The snow on top of the sea ice is prescribed as a part of the atmospheric forcings. We have filled the "pole hole" in the CryoSat-2 and added the following to the manuscript:

"The ice thickness from the CryoSat-2 is treated as one ice thickness category at each model grid in the CICE initialization. For grid cells starting with zero ice thickness in the CryoSat-2 data, the sea ice area from CFSR is reset to zero initially to be consistent."

>95% of the article is focussed on the Arctic but with approx. 4 sentences and a 1-panel figure on the Antarctic, which feels a bit orphaned within the bigger picture of this manuscript. I think the authors should drop the Antarctic and limit the scope of this study to focus on the Arctic only - particularly given that the impact of SIT initialisation cannot be evaluated there, which is actually the second half of the manuscript title!

The goal of the project was to evaluate sea ice forecasts both in the Arctic and Antarctic. We have added more discussion on Antarctica in Fig.1, in addition to Fig.3. We think there are some values in the discussion on Antarctica although we wish we could have more data to validate forecasts in the Antarctic.

the results are often only described in a very shallow way without any mechanisms or processes being given. For example, the increased basal & top melting for the runs with thinner sea ice is not obvious and so the mechanisms should be talked about
there is general confusion between 1D and 2D sea ice variables/quantities in the figures and accompanying text. For example, sea ice "extent", "area" and "concentration" seem to be used interchangeably and so are "thickness" and "volume"
many of the titles, legends and colourmaps used in the figures are not intuitive for the reader. there are also some 'rainbow' colourmaps, which are also problematic for people who suffer from colour-blindness.

(1) We have added discussion on increased basal melt:
"The biggest difference is a larger basal melt in the Arctic region between 120°E and 120°W in the alt-init experiments, where the ice thickness is mostly below 1.5 m compared to above 1.5 m in the control experiment. The basal melt pattern is consistent with the fact that the conductive heat flux at the ice bottom is inversely proportional to the ice thickness (Hunke, et al. 2015)."

(2) We fixed an error in the unit in Fig.7(c): the tendencies are measured in "%/day" for ice area and "cm/day" for ice thickness.

(3) We now distinguish 1D and 2D sea ice variables according to their unit throughout the manuscript:
   - sea ice extent and volume are 1D variables in the unit of m^2 and m^3
   - sea ice area and thickness are 2D variables in the unit of % and m

(4) We have replaced the rainbow colorbar in Figs. 4, 5, 6 and 8 by color-blindness friendly colorbars as much as possible.

Comments in Supplement:

Thanks for your feedback on the pdf file. It is greatly appreciated. We have addressed all your comments - some may be redundant with above.

Line 7: added "this bias"
Line 8: Is this true? The rate at which sea ice melts altogether is lower of course (as there's more ice) but is it true that surface and basal melting rates are impacted? Obviously basal growth rate is hugely dependent on thickness but how is melting? Can you describe the mechanisms?

Unlike the basal melt, the top melt is not directly related to the ice thickness. Some surface properties like long wave emissivity and aerodynamic roughness are a function of the ice thickness. We have revised the text in the abstract as follow:

"In addition, thicker ice has a lower basal melting rate in the warm season, contributing to this positive bias."

and added discussion on mechanisms in Fig.8:

"The biggest difference is a larger basal melt in the Arctic region between 120°E and 120°W in the alt-init experiments, where the ice thickness is mostly below 1.5 m compared to above 1.5 m in the control experiment. The basal melt pattern is consistent with the fact that the conductive heat flux at the ice bottom is inversely proportional to the ice thickness (Hunke, et al. 2015)."

Line 10: Changed "indicates" to "confirms"
Line 14: Removed discussion on ice area reduction in Antarctica.
Line 21: Changed to "in mid-latitudes"
Line 28: Changed to "predictive skill"
Line 31: Changed to "sea ice volume has more persistence than sea ice area"
Line 40: We are now referring to Hunke et al. 2020, as it included more information than in Tonani et al. 2015, and removed Shaffrey et al. (2009).
Line 50: We use ktherm=1, and have removed "mushy physics".
Line 68: Do you have a paper reference for these observations. I'm surprised that in Figure 1 your CS2 volumes are lower than for the PIOMAS model when many other studies have shown the opposite. For example Figure 3 of Laxon et al. (2013) [https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/grl.50193] and Figure 2 of Tilling et al., (2015) [https://www.nature.com/articles/ngeo2489].
The latter of these papers includes some overlap with the period that you are running here and the CS2 estimates look much closer to your model!

Are the AWI CS2 data really that different from the CPOM data? Most papers I've seen have said they are 'similar'!

We have replaced Fig.1 using the all-year mean and the standard deviation, and recalculated the Arctic SIV values from the CryoSat-2 data on its native grid instead of on the CICE model grid. That is why the Arctic SIV from the CryoSat-2 here is lower than in the original version.

As for the lower-left panel of Fig. 1,

(1) The CryoSat-2 values in Fig.1 are consistent with Laxon et al. (2013)
(2) The PIOMAS values in Fig.1 are consistent with what is shown at the PIOMAS website. It is unclear why the PIOMAS values in Laxon et al. (2013) are lower than what is shown at the PIOMAS website.

We compared the SIV from CryoSat-2 and PIOMAS during 2011-2017, see Fig.s3 attached, where PIOMAS is mostly higher than CryoSat-2 in winter and spring. The two datasets are comparable in fall.

Line 69: What do you do with the snow cover? Does that just stay as it is in CFSR?
The snow is arguably more important than the ice itself - especially for the evolution of the ice pack through winter (snow conductivity ~10x ice conductivity) but also for the onset of melting (higher albedo).

The snow cover is prescribed using CFSR as a part of the atmospheric forcings. The snow melting rate is added to Fig. 7, where the snow melt peaks one month earlier than the melt from top, basal and lateral.

Also how do you account for the fact that CS2 are not valid for thin ice (errors tangent to infinity as shown by Ricker et al., 2017, https://tc.copernicus.org/articles/11/1607/2017/).
Is the CS2 data you use QC'd with thin values removed or do you use them despite the high errors? If the former then how do you ensure not to set the SIC to zero too?
It might have been better to have used the blended CS2+SMOS product of Ricker et al.?

We were unaware of the QC issues with the CS2 data for thin ice and therefore used it as is. Sea ice area is set to zero at all points with zero ice thickness initially to be consistent. We will certainly use the blended CS2+SMOS product of Ricker et al. in the future application.

Line 71: It looks like you might have a problem with this approach because it doesn't seem to differentiate between areas where the CS2 thickness is not defined and where it is actually zero. By this I mean that you have a "pole hole" in Fig 4 (b) that is not in 4 (a) and so much be caused by the mechanism.
So as well as starting with thinner ice you will also have lower initial extent and more local ocean heating through the summer?

(1) We have filled up the missing value at the “pole hole” in the CS2 data.
(2) For other missing data in the CryoSat-2 dataset, we treated ice thickness to be zero, and then set the ice area to zero at all points with zero ice thickness.
(3) The errors in CS2 could contribute to the model bias as well. We have added this to the manuscript.

Table 1, This confused me a bit because the table is the transpose of what I'd expect.
Done. Now we managed to fit 4 columns across the width of the page.

Line 91: How is this calculated? Are you calculating the basin-scale extent (SIE) for each dataset and then calculating the RMSE of the yearly values for each month? Or are you accounting for spatial variability (similar to IIEE)?

Integrated ice-edge error (IIEE) is a very informative metric. In this study, we used the basin-scale SIE and SIV values in the standard CICE model output, as we find these values represent the messages we try to deliver here. As shown in Fig.s4, since both model experiments tend to overestimate the ice edge along the Arctic periphery, there is either a positive or near zero bias at each longitude. Thus the basin-scale SIE is a valid metric to use in this application.

Line 95: I would expect this to be driven by biases in the ocean given that the winter ice edge is controlled by the SSTs (i.e., how far south the ice can get).
One of the primary mechanisms for Arctic winter sea ice melting is northward heat transport into the GIN & Barents Seas.

Do you think this is likely caused by the fact you run the model stand-alone with only a mixed-layer ocean (i.e., no horizontal transport)?

If so (or anyway!) it would be good to include mention of this.

We agree and have added this:

“It is most likely due to the lack of interaction with the Atlantic Ocean, the mechanism that is missing in this experiment by using a simplified mixed layer ocean model mentioned earlier. Without northward oceanic heat transport from the Atlantic, the sea ice edge tends to extend too far southward in winter.”

Line 103: Lines 103-106 and Figure 3 represent the entirety of the Antarctic sea ice evaluation in this study. Much of what is said is that the observational coverage is too poor to do more (SIV anyhow).
So my obvious question is whether this brings any value to the paper?

My thoughts are that it might be better to drop these 4 lines and Figure 3 and focus the paper on the Arctic. Particularly given half the title (i.e., "Positive Impact of CryoSat-2 Ice Thickness Initialization") is not relevant to the Antarctic?

We added more discussion on Antarctica in Fig.1, in addition to Fig.3. We think there are some values in the discussion on Antarctica although we wish we could have more data to validate forecasts in the Antarctic.

Line 104: Perhaps similar mechanisms are at play here to the Arctic winter - i.e., ocean warming?

We have added this:

“It is possible that the missing mechanism in the ocean transport contributes to the RMSE in the Antarctic SIE.”

Line 130: Can you say a little about how these SIE tendencies are calculated? I know that CICE includes tendencies for sea ice area caused by dynamics (daidtd) and thermodynamics (daidtt). Do you convert these to extent somehow? Or start from scratch?
Or are you really plotting the SIA trends (which would be more informative anyhow)?

Either way the caption/units are wrong because "cm/day" is only relevant for volume (or volume per unit area!).

Thanks for pointing out the wrong unit in Fig.7(c): the tendencies are measured in “%/day” for ice area and “cm/day” for ice volume per unit area. The area tendency from dynamics and thermo-dynamics (daidtd and daidtt) and the volume tendency from dynamics and thermo-dynamics (dvidtd and dvidtt) used here are directly from the model archives.

Line 133: I presume you are just plotting CICE's "meltt" here for top melting (and meltl & meltb for others)? If so you should consider adding the snow melt ("melts") to the plot or combining meltt & melts to plot a total top melting.
Because generally the ice doesn't start melting (i.e., meltt=0) until the snow is gone.

Exactly. We added the snow melting rate in addition to top, basal and lateral melting rate in Fig.7, all monthly mean and averaged north of 67.5ºN. Their variable names are melts, meltt, meltb and meltl, respectively from model archives. As expected, the snow melt peaked in June, while the rest peaked one month later in July.

Line 149: Are you basing that on the PIOMAS comparisons in Fig 7? If so the wording is a bit strong because PIOMAS is itself only a model. Also (as noted elsewhere) several studies have suggested that PIOMAS underestimates the volume of late winter Arctic sea ice when compared with CS2 radar altimetry (see Laxon et al., & Tilling et al. references from earlier comments).

(1) We added a sentence emphasizing that PIOMAS is a reanalysis product.
(2) As mentioned earlier, attached Fig.s3 shows that compared to CS2, PIOMAS often overestimates SIV in winter and spring and is comparable in fall. The CryoSat-2 values used here are consistent with Laxon et al. and the PIOMAS values used here are consistent with what is shown at the PIOMAS website.

Lines 155, 157 & 161: change “basin” to “region”
Done.

Line 165: CS2 errors in the BKG region might also be relatively higher? This area will contain a mixture of new ice alongside old/thick ice. In particular the Barents Sea is also impacted by storms and is the area of the Arctic where snow-ice formation occurs. Such conditions can cause problems for the CS2 observations because it impacts the radar penetration properties (i.e., radar freeboard vs true freeboard).

Thanks for the information.

Line 182: I struggle a bit here to see what the relevant mechanisms are here - partly because these processes are not really addressed in the text.
What are the mechanisms that might cause more surface/basal melting with thinner ice?

In many ways basal meting and surface melting are largely independent of ice thickness.

On the other hand it's quite obvious that if you have less ice then you might have lower melting fluxes - because it will require less melting (and in fact be able to take less melting) before it melts-out completely.

So what are the mechanisms here?
Is it the case that reduced ice concentration/cover is allowing more in-situ ocean warming (radiative and/or conductive), which will amplify the basal melting?

Or is this related to the temperature profile in the ice - thicker ice can support a larger thermal gradient, which needs to be warmed to freezing point before melting can occur?

The conductive heat flux at the bottom surface is inversely proportional to the ice thickness, as shown in Equation (72) on Page 33 of the CICE document. Unlike the basal melt, the top melt is not directly related to the ice thickness. Some surface properties like long wave emissivity and aerodynamic roughness are a function of the ice thickness. We have revised the text by focusing on the basal melting rate. We also added a plot of ice concentration to show that a lower ice concentration in the alt-init experiment could amplify the basal melt. As seen in the attached Fig.s5, in the region where the basal melt differs most, the ice thickness changes a lot and there is a difference in the ice area up to 15% in the southern part of this region as well. The revised text now says:

"With the same prescribed atmospheric forcings applied to both experiments as discussed in Section 2.1, a relatively thinner ice in the alt-init experiment favors a higher basal melt rate than in the control experiment during the month of July, suggesting an important role of a proper ice thickness plays in the basal melting process. In addition, a lower ice concentration up to 15% in the alt-init experiment would allow more in-situ ocean warming, which would amplify the basal melting."

Line 185: I strongly suspect it's one (or a combination) of the 1st two!

We added this:

"It is most likely due to the lack of interaction with the Atlantic Ocean, the mechanism that is missing in this experiment by using a simplified mixed layer ocean model mentioned earlier. Without northward oceanic heat transport from the Atlantic, the sea ice edge tends to extend too far southward in winter."

Line 191: Can you explain more? In many cases coupled feedbacks would be expected to increase RMSE and model drift!

Indeed, relaxation methods are often used in models to relax boundary conditions to observations in order to prevent models from drifting away. In this case, the prescribed atmospheric and ocean boundary conditions are model simulations, which can contain errors and thus lead to a bias in the sea ice model. When the error occurs in the sea ice model, it tends to stay in the model as there is no feedback mechanism in the uncoupled model to get rid of it.

We added this:

"this study uses an uncoupled sea ice model with prescribed atmospheric and ocean boundary conditions from model simulations, which may contain errors and are not always 'in tune' with the sea ice model."

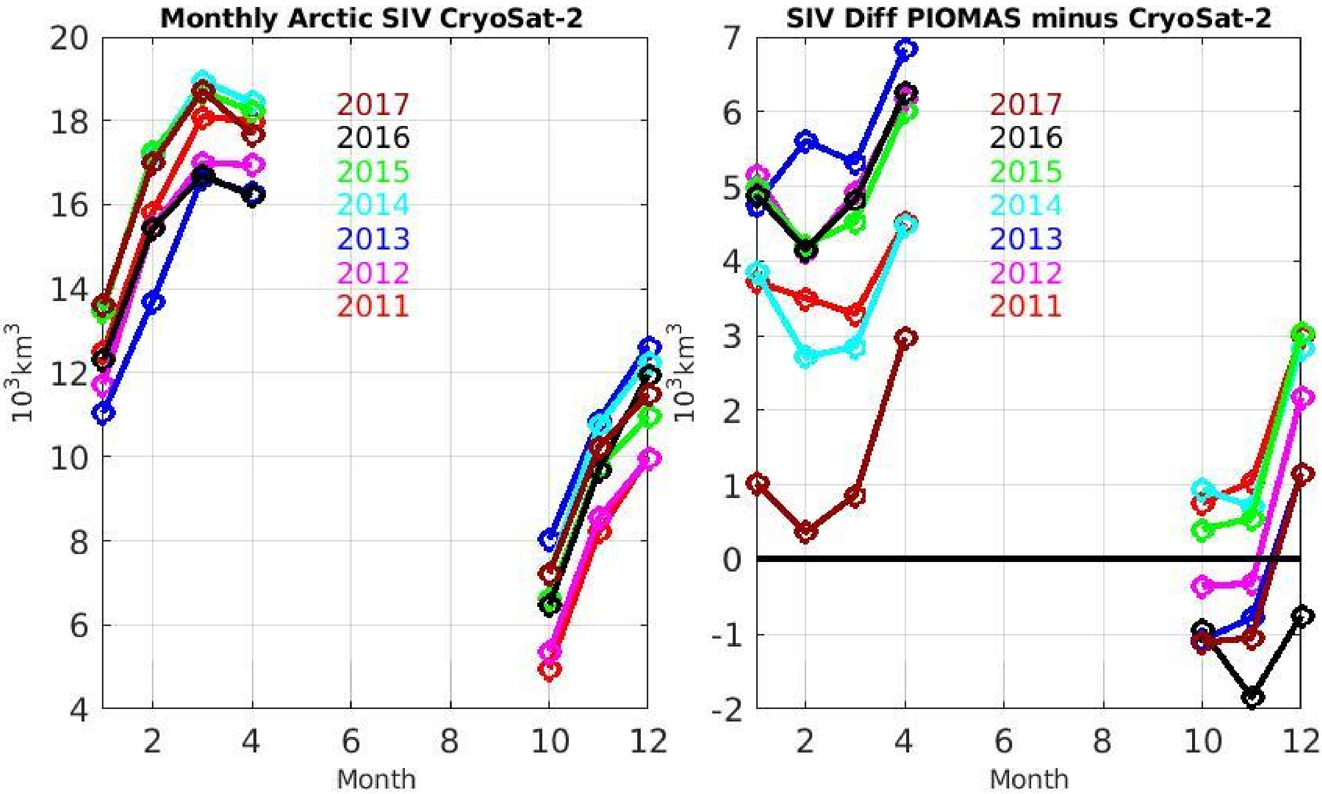

Fig.s3: Arctic sea ice volume in CryoSat-2 (left) and the difference of PIOMAS and CryoSat-2 (right) from 2011 to 2017.

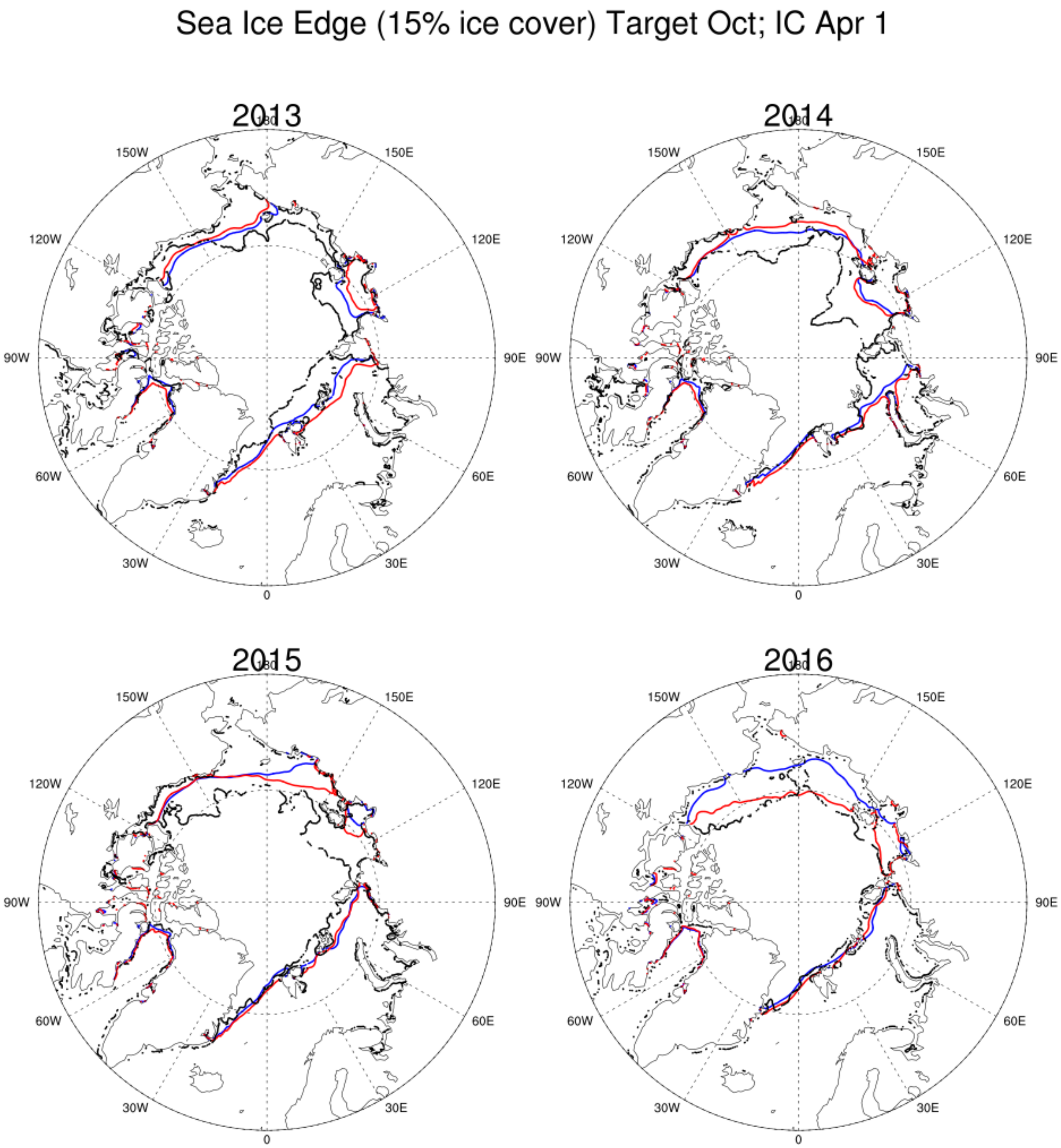

Fig.s4: Sea ice edge (15% sea ice concentration) from control (blue) and alt-init (red) experiments in October 2013 - 2016, from models initialized on April 1 each year, and AMSR2 (black).

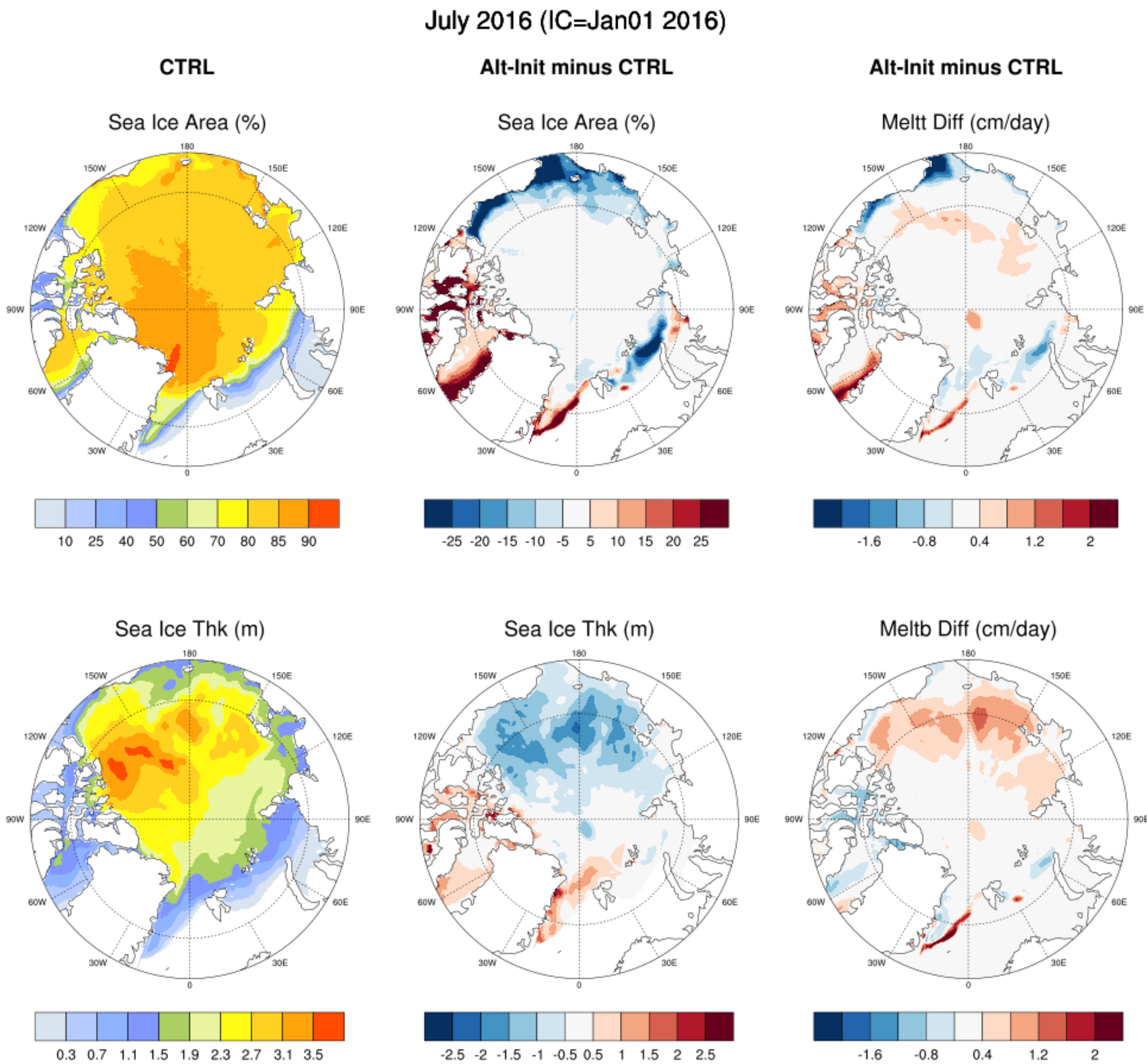

Fig.s5: Monthly mean Arctic SIA (%) and SIT (m) in the control (left), their differences (middle) and the melt difference (cm/day) at the top and basal (right) between the alt-init and control experiments. All valid in July 2016 initialized on January 1, 2016.