The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-122-RC1, 2017

The Cryosphere Discussions
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Interactive comment on "Mechanisms influencing seasonal-to-interannual prediction skill of sea ice extent in the Arctic Ocean in MIROC" *by* Jun Ono et al.

Anonymous Referee #1

Received and published: 18 August 2017

This work investigates the seasonal-to-interannual prediction skill of Arctic sea-ice extent (SIE) using a set of hindcast experiments performed with the MIROC GCM. The authors investigate prediction skill for detrended Arctic SIE, identifying skillful predictions up to one year in advance. They also examine the key physical mechanisms impacting prediction skill, concluding that North Atlantic ocean heat content anomalies are a source of skill for December SIE predictions and that sea ice volume is a source of skill of September SIE predictions.

I commend the authors for their focus on physical mechanisms and their relation to the reported SIE prediction skill. However, I have a number of serious concerns with the

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Discussion paper

manuscript in its present form. In particular, my major concerns are: (1) the authors' choice of Arctic domain, and how this choice biases and confuses results throughout the manuscript; (2) the definition of ocean heat content and its impact on the proposed advective ocean heat content mechanism; and (3) the apparent disagreement of SIE lagged correlation values with previously published literature. Specific comments detailing these concerns are provided below.

Note: I will use the convention p.l throughout this review to refer to page number p and line number l of the discussion paper.

Major Comments:

Before beginning the major comments, I would like to clarify a convention. The authors use a different lead-naming convention than the hindcast studies cited on 2.7. For example, a July 1 forecast of September SIE is referred to as a "lead-2" forecast in the literature cited on 2.7. In the manuscript, the authors refer to this forecast as a "lead-3" forecast. The authors should change their naming convention to be consistent with previous hindcast studies. I will use the commonly used convention in this review.

Major Comment 1) Choice of Arctic domain

The author's define their Arctic Ocean domain as all gridpoints north of 65N. They also exclude Baffin Bay and Hudson Bay from their Arctic Ocean domain without providing any justification for this decision. The Arctic Ocean domain choice directly affects the interpretation of essentially all reported results in the paper. I suspect that Figures 1, 2, 3, and 4 would all be notably different if the authors analyzed the commonly used pan-Arctic domain (i.e. all northern hemisphere gridpoints). Unless the authors have a compelling reason to focus on the domain north of 65N (and also to exclude Baffin/Hudson Bay), I suggest using a Northern Hemisphere domain throughout the paper. This would greatly reduce confusion and make the results more plainly interpretable. This would also make these results directly comparable to the seasonal prediction skill estimates that the authors cite on 2.7, which would make this work much more relevant to a broader community.

The authors' definition of Arctic domain and corresponding SIE (SIE_AO in the manuscript) is confusing because it systematically excludes many regions of high winter SIE variability, including the Labrador Sea, Bering Sea, Sea of Okhotsk, and Hudson Bay. This means that SIE_AO behaves like pan-Arctic SIE during the summer months, and behaves like GIN and Barents SIE in the winter months. In the melt/growth seasons, SIE_AO is a complex mix between these two. For each month, the reader is forced to perform a mental masking of the Arctic and think about what regions are actually contributing to SIE_AO variability in that given month. This significantly clouds the results of the paper. My specific comments related to this confusion are:

3.27-32: Figure 1a shows significantly higher melt season to growth season reemergence that Fig S1a. This is because Barents/GIN SIE anomalies are more persistent than anomalies in other Arctic regions, and these anomalies dominate the winter SIE_AO signal. I suggest checking the ratio of March SIE_AO standard deviation to pan-Arctic SIE standard deviation. This will indicate the amount of variance being lost due to the chosen AO mask (more on this in Major Comment 3, below)

4.4: The RMSE values in Fig. 2b are artificially low because SIE_AO doesn't have much winter SIE variability.

4.4-9: Why are the ACC values in Fig 2a and Fig S2a so different? In Fig. S2a there are a number of cases in which the short lead forecasts are less skillful than the long lead forecasts. For example, for the Jan 1 initialization, the lead 0-2 skill is substantially lower than the lead 9-11 skill. This is strange behavior and should be reported/commented on. Fig S2a is highly relevant as a direct comparison with other hindcast studies. Therefore, I believe that this figure should be a centerpiece of this paper.

4.13-16: The difference between Fig 2d and Fig S2d directly shows the effect of the domain choice. I expect this effect to be even larger for Jan, Feb, Mar, Apr sea ice. On the other hand, the September SIE curves in Fig 2c and Fig S2c are identical.

4.27-29: The summer to winter differences in SIV-SIE correlations are much less pronounced when using a northern hemisphere domain for SIE (Fig 3a vs Fig S3a). This should be commented on in the text. Also, in Fig. S3 is SIV/OHC computed north of 65N or using a northern hemisphere domain?

5.5-6: This is not very surprising, given that other most other regions have been excluded!

5.6-7: This may be true, but the domain choice biases results towards finding a signal in the Barents/GIN seas.

Major Comment 2) Definition of OHC and advection mechanism

The authors define ocean heat content by integrating vertically from the base of the mixed layer to 200m depth. What is the rationale for excluding the mixed-layer heat content from this integral? I believe it is crucial to include the heat content from the mixed layer, as this is the heat that has direct access to the sea ice and therefore has greatest potential to influence sea ice variability. Moreover, by excluding the mixed-layer heat content, the OHC field becomes undefined when mixed layers become deeper than 200m in the winter months. This creates a very notable “hole” in the winter OHC fields in the Barents and GIN Seas. The authors claim that shifting correlation patterns in Fig 4c-f are evidence of advective processes. However, the main feature that I see is a shifting domain over which the OHC field is defined.

I strongly suggest the authors recompute OHC by integrating from the surface to 200m, and produce new versions of Fig 3 and 4 using this OHC field. This will allow the maps in Fig 4c-f to be defined at all gridpoints, and allow for a better assessment of the proposed adjective mechanism. Also, I am interested to see if the winter OHC correlations in Fig 3d-f become stronger with this new definition.

Also, is the December SIE_AO time series used in Fig. 4 computed using the modelpredicted SIE or observed SIE? In other words, is this proposed mechanism based on correlations with observations, or is it a "perfect model" mechanism?

Major Comment 3) Lagged correlation analysis

The lagged correlation results shown in Fig. 1a are significantly higher than those reported in Day et al. (2014). On first reading, this seems like a striking discrepancy. However, I believe this difference can primarily be attributed to the authors SIE_AO domain choice. It needs to be made very clear that Fig. 1a should not be compared directly with the Day et al (2014) results. Also, SIE_AO lagged correlations with NSIDC data should be added to Fig S1. Note that changing from the AO domain to the NH domain would alleviate this concern.

Minor Comments:

1.29: I suggest changing "predictions" to "projections", to make this distinct from the seasonal predictions that are the primary focus of this paper.

2.6: Is this based on detrended SIE or full SIE anomalies?

3.1: Should specify that this is ocean temperature.

3.2: What ocean data goes into the objective analysis of Ishii et al. (2006)? What SIC data is used?

3.19-20: This is unclear and needs to be explained more precisely.

3.28: How close is the SIC from Ishii et al. (2006) to SIC observations? Are there any known biases/differences?

Fig 2: Legends should be added to panels c and d

Fig 2 caption: Is July 1 referring to panel c and Jan 1 referring to panel d? This is currently unclear.

4.18-20: I disagree with the second half of this sentence. The July 1 forecasts appear to have significant skill for Oct, Dec, Feb, and Mar.

4.19: What is “the longest lead time” referring to here? Do you mean “long lead times”?

Figure 4: Text labels should be added to the various panels to make this figure more readable.

5.24-26: I suggest adding Fig. S7 to the manuscript. Also, in this figure is the September SIE_AO the observed time series, or the time series from the hindcast experiments? This needs to be clarified.

6.7-9: These two sentences contradict one another. Please clarify.

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