Arctic sea ice-free season projected to extend into fall,
Discussion paper submitted to The Cryosphere by M. Lebrun et al.

Reply to Referee #2

Here is a systematic answer to the comments provided by Referee #2. For each item, we include an answer and propose practical means to revise our paper.

We gratefully thank the referee for the time dedicated to our manuscript and for his.her constructive spirit.

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.

Answer:

An analysis similar to that proposed by the reviewer had been done at the very beginning of our investigations and is included in the Discussion manuscript. Whereas the reviewer proposes to do that with CMIP5 models, we did that with satellite observations, but the principle is the same and do not see any reason why the outcome would be different if CMIP5 models output were used instead of satellite data.

The results of our investigations were already available in the discussion manuscript (Table S2) and were discussed page 6, lines 113-120.

Just as a reminder, we have compared three methods of comparison: we have calculated ice retreat and advance date and ice-free season length from three different sources:

1. the daily ice concentration directly from satellite observations ("daily").
2. the monthly sea ice concentration, averaged from daily concentrations ("monthly").
3. the monthly fields, re-interpolated daily ("interpolated").

We had identified systematic biases in "interpolated" ice retreat and freeze-up dates on the order of 5 days as compared with "daily" fields - yet these biases do not affect the trends, furthermore they are much smaller than if we used the raw monthly data and much smaller than the investigated signals.

For information, we attach a figure here below that we will consider incorporating into the paper, as a complement to the table.

Action: We will make sure that the aforementioned material is more visible, and potentially after discussion with the editor, see if we add the new figure to make our point absolutely clear.

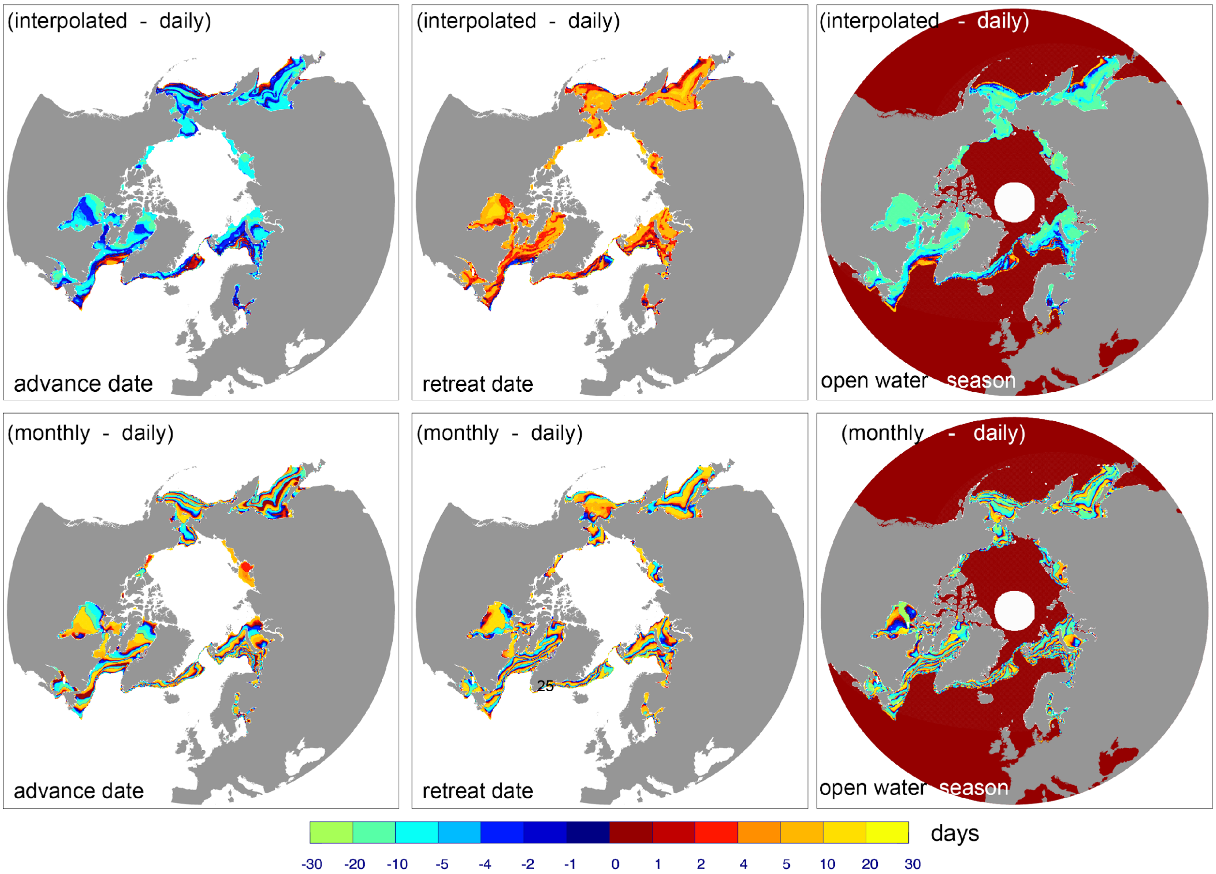

Figure : Differences between interpolated (top) /monthly (bottom) and daily ice advance date (left), ice retreat date (middle) and open water season (right).

2. The current literature on the magnitudes of changes in ice retreat and advance should be clarified.

Answer: The referee is right, but this is only partly our fault: the literature is itself complicated: there are different diagnostics (melt season, melt onset, freeze onset vs retreat, advance and length of open water season) different computation means and different periods.

Action: We will explain that in the introduction (different diagnostics, computation methods, periods, …).

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.

Answer: That "*no mechanism is offered to explain why this might be expected or surprising"* was intentional. Before this paper, we just have a few observational facts, but there has been no paper dedicated to mechanisms, so we cannot really have expectations (because there has not been much theoretical analysis). We thought our sentence "*Less Arctic sea ice also implies changes in ice seasonality*" was neutral enough.

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).

Answer: The reality is not as clear. First, the study of Stroeve et al 2014 focuses on the melt season which is not the open water season and so is not really relevant here.

The study of Johnson and Eicken 2016 has specific diagnostics for the start and the end of the freeze-up and break-up seasons. Besides, it is difficult from their Table 4 to conclude that the trends in freeze-up date exceed those in break-up dates for 1979-2013.

Both studies of Johnson and Eicken and of Serreze et al are specific to the Chukchi/Beaufort region.

So in the end, we are facing a situation where there is no clear global pattern (one global study, two regional studies), and it is true that our wording suggests that earlier retreat should dominate.

Action: We propose to word our text with a more neutral approach, without favouring either the ice retreat or advance date as the most active driver. In particular, we would not say that the open water season increases "generally due to earlier ice retreat and less so due to later-freeze-up", but rather insist that the current set of publications is contrasted.

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

Answer: We do not claim they are peculiarities, we just meant that these two studies could reflect peculiarities of the region or of the model that was used. Yet we agree that our wording could be improved, in light of the available studies.

Action: We will express our arguments the other way around when needed - that the general view seems to suggest that freeze-up date to be the dominant driver in the modelling literature (3 publications). For instance, *"the simulated future increases in the ice-free season duration seem dominated by the later freeze-up rather than by earlier retreat as in contemporary observations"* will be reworded into *"the simulated future increases in the ice-free season duration seem dominated by the later freeze-up".* We will also change "could be peculiarities" into "are features to be confirmed with a larger set of models and regions".

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.

Answer: There are already substantial explanations on the meaning of Rshort and Rlong in the discussion paper, lines 136-149. As an example of use, we get "*By definition, R>1 if the long-term trend in freeze-up date exceeds the long-term trend in retreat date in a particular pixel, otherwise R<1"*. The referee is right, however, that there are a few missing elements. Indeed, more could be told about the length of the computation periods, the physical implications of a positive R>0 vs R>1, and why the R metrics are more useful than other analyses.

Action: In section 2.4, we will explain why the different periods have been used (36 is the length of the available observation period and is close to the standard 30 years used in climate science, whereas 200 years is the total amount of years we can use to qualify changes).

The advantage of R is to synthesize several pieces of information about retreat and freeze up in a single number will be underlined in Section 2.4. We will incorporate that at L. 138.

Then we will also explain how: "R>0 means that earlier retreat implies later freeze-up" and that "R>1 means amplified freeze-up", which will also be reminded in Section 3.2. We will also say that missing values mean either that the trends are not significant or that the point is out of the seasonal ice zone.

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.

Answer: Internal variability is not covered in the discussion paper. Actually, observations only display one climate realization (by definition), the ocean-sea ice model NEMO-LIM also offers one realization (forced with one atmospheric forcing) and the 9 CMIP5 models that we analyze only propose one ensemble member up to 2300. Thus, the notions of internal variability and interannual variability cannot be construed in the text, since the former is not even treated.

What the reviewer seems to suggest is that we should get a better sense of how our diagnostics are sensitive to internal variability. We agree that checking that our diagnostics are robust with respect to internal variability would strengthen our study.

Action: We will reproduce the R_short and R_long diagnostics in five realizations of the same climate model (IPSL-CM5A-LR), and provide that at least in supplementary material. We wanted to provide this information here, but had no time.

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.

Answer: Agreed

Action: We will change that throughout the manuscript.

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).

Answer and action: See answer to general comments.

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

Answer: We meant internal

Action: We will specify.

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.

Answer and action: See answer to general comment.

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.

Answer and action: See answer to general comment.

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).

Answer: It is indeed a result of our analysis. However, that the evaluation of our modelling tools is methodological is a point of view that is often adopted. For this reason, we would probably not change that unless specifically requested.

We will explicitly link Figure 2 and Figure S1 in the caption.

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.

Answer and action: We will clearly specify the periods over which diagnostics are computed, in particular in the case of Table 1. Linear trends seem generally appropriate (see Fig. S6) - maybe not for CSIRO - and we select significant trends in the computation of our diagnostics.

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.

Answer: We understand your point. As Fig. S1 corresponds to Fig. 2, Fig. S2 is a repetition of a Fig. 3 and it is easy to put them side by side as they are exactly symmetrical.

Action: We will explicitly make the connection between the main text and SM figures.

Mean state issues are brought up multiple times without being fully explained (again in Pg. 11 Line 214).

Answer: Thanks for noticing. We will make clear in the methods section how the forced run and the best among the coupled runs can be used or not to argue for mean state issues.

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.

Answer and action: It is true that this statement is a bit weak and should be reinforced. We would cite Serreze et al (2016) for the heat flux issue. Regarding the role of resolution, it was a speculation that proves unsupported by the literature (in particular Clement Kinney et al., 2014), so we would remove that part.

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.

Answer: The rationale that was adopted is the following. Models seem consistent in their long-term response. Because our diagnostics do not support multi-model average, one of them had to be used for illustration of the others. Of course, we chose IPSL-CM for our analysis, because we locally have more output (e.g., daily) for that model than available in the CMIP5 database.

We agree, though, that since all models show similar patterns and evolution for Rshort and Rlong, it would probably reinforce the paper to show multi-model maps.

Action: Smaller versions of Fig. S3 and S4 could be moved to the main text as replacement to Fig. 4 and Fig. 5.

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 Rvalue would be useful.

Answer: As explained earlier, we will introduce explanations in the methods section and remind those in the paragraphs of the result sections.

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

Answer: We guess this might be a grammatical issue. This sentence refers to the description of figure S1 to compare with observation in figure 2 (Pg. 6 and 7 line 122, 123). We meant "none of the model positions the ice edge correctly everywhere"

Action: Reword the sentence.

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.

Answer and action: Because of Referee #1, this section will have to be revamped to include also winter processes. We will try to account for your comment and do the best to weigh the qualitative explanations and the technical aspects, which was our main concern when writing it.

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

Answer: Here we refer to the fact that in CMIP5 models R<1 over the observational period.

Action: We will reword the sentence to make that absolutely clear.

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.

Answer: The 35-years trend is done for the long-term coefficient. When we talk about interannual-timescales we refer to the short-term coefficient, Rshort, which is defined as minus the linear regression coefficient between de-trended advance and ice retreat dates. The sentence "the freeze-up amplification mechanism is not dominant at interannual-timescales" means that, even if an earlier retreat implies a later advance, the offset in the advance date is not larger than the offset in retreat date at interannual time scales.

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

Answer: Maybe the comma is misplaced. Our goal was to express that the long-term response of ice seasonality to warming becomes visible by mid-century in CMIP5 models; i.e. Rlong becomes >1 by mid-century.

Action: Remove comma.

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.

Answer: Reviewer 1 had a very similar comment, which suggests that something might be true in here.

Action

We would reword the incriminated statement:

Variability seems essentially driven by dynamical processes, a setup that has other analogs in climate change studies

as follows:

We have not found means to explain changes at inter-annual time scales based on thermodynamic processes. This points to dynamical processes as most likely drivers, a setup that would have other analogs in climate change studies (Bony et al., 2004; Kröner et al., 2017; Shepherd, 2014), but would need further analysis for confirmation.

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

Answer: We admit some issues in this paragraph (the order of the elements is far from perfect). Yet this paragraph is supposed to incorporate the implications of our findings, and as such is well placed.

Action: Make sure elements are in the good order and that only implications of our research findings are kept in there.

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.

Answer and action: For the first part, the upper ocean energy budget is indeed the most important component of the mechanism that is discussed, so we would keep that. We would remove the second part (about resolution) only.

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

Answer: Thank you

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

Answer: We agree with the referee, it’s a mistake.

Action: We will change the histograms size in Figure 2.