Review for "Warm Winter, Thin Ice?" by Stroeve et al.

We thank the reviewer for their thoughtful comments and our responses are shown in red below.

Summary

This paper uses model simulations from the Los Alamos sea-ice model (CICE) and CryoSat-2 thickness estimates from three different data providers to investigate the impact of the 2016/2017 anomalously warm winter on Arctic sea ice thickness. The authors consider free CICE simulations as well as CICE simulations initialized with CryoSat. Coinciding with the least amount of freezing degree days north of 70N since 1979, the authors find that CICE simulations in April 2017 show the thinnest ice cover in the Arctic Basin over the CryoSat-2 data period. However, this finding is not entirely supported by the satellite retrievals. CICE simulations are also used to investigate the processes leading to ice thickness anomalies, separating dynamic and thermodynamic contributions. It is concluded that free CICE simulations from 1985 to 2017 reveal that the correlation between winter ice growth and November ice thickness is stronger than between growth and FDDs, although this correlations has become weaker since 2012, and delayed freeze up due to warmer winter temperatures play a bigger role.

General comments:

The impact of warmer winter seasons on the Arctic ice cover is of high interest for the sea ice and climate science community. In addition, the comparison between sea ice thickness retrievals from different providers adds some valuable information here. The manuscript itself is well written, but there are lots of information in the figures and tables which are not easy to capture. For example, color bars in Figure 4 show different scales, which is a bit confusing. Also the quality of the figures in general can be improved. See more detailed comments below.

Apart from that, my major concern is that it is **not really well explained how reliable the model simulations are**, both CICE free and CICE initialized with CryoSat. Although the mean monthly values seem to fit quite well to the satellite observations, considering Figure 3 and Figure 5, **regional anomalies disagree quite significant in several cases**. For example, the significant positive thickness anomaly north of the Canadian Archipelago in April 2014 and 2015 is rather weak in the model simulations. I don't think that this is due to the usage of a snow climatology in the satellite retrievals, since this area is mostly covered by multiyear sea ice. I also wonder why this strong positive anomaly is not present at least in the CICE simulations initialized with CryoSat. Based on these concerns, I also wonder **how reliable the findings and conclusions regarding the results presented in Figure 9** are. Could you include the satellite observations here as well? Also difference maps and scatter plots between simulated ice thicknesses and CryoSat ice thicknesses would be interesting and could potentially help to support the conclusions and show more explicit the limitations of the model simulations. For example, how meaningful are the correlations given in the maps of Figure 9 if the model is limited in reproducing regional anomalies as described above?

Local and to a lesser extent regional results from our model simulations are affected by a variety of uncertainties, including slightly shifted location of moving cyclones can result in wrong pattern of ice drift and ice divergence, and reanalysis precipitation likely has biases as well. Thus, we do not believe, nor do we state that all the small regional features shown in

the maps in Fig. 4 to 6 are realistic. At this scale we are only confident for regions where CryoSat-2 products and CICE simulations agree (see original paragraphs lines 263-285). In Figure 9, however, we are looking at an Arctic Basin wide mean. For the Arctic Basin wide mean, thermodynamic processes are dominating over the dynamic processes (see Table 3) and the thermodynamic winter ice growth has been tuned successfully to agree with the Cryosat winter ice growth. Thus, our results on this scale are reliable as further demonstrated by Fig. 1b. There are no satellite observations of ice thickness available which cover a period of more than 30 years and thus, it would not be correct to use those for Figure 9 as the time-period is simply too short for meaningful correlations. We have added a comment on lines 268-270 to highlight that fact up front (*While we discuss some of the regional differences below, we are most confident in the model simulations on the Arctic Basin-wide scale over which CICE has been tuned to agree with CS2 winter ice growth*.).

In response to the comments on the plots and color bars, we have made improvements that hopefully satisfy the reviewers concerns.

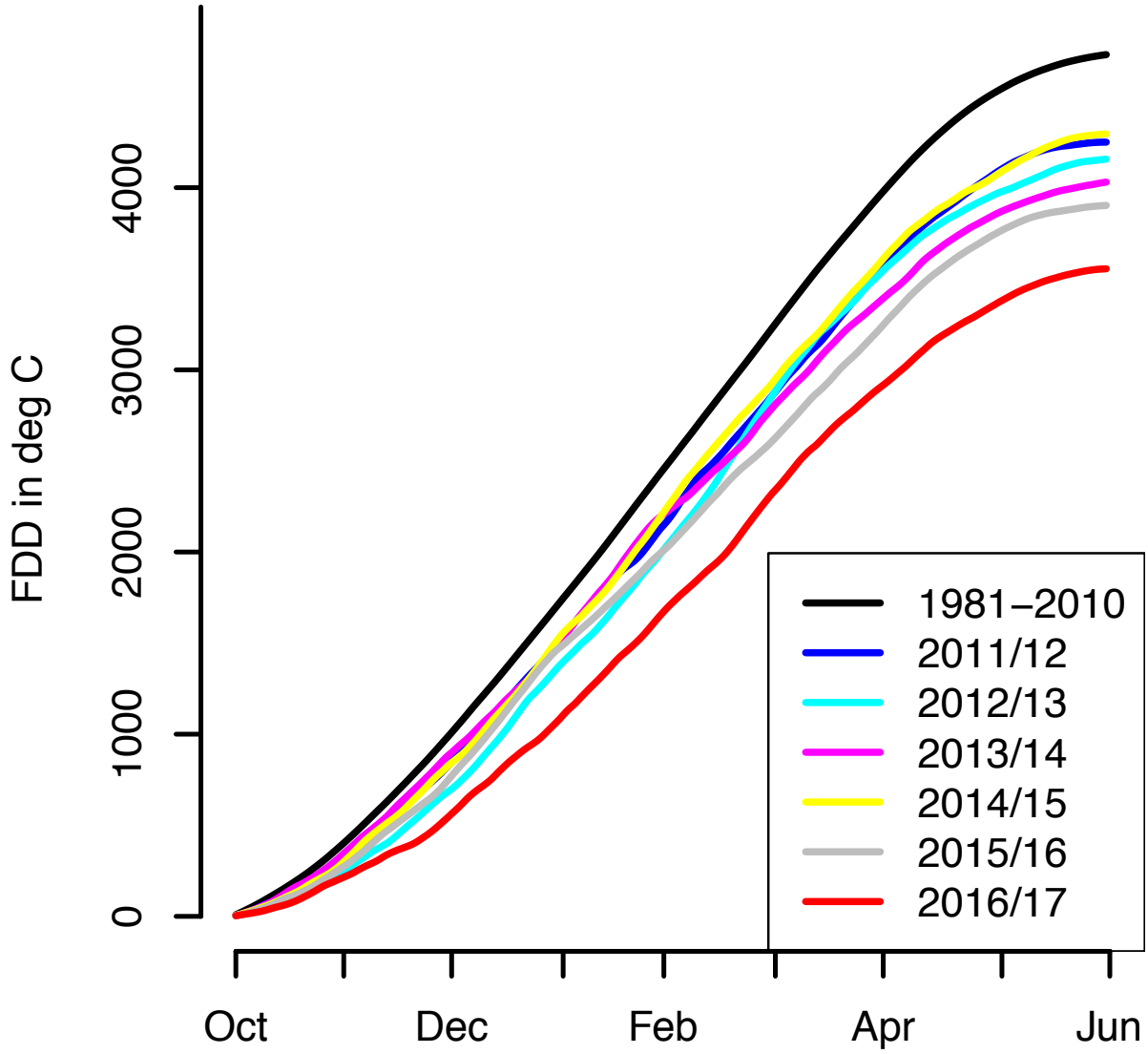
Detailed comments:
P3 L109: The CPOM product is derived using a 70 % threshold, not 50 % as stated in this paper (and in Laxon et al. (2013) because of a typo). There is an erratum for Laxon et al. (2013) where a 70% threshold is reported.
Thank you for pointing this out, it has now been corrected.

P3 L124: Category 1 ranges up to 0.6 m. But when you discard any measurements below 0.5 m, then you this category only covers a very narrow range of thickness. Isn't that a limitation for the initialization of the model then?
We discard grid point with a mean thickness below 0.5m, but otherwise we include all individual measurements. We state that "Grid points with less than 100 individual measurements and a mean SIT < 0.5 m are not included." But have now added the extra statement to avoid confusion: "Otherwise, all individual observations are included"

P3 L138: CICE simulations - What are the grid cell ice thicknesses in the CICE simulations representing? The mean thickness of the ice covered area or the mean thickness of the entire area including open water? This information should be given in this section, because it is crucial when comparing it with the satellite data.
This is a good point. We have now added at the end of this section the statement: "For comparison with CS2 we present the mean thickness of the ice covered area. In winter the sea ice concentration in the model is generally between 0.98 and 0.995% apart from locations close to the ice edge".

Figure 1 c): Information about the red and the yellow areas is missing.
Corrected.

Figure 2, L677: I cannot see any light gray areas. The legend in Fig 2c is very small.
We have increased the size of the legend. We removed the statement about the light gray areas as they are actually shown in white in Figure 2d. Here is the new Figure 2c.



Figures 3, 5, 6, 11: The labels of the color tables are too small. Since all maps of each figure correspond on the same thickness range, I suggest to use just one color bar and make it bigger.
We have removed the individual color bars and now just use one larger horizontal color bar.

Figure 4: It is a bit confusing that you use different thickness ranges for the CICE anomaly contributions from thermodynamics and dynamics (+/- 0.4), while for the other maps, you use +/- 0.8. I suggest to use a uniform range, e.g. +/- 0.8. This would make a comparison with the other maps easier.
We agree and made the suggested change.

Second, I wonder how to interpret the thermodynamic and dynamic contributions. For example, there is a positive CICE anomaly north of the archipelago (middle left), while both the thermodynamic (middle center) and dynamic (middle right) contributions show negative anomalies. How is this explained?

Well, in your example a very strong positive CICE anomaly in Nov 2016 (Fig. 3) has been reduced by thermodynamic and dynamic processes (positive anomalies) to result in a weaker, but still positive anomaly in April 2017. Thus, the initial conditions in November are responsible. Thermodynamic contribution consists of local ice growth/melt and dynamic contribution of advection and ridging processes during the period November to the following April.

Moreover, there is a typo in the caption (L692). I suppose contribution of dynamics is shown in the "right“ column.
Yes, thank you.