Arctic sea ice-free season projected to extend into fall,

Discussion paper submitted to The Cryosphere by M. Lebrun et al.

Reply to Referee #1

Here below is a systematic answer to the comments provided by Referee #1. 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.

The manuscript addresses an important topic of how and why sea ice seasonality is projected to change. The study focuses on changes in the timing of ice retreat and advance and shows that trends in ice advance timing exceed those in retreat timing in 21st century climate model projections (in contrast to observations).

I believe that the unique aspect of this study is that they propose a mechanism for this difference in ice retreat and advance trends. This mechanism is that the solar heating occurring over the summer is slow to be released during the fall freeze-up. I agree that this can explain why the fall freeze-up is delayed. However, it is not clear to me that this has relevance for the magnitude of the trends in the timing of ice retreat (or the relative trends in retreat and advance). I expect the ice retreat timing trends may instead be related to ice thickness present at the beginning of the melt season. However, the study provides little analysis on what actually drives the trends in ice retreat timing and how those might be changing in the 21st century. Because of this, I do think that the study convincingly explains the time-evolving differences in ice advance timing and retreat timing trends. More work is needed to either (1) better explain the relevance of their proposed mechanism to the relative difference in the trends or (2) better understand the controls on ice retreat timing, how those are changing in the 21st century and what that means for 21st century trends. Without this, the mechanism proposed to explain the differences in freeze-up and retreat date trends is not very convincing and seems incomplete. I am recommending major revisions to the paper to allow the authors the opportunity to address these concerns.

Answer:

Thanks for identifying the date of retreat as a missing part of the argument. Indeed, it seems obvious to explain why the ice retreat date moves slower than the ice advance date, which was implicit in our reasoning, but not really explicit in the paper. A revised version of the mechanisms at play of changing Arctic sea ice seasonality would read as follows:

- The ultimate driver of the change in ice seasonality is the applied radiative forcing. A 0.1 W/m2 increase has a direct impact of about 0.5 d/yr of both earlier retreat and later advance.

- Because of non-linearities in the system, there are also two positive feedback components, associated with links between the ice advance and ice retreat dates. Annual changes in both ice retreat and advance dates can therefore be expressed as the sum of a forced response and a feedback: $\Delta d_a = \Delta d_{a,forced} - R_{summer}\Delta d_r$; $\Delta d_r = \Delta d_{r,forced} - R_{winter}\Delta d_a$.

- The feedback of changing ice retreat date onto ice advance date is as already described in the text: $\Delta d_{a,feedback} = - R_{summer}\, \Delta d_r$, with $R_{summer} \sim 2$. Hence the feedback component is the largest driver of changes in advance date (~2 d/yr).

- The 1D model results suggest that the feedback of ice advance date onto ice retreat date is comparably much weaker, with $R_{winter} \sim 0.25$. Hence the feedback component for ice retreat date is as large as the forced component (0.5 d/yr).

- The reason why R_{winter} is small is twofold. First, the growing season is about twice as long as the melting season. Second, the 1/h dependence of the ice growth rate implies that maximum winter ice thickness does not decrease as much as if the growth rate was constant. Both contributors imply that changes in ice advance date are divided by about two when translated into changes in retreat date: the first by homothety, the second because of the 1/h non-linearity. In this reasoning, changes in maximum winter ice thickness are pivotal for linking ice advance and retreat date. Both the asymmetry of the growth/melt seasons and the growth-thickness feedbacks are likely active in the CMIP5 models.

- All in all, the relative changes in ice retreat and freeze-up dates are largely dominated by the summer feedback described in the discussion paper. Yet the magnitude of the two aforementioned winter processes are important for the summer processes to emerge.

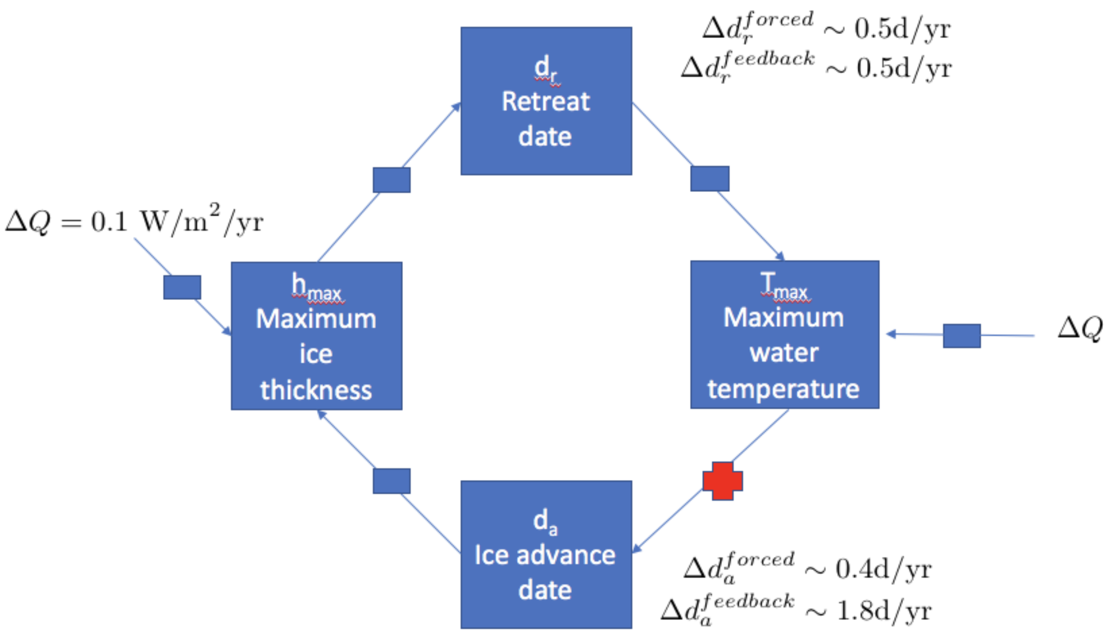

Figure: Schematic representation of the links between retreat date and advance date

Action:

- We do not plan to add more analyses to the paper, which could drastically inflate it.

- We will use basic physics to justify why the aforementioned winter feedbacks are much weaker than summer ones. We will do that in Section 3.4. We would also add information on winter processes in the 1D model in Appendix A.

- We will also acknowledge in the last section that there probably is room for a more systematic study with a dedicated experimental setup to investigate the balance between winter and summer effects.

- We will specifically focus the paper on the summer effects (abstract, introduction, …).

Specific comments.

P5, line 92-93. "Such a simulation, not only performs generally better than a free-atmosphere . . ." This may be generally true. However, I don't believe that this is shown anywhere in the paper for this specific run. Does the NEMO-LIM run really have better ice extent than the ESMs? (It does not appear to be the case from Figure S1 where the "forced run" seems to show extensive ice in the Labrador Sea as compared to many of the ESMs.) If the authors choose to use this argument regarding their NEMO-LIM run, then they need to actually quantify the NEMO-LIM performance relative to the ESMs. For example, what is the annual cycle of ice extent compared to the ESMs?

Answer: It is true that we are not explicit enough to make that point.

Action: We would add the following figure as supplementary material, to show that our forced run is less biased than our CMIP5 subsample.

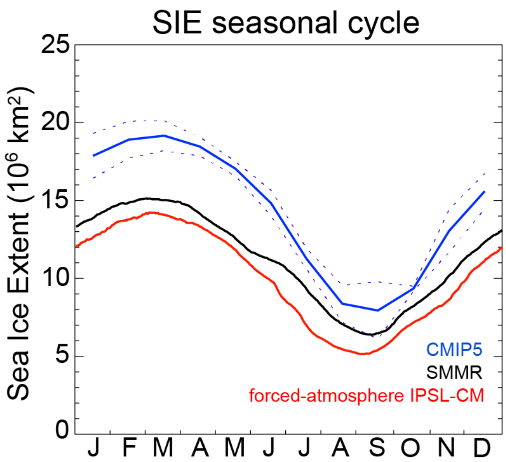

Figure: CMIP5 (blue; median ± IQR of the 9 models), Satellite observations (black) and forced-atmosphere IPSL-CM simulation (red) sea ice extent seasonal cycle between 1980-2015.

P6, line 122. "Larger errors in the individual models" Quite a few of the individual models look better than the ensemble mean. Please revise to "Larger errors in some individual models"

Answer: The referee is right.

Action: Change the sentence as proposed.

P7. Line 125-127 "Such an interpretation is supported by the good consistency..." I believe that the NEMO-LIM run that is referred to is labeled as the "forced" run in Fig S1. If so, then the seasonality diagnostics in this forced run look considerably worse than many of the ESMs. They do look modestly better than the IPSL run but not in all regions. I'd suggest that you better quantify what you mean by "good consistency" with observations.

Answer: The analysis of the referee is correct.

Action: We would just not only use the forced run, but also the runs with known better mean state (CESM, CNRM, MPI) and less good mean state (CSIRO, BCC, IPSL) to justify that. We will mention the comparison is made visually.

P9, line 175. "Individual models show larger errors, to be related with mean state issues . . ." The NEMO-LIM model differs from the ESMs in that it is driven by observed atmospheric conditions. As noted by the authors, this influences the mean state of the model. However, it also influences the variability (internal variability is now timed to the real world) and feedbacks with the atmosphere. Because of this, it is not necessarily the case that the NEMO-LIM comparisons indicate that the mean state errors are responsible for the differences in trends with observations. It could instead be a consequence of internally generated multi-decadal variability (for example in AMOC which is known to affect sea ice trends). The authors should be more careful at making simple statements here and elsewhere in the paper (line 214), that the better agreement of NEMO-LIM and observations somehow implies something about the role of mean state biases. More analysis would be needed to actually show this.

Answer: We agree with the analysis of the referee, that the forced run does not provide a formal proof that the mean state ultimately controls the better dynamics of the model.

Action: We will therefore temper all the incriminated statements, and explain that the forced run only provides part of the formal proof that would be enhanced, for instance, by using another forced run with different parameters that would deteriorate the mean state.

P11. Line 211. "the simulated Rlong is slightly higher . . ." Is this for the ensemble mean or the IPSL model? Please clarify.

Answer: We indeed need to be more precise.

Action: We will specify that the simulated Rlong holds for the ensemble mean.

P12. Section 3.4. As mentioned in my general comments, I do not find the argument provided here on the reasons for an amplified delay in freeze-up date very compelling. The argument focuses solely on what drives a delay in the fall freeze-up. However, it does not consider what drives the earlier retreat. It seems to suggest that the earlier retreat is driven by Q+ but this doesn't make sense to me. Instead, I'd expect that Q+ varies in response to the changing ice retreat. The authors need to more explicitly state what drives the earlier ice retreat and how those factors change (or do not) in the 21st century. Otherwise, it seems like the mechanism proposed is only a part of the story and does not necessarily explain the differences in ice *retreat and advance timing trends in the projected climate. These considerations could also be important when analyzing the interannual variability.*

Answer: All aspects mentioned here are covered in our answer to the general comment.

P15, line 311. "Variability seems essentially driven by dynamical processes" I don't believe that this study has shown this in any way. Either provide evidence for this or remove/reconsider this statement.

Answer: It is true that we have not proven this directly. Our results rather suggest that by the absurd.

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 the behaviour 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.

Supplementary material. Figure S1. Results are shown for a "forced-atmosphere IPSL- CM simulation". Is this the same as the NEMO-LIM simulation referred to in the text? If so, please be consistent with the terminology.

Answer: The referee is correct, thanks for noticing.

Action: We will use the "forced-atmosphere IPSL-CM simulation" as a standard name.