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

Anonymous Referee #1

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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

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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.

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?

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"

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.

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 by 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.

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

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.

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.

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.

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