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Mechanisms influencing seasonal-to-interannual prediction skill of sea ice extent in the Arctic Ocean in MIROC

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Response to Anonymous Referee #1

We deeply appreciate the referee's kind remarks about our paper. Detailed comments from referee are numbered consecutively and cited in italics, followed by our reply in bold face.

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 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.

Thank you very much for your concerns on our study. (1) Since we focus on the physical processes in the Arctic Ocean, we did not change the domain (please read our responses to referee's comments 1 and 2). (2) We recalculated ocean heat content and newly reconstructed Figures 3 and 4, according to your suggestions, and also partly rewrote the text. (3) Since our statements in the previous manuscript were not correct, we rewrote the text.

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.

Thank you very much for letting us know about a lead-naming convention. In accordance with your advice, we modified the lead-naming and the corresponding text. For example, we replaced "1 year" with "11 months" in the revised manuscript (1.12).

Major Comment 1) Choice of Arctic domain

1. 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.

So far, many previous studies on the predictability of Arctic sea ice extent with climate model have focused on the Pan-Arctic (or the Northern Hemisphere) domain. Furthermore, recent studies ([1]Sigmond et al., 2016; [2]Bushuk et al., 2017) have evaluated the regional predictability in the Pan-Arctic domain. On the other hand, we focus on

physical processes in the Arctic Ocean interior contributing to the seasonal-to-interannual predictability of the Arctic sea ice extent. In the present study, therefore, we would like to use the domain north of 65°N where sea ice has experienced rapid changes especially in the Pacific Sector of the Arctic Ocean (e.g., [3]Comiso, 2012). In that case, the Baffin Bay and Hudson Bay are partly included in the domain, but the directions of main surface currents are heading from the Arctic Ocean interior (shelves and basins) to the Baffin Bay through the straits of the Canadian Archipelago (e.g., [4]Aksenov et al., 2011). Thus, direct impacts of the Baffin Bay and Hudson Bay on physical processes through the Arctic Ocean interior are considered to be small. To clearly extract the impacts of physical processes through the Arctic Ocean interior on the Arctic sea ice, we did not consider the Hudson Bay and Baffin Bay.

1. Sigmond, M., Reader, M. C., Flato, G. M., Merryfield, W. J., and Tivy, A.: Skillful seasonal forecast of Arctic sea ice retreat and advance dates in a dynamical forecast system, Geophys. Res. Lett., 43, 12457-12465, doi:10.1002/2016GL071396, 2016.

2. Bushuk, M., Msadek, R., Winton, M., Vecchi, G. A., Gudgel, R., Rosati, A., and Yang, X.: Skillful regional prediction of Arctic sea ice on seasonal timescales, Geophys. Res. Lett., 44, doi:10.1002/2017GL073155, 2017.

3. Comiso, J. C.: Large decadal decline of the Arctic multiyear ice cover, J. Clim., 25, 1176-1193, 2012.

4. Aksenov, Y. Ivanov, V. V., A. J. G. Nurser, S. Bacon, I. V. Polyakov, A. C. Coward, A. C. N. Garaboto, and Moeller, A. B.: The Arctic circumpolar boundary current, J. Geophys. Res., 116, C09017, doi:10.1029/2010JC006637, 2011.

In the revised manuscript, we removed "Note that Hudson Bay and Baffin Bay are excluded" (3.23-24 in the previous manuscript) from the text, and newly added "In that case, the Baffin Bay and Hudson Bay are partly included in the domain, but the directions of main currents are heading from the Arctic Ocean interior (shelves and basins) to the Baffin Bay through the straits of the Canadian Archipelago (e.g., Aksenov et al., 2011). Thus, direct impacts of the Baffin Bay and Hudson Bay on the Arctic Ocean interior are considered to be small." to the text (3.30-33). In addition, we removed Figures S3 and S4

in the previous supplement to focus on the physical processes in the domain north 65°N, although Figures S1 and S2 are remained to compare the previous studies.

2. 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:

For the reasons mentioned in our response to referee's comment 1, the area north of 65°N excluding Baffin Bay and Hudson Bay is defined as the Arctic domain in this study. As you pointed out, since the Labrador Sea, Bering Sea, Sea of Okhotsk, and Hudson Bay are excluded, the signal of winter SIE_{AO} might be limited to the Barents Sea and GIN Sea. However, one of the main results of this study is the December SIE_{AO}. In that case, positive regression and correlation spatial patterns are seen in the Barents Sea even in the results for the Northern Hemisphere domain (please see Figure S4 in the previous supplement). Thus, the definition of the Arctic domain does not seem to affect the main results of this study, at least, for the December SIE_{AO}.

3. 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)

According to your advice, we checked the ratio of SIE_{AO} standard deviation to pan-Arctic SIE standard deviation for March. As a result, the value was 0.64. As you suggested, the remaining 36% is lost due to the domain selection, which might be explained by variability in the Labrador Sea, Bering Sea, Sea of Okhotsk, and Hudson Bay, and affect the

difference in the winter reemergence between Figure 1a and Figure S1. In the revised manuscript, we removed "In addition, the correlation coefficients are higher than those shown in Day et al. (2014b), for example, at a lead time of one month for May. This may be due to differences in the observations, temporal periods, and areas used for calculating the sea ice extent (Fig. S1)" (3.30-32 in the previous manuscript) from the text, and then added "As for the SIE in the Northern Hemisphere (Fig. S1a), the correlation patterns are similar those in Day et al. (2014b), except for a lead time of one month for May which may be due to difference in observations (Fig. S1d). However, reemergence in winter is weaker than that for SIE_{AO}. This is because SIE_{AO} exclude other regions contributing to the winter sea ice variability." to the text (4.9-12).

4. 4.4: The RMSE values in Fig. 2b are artificially low because SIE_AO doesn't have much winter SIE variability.

Referee is quite correct. We added the reason why the RMSE values are low in winter as follows. "The RMSE values in winter are large (Fig. S2b) compared to Fig. 2b because SIE_{AO} does not include the area where sea ice variability is large." to the text (4.28-29).

5. 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.

In the Sea of Okhotsk, the Bering Sea, and the Labrador Sea, the ACC and RMSE between the observations and the hindcasts for sea ice concentration are lower and higher at the short lead time, respectively, for the hindcasts started in January and April 1st (not shown). This might influence the ACC for SIE in the Northern Hemisphere. In the revised manuscript, we added "The lower ACC at the short lead time for the hindcasts started from January and April (Fig. S2a) may be due to the lower ACC and higher RMSE for sea ice concentration in the Sea of Okhotsk, the Bering Sea, and the Labrador Sea (not shown)." to the text (4.26-28).

6. 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.

As you pointed out, the difference between Figure 2d and Figure S2d is due to the effect of the domain choice. In the revised manuscript, we added "The difference between Figure 2d and Figure S2d is also due to the effect of the domain choice." to the text (4.29-30).

7. 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?

As you pointed out, correlation coefficients between SIV and SIE are significant in all season for the Northern Hemisphere domain (Figure S3a in the previous supplement). In the previous manuscript, we used the domain north of 65^{o}N for computations of SIV and OHC. However, the same domain as the SIE_{AO} should be used, as pointed out by referee #2. The difference between Figure 3a and Figure S3a might be due to the calculation method. In the revised manuscript, we recalculated SIV and OHC in the domain north of 65^{o}N excluding the Hudson Bay and Baffin Bay (please see new Figure 3). Here OHC is integrated from the surface to a depth of 200 m, according to referee's comment 11. On the other hand, we removed Figure S3 in the previous supplement for the reasons mentioned in our response to referee's comment 1.

8. 5.5-6: This is not very surprising, given that other most other regions have been excluded!

As you pointed out, the sentence of "The most significant signals for both SIC and SIT are found in the Barents Sea (BS) of the Arctic Ocean (Figs. 4a and 4b)" may be not surprising result. However, even in the SIE in the Northern Hemisphere (Figures S4a and S4b in the previous supplement), similar but somewhat weak spatial patterns are seen in the BS. This indicates that the BS is one of dominant regions for the December SIE variability not only in the north of 65^{o}N but also in the Northern Hemisphere.

9. 5.6-7: This may be true, but the domain choice biases results towards finding a signal in the Barents/GIN seas.

As mentioned in our response to referee's comment 8, significant signal in the BS can be seen even in the case of the Northern Hemisphere domain, although a signal in the GIN Sea disappears and significant signal appear partly in the North Pole, the Labrador Sea, and the Hudson Bay.

Major Comment 2) Definition of OHC and advection mechanism

10. 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.

As suggested by the previous studies [e.g., [5]Nakanowatari et al., 2014], ocean temperatures around a depth of 200 m are effective for the sea ice prediction at the long lead-time. Motivated by the previous studies, we focused on the subsurface water as one of key variables that could provide memory on seasonal-to-interannual sea ice variability. In the previous manuscript, we did not consider the heat content within the mixed layer, to remove the direct effects due to the atmospheric heating and cooling. However, referee #2 has also commented the definition of the OHC and advection processes. In the revised manuscript, we recalculated the OHC. Please read our response to referee's comment 11.

5. Nakanowatari, T., Sato, K., and Inoue, J.: Predictability of the Barents sea ice in early winter: Remote effects of oceanic and atmospheric thermal conditions from the North Atlantic, J. Clim., 27, 8884-8901, doi:10.1175/JCLI-D-14-00125.1, 2014.

11. 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.

According to your suggestions, we reconstructed Figures 3 and 4 using the OHC from the surface to a depth of 200 m (please see new Figures 3 and 4), and rewrote the text (please read Section 4 in the revised manuscript). For comparison, we also added Figures 3d-3f and Figures 4c-4f in the previous manuscript to supplement as new Figure S4.

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

In Figure 4, we used only data from the hindcasts (i.e., the model-predicted SIE_{AO}).

Major Comment 3) Lagged correlation analysis

13. 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.

As you pointed out, comparison of Figure 1a and result of Day et al. (2014) was not fair. In the revised manuscript, we rewrote the text by comparing Figure S1a and Day et al. (2014) as follows. "As for the SIE in the Northern Hemisphere (Fig. S1a), the correlation patterns are similar those in Day et al. (2014b), except for one month lead time of May which may be due to difference in observations (Figs. S1d)" (4.9-10).

14. 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.

As suggested, we added "Lagged correlations of SIE_{AO} with NSIDC data" to new Figure S1 (please see new Figure S1e). For the reasons mentioned in our response to referee's comment 1, however, we mainly show results using the domain north of 65°N.

Minor Comments:

15. 1.29: I suggest changing "predictions" to "projections", to make this distinct from the seasonal predictions that are the primary focus of this paper.

As suggested, we replaced "predictions" with "projections" (1.29).

16. 2.6: Is this based on detrended SIE or full SIE anomalies?

This is based on detrended SIE. We added "detrended" to the text (2.6).

17. 3.1: Should specify that this is ocean temperature.

As suggested, we rewrote the text (3.1).

18. 3.2: What ocean data goes into the objective analysis of Ishii et al. (2006)? What SIC data is used?

Ocean data is based on the latest observational databases [the World Ocean Database (WOD05), World Ocean Atlas (WOA05), and Global Temperature Salinity Profile Program (GTSPP) provided by the U.S. National Oceanographic Data Center (NODC) and a SST analysis [Centennial in situ Observation Based Estimates of variability of SST and marine meteorological variables (COBE SST); [6]Ishii et al. (2005); [7]Hirahara et al. (2014)]. Also, SIC data is based on satellite observations from the Nimbus-5 Scanning Multichannel Microwave Radiometer (SMMR), the Special Sensor Microwave Imager (SSM/I), and the Special Sensor Microwave Imager/Sounder (SSMIS; [8]Armstrong et al., 2012).

6. Ishii, M., Shouji, A., Sugimoto, S., and Matsumoto, T.: Objective analyses of SST and marine meteorological variables for the 20th century using ICOADS and the Kobe Collection. Int. J. Climatol., 25, 865-879, doi:10.1002/joc.1169, 2005.

7. Hirahara, S., Ishii, M., and Fukuda, Y.: Centennial-scale sea surface temperature analysis and its uncertainty. J. Climate, 27, 57-75, doi:10.1175/JCLI-D-12-00837.1, 2014.

8. Armstrong, R. L., Knowles, K. W., Brodzik, M. J., and Hardman, M. A.: DMSP SSM/I-SSMIS Pathfinder daily EASE-grid brightness temperatures, Jan 1987-Dec 2011. National Snow and Ice Data Center, CO, digital media. [Available online at http://nsidc.org/data/nsidc-0032.html.], 2012.

In the revised version, we added "Ocean data is based on the latest observational databases [the World Ocean Database (WOD05), World Ocean Atlas (WOA05), and Global Temperature Salinity Profile Program (GTSPP) provided by the U.S. National Oceanographic Data Center (NODC) and a SST analysis [Centennial in situ Observation Based Estimates of variability of SST and marine meteorological variables (COBE SST); Ishii et al. (2005); Hirahara et al. (2014)]. Also, SIC data is based on satellite observations from the Nimbus-5 Scanning Multichannel Microwave Radiometer (SMMR), the Special Sensor Microwave Imager (SSM/I), and the Special Sensor Microwave Imager/Sounder (SSMIS; Armstrong et al., 2012)." to the text (3.3-9).

19. 3.19-20: This is unclear and needs to be explained more precisely.

Probably, we are misleading referee's comment. Here, we calculated the climate drift following to method by INTERNATIONAL CLIVAR PROJECT OFFICE (ICPO, 2011) to remove the climate drift from the hindcasts.

20. 3.28: How close is the SIC from Ishii et al. (2006) to SIC observations? Are there any known biases/differences?

Figure A1 shows the differences between Ishii et al. (2006) and HadISST for summer (July-August-September) and winter (January-February-March) sea ice concentration (SIC). Here we used sea ice concentration from HadISST as observation because of the same horizontal resolution (1^o x 1^o). In summer, higher SIC (+10%) are seen in the Atlantic Sctor of the Arctic Ocean and lower SIC (-10%) in the Pacific Sector (Figure A1a). Although the biased SIC patterns in winter are similar to those in summer except for the Okhotsk Sea (Figure A1b), particularly higher SIC (+20%) are apparent in the

GIN Sea, Labrador Sea. However, these differences are smaller than standard deviation in SIC from the HadISST.

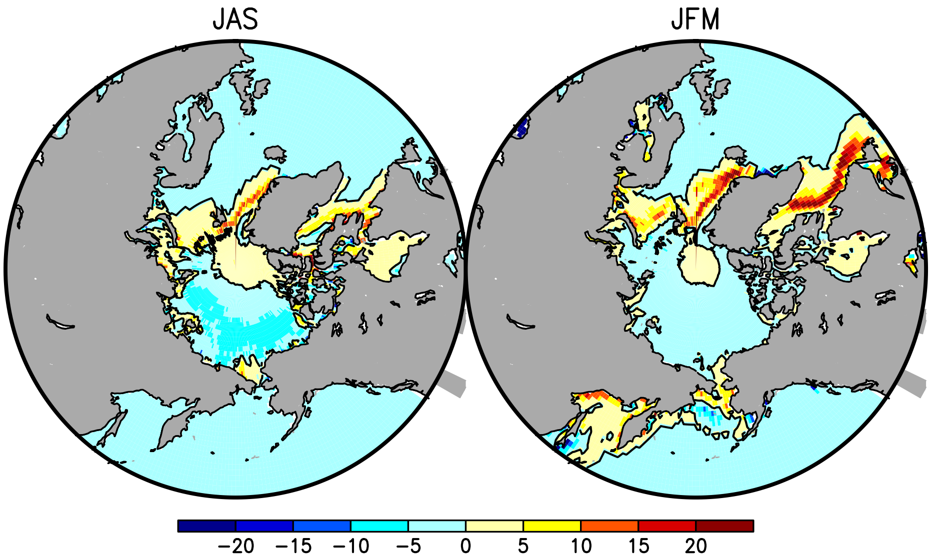

Figure A1. Differences between Ishii et al. (2006) and HadISST for summer (JAS; July-August-September) and winter (JFM; January-February-March) averaged sea ice concentration (SIC, %). Positive and negative values mean that SIC is higher and lower in Ishii et al. (2006) than HadISST.

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

As suggested, we added legends to Figures 2c and 2d. Please see new Figure 2.

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

We modified Figure 2 caption. Please see new Figure 2.

23. 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.

Referee is quite right. We removed "only" from the text (5.3).

24. 4.19: What is "the longest lead time" referring to here? Do you mean "long lead times"?

As you pointed out, "the longest lead time" means long lead times. In the revised manuscript, we replaced "the longest" with "long" (5.3).

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

As suggested, we reconstructed Figure 4. Please see new Figure 4.

26. 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.

According to your suggestion, we added Figure S7 in the previous supplement to the main text as new Figure 5 after the modification using OHC from the surface to 200 m. Please see new Figure 5. Also, this figure is based on the hindcasts as in Figures 3 and 4.

27. 6.7-9: These two sentences contradict one another. Please clarify.

As you pointed out, these two sentences were contradictory. In the revised manuscript, we removed the second sentence "Nevertheless, we note that the forecast skill of summer SIE_{AO} is not necessarily low, because the hindcasts initialized in January and April have significant skills for SIE_{AO} in August and September" (6.7-9 of the previous version) from the text.