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The Cryosphere
Discussions

Interactive comment on "Future interannual variability of Arctic sea ice in coupled climate models" *by* John R. Mioduszewski et al.

Anonymous Referee #1

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general comments:

This is an interesting study addressing the evolution of the interannual variability of Arctic sea ice area and its causes. The study is based on analyzing historical and RCP8.5 simulations of CESM-LE and, in part, 12 CMIP5 models. The study primarily finds an inverse relationship between the rate of sea ice retreat and the magnitude of variability. The authors further show that a sufficiently thin ice cover fosters the variability and that thermodynamical processes dominate over dynamical processes in causing this variability. Although I find the study relevant, well written and structured, I have some major concerns about the novelty of the findings, some methodological aspects and the robustness of their conclusions. I recommend publication in TC only if these major concerns will be addressed.

specific comments:

Title: The paper does not only address the future interannual variability but also the past.

#1 ll. 47-48 The "important physical and societal consequences" could be given more specific.

#2 ll. 60-62 It is not clear from the abstract what "thermodynamic processes" exactly mean. I would like to have this more specific (e.g., open water formation efficiency), especially because you specifically name the dynamic processes, which you find to be less important, but not the important thermodynamic ones.

#3 l. 106 Where does the judging statement "likely" comes from? Is this justified in the given references according to IPCC language. As you mention, the likelihood of summer ice-free conditions strongly depends on the emission scenario. For RCP8.5, it might be rather certain, for RCP2.6 it is not. "Likely" is vague here.

#4 l. 117 To me, there is no logical link between the reduction in sea-ice extent and the loss of multi-year ice. The reduction in sea-ice extent is not obviously the cause for the loss of multi-year ice. Please rephrase.

#5 ll. 122-123 How does decreased ice thickness amplifies the ice-albedo feedback? Please explain.

#6 ll. 141-142 I appreciate that you specifically mention the novel aspects of your study. However, I find these aspects only partly novel. The first aspect is not truly novel. Olonscheck and Notz, 2017 (Consistently Estimating Internal Climate Variability from Climate Model Simulations. Journal of Climate) distinguish changes in the variability of winter and summer Arctic sea ice area. The second aspect is also touched by Olonscheck and Notz, 2017 but your study goes beyond this by investigating the underlying processes for the model-simulated changes in CESM-LE. However, the very recent study by Massonnet et al., 2018 (Arctic sea-ice change tied to its mean state through thermodynamic processes. Nature Climate Change) covers parts of your findings. I recommend to more clearly work out the novel aspect of your study, to distinguish your results from the mentioned studies, and to discuss your results in the context of their findings.

#7 ll. 147-148 I don't believe that the internal variability is robustly characterized from just one model. The internal variability largely differs between the CMIP5 models. How do we know that CESM-LE is representative? I assume you mean that 40 ensemble members allow to robustly quantify the internal variability WITHIN THAT MODEL, but I don't believe that your statement is correct as it is now. Please be more precise here.

#8 ll. 162 A medium ensemble of 15 members for RCP4.5 described in Sanderson et al., 2015 (A new ensemble of GCM simulations to assess avoided impacts in a climate mitigation scenario. Climatic Change) and recent ensembles, e.g., for RCP2.6 described in Sanderson et al., 2017 (Community climate simulations to assess avoided impacts in 1.5 and 2°C futures. Earth System Dynamics) also exist.

#9 ll. 170-172 For two reasons, I am not convinced by the usefulness of this selection criteria. First, because the threshold of 20-percent error seems arbitrary to me. How is this justified? Second and more importantly, there is no reason to believe that models that fit the observations comparatively well are better than others because of the large influence of internal variability. When taking model-specific internal variability into account the sea-ice simulations of most CMIP5 models are plausible. I would like to see whether or not your basic conclusions change when using the full set of CMIP5 models. Also, as a reader I would like to know which CMIP5 models you used without having to look this up in Wang and Overland, 2015.

#10 ll. 176-177 To calculate the statistics for each of the 33 ensemble members and to then average them gives a biased estimate, because models with more ensemble members have a larger weight than models with only few (or even one) members. Again, I would like to see whether your basic conclusions would change when you

always use e.g. three ensemble members from a model. As it is, I don't find the approach convincing.

#11 ll. 212-217 I find the analysis of the CMIP5 models rather weak. To me, it is no proof that the variability is indeed increasing as shown by e.g., Goosse et al. 2009. This is because I see no logic behind simply averaging the CMIP5 models. As you write, the timing of ice retreat is very different in the different models, so averaging them will smooth out possible signals. For instance, one could normalize the timing of sea ice retreat before doing the analysis. I think that more analysis of the robustness of the results based on the CMIP5 models is needed.

#12 ll. 230-232 Related to the previous comment, I would like to know which of the two reasons is more relevant.

#13 ll. 219-220 See again Olonscheck and Notz, 2017.

#14 ll. 304-322 It is not very clear to me how exactly you calculate the thermodynamic and dynamic component. For instance, do you sum up top, basal and lateral melt for the thermodynamic melt component? I think I can guess what you did, but it is not written down precisely.

#15 l. 373 I recommend one or two introductory sentences here to guide the reader. This would also help to improve the structure of the discussion section.

#16 l. 443 This should be "projected", instead of "predicted".

#17 ll. 448-449 I very much appreciate that your work includes the analysis of CMIP5 models. But I question that the presented analysis is sufficiently well done to justify this statement on robustness. Especially, because the CMIP5 models are only used for section 3.1 and not in the later sections that deal with the mechanisms. The questionable (see comment #11) and generally weak inverse relationship between variability and rate of retreat that you show for the CMIP5 models does NOT necessarily imply (and also does not suggest) that the same mechanisms are at work like the ones you describe for CESM-LE. This statement is too strong. I recommend to either extend your analysis of the mechanisms to the CMIP5 models (if possible) or further weaken or delete this statement.

technical corrections:

l. 331 I prefer "the variability in the thermodynamic term", rather than "the thermodynamic term variability"

Figures: I suggest to make the figures look more consistently, i.e. Figures 3 and 6 like Figures 1, 2 and 4. Also, I find the different axis labeling in Figure 6a and 6c confusing. For Figure 6, a title for each panel would increase the readability and lines at 0 percent and 100 percent in panels a and c, too.

l. 345 frazil = frazil ice?

References: Comiso et al ... The year of publication is missing. Zhao et al., 2018 ... This reference appears twice.

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