The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-69-RC2, 2018

The Cryosphere
Discussions
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Interactive comment on "Arctic sea ice-free season projected to extend into fall" *by* Marion Lebrun et al.

Anonymous Referee #2

Received and published: 30 June 2018

General comments

In the manuscript "Arctic sea ice-free season projected to extend into fall", the authors use both CMIP5 models and satellite observations to assess changes in Arctic sea ice seasonality. The authors find that changes in retreat and freeze-up contribute equally to the lengthening of the ice-free season in both satellite observations and a subset of CMIP5 models over the period 1980-2015. Additionally, an earlier ice retreat date yields a later freeze-up date in satellite observations, though it is less clear from the analysis if CMIP5 are consistent with these observations. By 2040, the chosen subset of CMIP5 models project that the change in freeze-up will be larger than the change in retreat. Furthermore, a proposed thermodynamic mechanism derived from a 1-D model shows that the change in freeze-up should be larger than the change in retreat:

the surface gains heat quickly after ice retreat, but is slow to lose heat until freeze-up due to “non-solar” fluxes. The proposed mechanism is thought to be a long-term process not seen on interannual timescales.

The authors do a good job of including a variety of data for comparison and analysis. It is interesting to see an analysis of CMIP5 models paired with a discussion of mechanisms using a 1-D model. The appendices provide valuable information on the proposed mechanism and might warrant inclusion in the main text. However, I would like to recommend major revisions to address the following concerns:

1. It must be clearly shown how linearly interpolating monthly mean SIC affects ice retreat and advance within the CMIP5 models. The authors should consider looking at models that have SIC available on both daily and monthly timescales, and then comparisons of daily and monthly-interpolated data should be displayed in a figure. This figure will hopefully show that SIC actually changes linearly over a month and that linear interpolation is an acceptable approach. Justifying the linear interpolation of monthly SIC is extremely important, as it forms the foundation of the analysis.

2. The current literature on the magnitudes of changes in ice retreat and advance should be clarified. The authors claim that, “Over the satellite period (1979-2013), the Arctic open water season duration has increased by >5 days per decade (Parkinson, 2014), generally due to earlier ice retreat and less so due to later-freeze-up (Stammerjohn et al., 2012).” (Page 3 Lines 40-42). No mechanism is offered to explain why this might be expected or surprising. Additionally, it seems there are multiple observational studies suggesting changes in freeze-up to be the main driver of the increasing number of open water days (Stroeve et al., 2014; Johnson and Eicken, 2016; Serreze et al., 2016) and one study suggesting that changes in ice retreat are the main driver (Stammerjohn et al., 2012). When considering modeled changes in open water days, the authors only reference studies that project freeze onset to be the main driver (Wang and Overland, 2015; Barnhart et al, 2016), yet claim these studies to be peculiarities. See also Wang et al., 2017. If there are additional studies suggesting that earlier ice

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retreat is currently (or projected to be) the main contributor to increasing open water days, those studies should be discussed.

3. The defined R-values appear to have utility, but they are currently very difficult to interpret. This should be addressed by providing examples of how a given R-value is calculated and used to draw conclusions. More information should be provided in-text about the length of the trend periods for each Rshort and Rlong (and how these timeframes were chosen), as well as the physical implications of a positive R-value versus an R-value greater than one. Explanation is needed as to why these metrics are more useful than other forms of statistical analysis.

4. There should be a clearer distinction between discussions of internal variability and inter-annual variability. There are multiple places in the article where the two seem to be construed. References to Barnhart et al, 2016 should clearly indicate which kind of variability is being discussed (since both are addressed in that particular study). The authors rely heavily on evaluating the ESMs and forced model against the satellite observations, and greater effort should be made to put all of these data sources into the context of internal variability.

5. "Ice advance" would be a more appropriate term than "freeze-up". The authors are using a metric based on ice concentration versus the initiation of ice growth. The term "ice advance" would also give consistency to the manuscript, since the term "ice retreat" is used.

Specific comments

Pg. 3 Line 42: More context is needed here. This conclusion from Stammerjohn et al., 2012 is referring to where ice cover is changing fastest, and a later study with a slightly different methodology found that freeze onset dates are the main driver of changes in the open water period (Stroeve et al., 2014).

Pg. 4 Line 58: Do the authors mean inter-annual variability or internal variability?

Pg. 4 Line 63: Are these truly peculiarities? If so, other model studies should be referenced to show that findings from the CESM LE and Alaskan Arctic are indeed unique.

Pg. 6 Line 115: I'm not confident that interpolating the ESMs from monthly to daily SICs and treating the satellite observations as a "perfect reference" makes sense.

Pg. 7 Lines 123-127: "Overall. . .compared with IPSL-CM5A-LR". Is this a result of your analysis, and if so why is it in the methods section? It's not clear what is meant to be compared here (no satellite observations in Figure S1).

Pg. 7 Lines 137 and 146: In this section the authors should explain over how many years the trends are taken for Rlong and Rshort. I later found in Table 1 that Rlong is over the period 2000-2200...authors should verify that linear trends are appropriate over this long of a time scale.

Pg. 9 Lines 175-177: "Individual models. . .than any ESM simulation." I don't see the satellite observations in Fig. S2, so it's difficult to verify this statement. Mean state issues are brought up multiple times without being fully explained (again in Pg. 11 Line 214).

Pg. 9 Line 179-180: How is it known that weak heat flux is responsible for the trends, and that this is related to low resolution? This should be removed or a reference should be provided.

Pg. 10 Lines 187-188: Why is there specific focus on the IPSL-CM5A-LR model in a CMIP5 paper? There should be clear explanation here as to why this model is singled out. It would be preferable to show at least one figure that includes all nine models in the main text. Otherwise, the manuscript may be interpreted more as an IPSL paper than a CMIP5 paper.

Pg. 10 Line 196 and Pg. 12 Lines 230-231: I'm not understanding the distinction between the R values being positive and being >1. An example of interpreting an R value would be useful.

Pg. 11 Lines 211-212: What does “Since models never position the ice-edge correctly” mean?

Pg. 12 Line 248: Perhaps more of the Appendix material should be explored in the main section here. Otherwise this relationship seems to come out of nowhere.

Pg. 13 Line 274: What does “generic behavior we see in CMIP5” mean?

Pg. 14 Lines 280-285: “. . .the freeze-up amplification mechanism is not dominant at interannual-timescales.” Does this mean that in a given year, earlier retreat may not actually yield a later freeze-up, even though the ice is trending this way over time? I don’t understand how 35-year trend analysis (Fig. 5) is being used to comment on synoptic to inter-annual timescales.

Pg. 15 Line 309: What is meant by “ice seasonality, turns up”?

Pg. 15 Lines 309-312: “The long-term response. . .in climate change studies.” It seems like the variability referred to here is internal variability, but it’s unclear what is meant by “essentially driven by dynamic processes”. This should be removed as it is not backed up by any analysis or reference.

Pg. 16 Lines 317-332: If this paragraph is included it should be in the introduction, not the conclusions.

Pg. 16 Lines 334-336: “Pinpointing the drivers of sea ice seasonality, in particular the upper ocean energy budget (Donohoe and Battisti, 2013) as well as understanding the impact of better resolved ocean currents are critical to reduce uncertainties.” Since this isn’t shown anywhere in the manuscript, it should be removed.

Pg. 35: Figure 1 is well done and interesting.

Pg. 36: The histograms in Figure 2 are too small and should be at least as large as in Figure 3.

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