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Interactive comment on "Seasonal sea ice prediction based on regional indices" *by* John E. Walsh et al.

Anonymous Referee #1

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Overview and broad comments

This study by Walsh et al uses a long historical record of sea ice coverage data to estimate the amount of variance explained in two (mainly September) quantities, the Beaufort Sea Index and pan-Arctic sea ice extent, by the sea ice extent in different Arctic sub-regions. They consider both concurrent correlations and lagged correlations of these quantities with sea ice conditions for previous months, and separate these correlations into a total and interannual estimation. They argue that a piece-wise linear trend is most appropriate for detrending the different time series for estimating the latter.

The noteworthy pieces of information to come out of this study are (1) a piece-wise linear trend is the best option for detrending the time series considered in the study, (2) the break-point year emergent in that analysis which can be interpreted as an acceleration of the ongoing negative trend in sea ice cover is in the mid- to late- 1990's, (3) that interannual variability in the BSI can be explained by June sea ice coverage in the Beaufort Sea and by July coverage in the Chukchi Sea, (4) consistent with other studies the September pan-Arctic ice extent has significant autocorrelation back to July (about two months), and (5) that the Laptev and East Siberian Seas explain the most concurrent correlation in September pan-Arctic SIE.

As it stands though, in my opinion the paper requires substantial revisions and additional analyses before being re-submitted.

The key goal of the study seems to be to provide baseline metrics against which sea ice prediction studies can be evaluated. However, that baseline has already been established for one of the two predictands considered in this study, pan-Arctic SIE, in several other studies based on autocorrelation (i.e. persistence). No reference to these other studies is ever made. The one thing that separates the result about the pan-Arctic SIE predictand in this study from others is that a very long historical record was used here, but that point should be emphasized when motivating the study (it's currently not mentioned). The rest of the lagged correlation analysis for pan-Arctic SIE, which was between it and the SIE for the various subregions, didn't yield higher correlations than lagged pan-Arctic sea ice extent itself. So, for evaluating prediction skill, is it not the autocorrelation of pan-Arctic SIE that is the important baseline to beat? What therefore is the motivation to consider the lagged correlation analysis with the different sub-regions?

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With respect to the BSI predictand, that analysis seems okay and is fine to report on, but I'm not sure all of the information presented (particularly in the tables; see specific comments below) is needed to reach the conclusions made.

I was also a bit surprised (and disappointed) to see that only these two predictands were considered if the goal is to provide baseline skill numbers, especially considering the first predictand has already received considerable attention in other studies. Like in the supplementary material of Bushuk et al 2018 (reference below), it would be good to extend the analysis to treat all of the different regions as separate predictands and compute the autocorrelations for each of those regions. This would help put the results of the Bushuk et al study into context, as they used a shorter and different observational dataset than is used here. As of now, and considering some of my suggestions to remove certain parts of the results section (see specific comments below), this additional analysis would help strengthen that section and would provide useful baseline numbers for future studies.

I was missing from the introduction a sufficient argument for the need for this study in the context of current literature on sea ice prediction. The additional references to, and discussion of some key studies listed below are needed to place the goals of this study and its findings into perspective. Specifically here are studies on persistence/autocorrelation of observed and modeled pan-Arctic and regional sea ice extent:

Blanchard-Wrigglesworth, Edward, et al. "Persistence and inherent predictability of Arctic sea ice in a GCM ensemble and observations." Journal of Climate 24.1 (2011): 231-250.

– Includes estimates of autocorrelation for pan-Arctic SIE and SIA in both observations and models. Also includes contributions of lagged Beaufort Sea sea ice thickness to september SIA and SIE variability.

– This paper was referenced in the study, but it's odd that only the SST re-emergence result was mentioned.

Sigmond, M., et al. "Seasonal forecast skill of Arctic sea ice area in a dynamical forecast system." Geophysical Research Letters 40.3 (2013): 529-534.
– Their Fig. 3 shows persistence forecasts of SIE from two observational datasets.

Bushuk, Mitchell, et al. "Regional Arctic sea–ice prediction: potential versus operational seasonal forecast skill." Climate Dynamics (2018): 1-23.
– In their supplementary material, they show autocorrelations for individual regions from observations and compare against models.

Throughout the paper, the word "predictability" is used somewhat misleadingly. It should be made clear early onthat what is being referred to as predictability throughout the rest of the paper is a very specific estimate of predictability – that is, the predictability of a certain sea ice coverage quantity based on the lagged cross-correlation or auto-correlation (often referred to as memory or persistence) between that quantity and other sea ice coverage quantities. Otherwise, it sounds like the authors are referring to predictability in general, which encompasses all sources of predictability and estimates of theoretical predictability limits.

Guemas, Virginie, et al. "A review on Arctic seaâĂŘice predictability and prediction on seasonal to decadal timeâĂŘscales." Quarterly Journal of the Royal Meteorological Society 142.695 (2016): 546-561.
– Provides a review on sources of predictability, including memory/persistence of sea ice coverage and thickness. See references therein.

Bushuk, Mitchell, et al. "Regional Arctic sea–ice prediction: potential versus operational seasonal forecast skill." Climate Dynamics (2018): 1-23.

– Focuses on regional predictability.

The de-trending analysis could be expanded on. The 1979-onward period is what is commonly used in prediction studies, and there is an ongoing debate in the literature about what de-trending method is most appropriate for pan-Arctic SIE, particularly when the most recent few years are included. Extending the de-trending analysis to focus also on this shorter period would be helpful for future prediction studies. While most authors choose to fit a linear trend over that period, Dirkson et al 2017 suggested a quadratic fit; others suggest de-trending with a high-pass filter but this has the unfortunate effect of removing the first or last sample from the data over an already short period. It would be worthwhile, and would add to the paper, if the authors could make a case for the piece-wise linear trend over this shorter period if indeed it is much better than a linear or quadratic fit. Also, as prediction studies are beginning to focus more on regional sea ice prediction than on pan-Arctic SIE/SIA, it would be helpful to know what choice is most appropriate for the different regions and whether this choice depends on the month or season. For instance, Bushuk et al 2017 and Dirkson et al 2017 argued that linear de-trending was sufficient for regional SIE and local SIC over a similar period considered here.

Dirkson, Arlan, William J. Merryfield, and Adam Monahan. “Impacts of sea ice thickness initialization on seasonal Arctic sea ice predictions." Journal of Climate 30.3 (2017): 1001-1017.

Bushuk, Mitchell, et al. “Skillful regional prediction of Arctic sea ice on seasonal timescales." Geophysical Research Letters 44.10 (2017): 4953-4964.

Specific Comments

- It should be stated in the abstract early on (before L11-13) the prediction method that will be used in this paper.
- L19: "statistical predictability" should be replaced with "statistical skill..." based on said approach. It's not statistical predictability in the sense of a theoretical limit, which isn't directly possible to estimate .
- L17-18; Which month(s) are being referred to here?
- L31-32; Statement requires a reference.
- L37-39; Should cite:
  Maslanik, James, et al. "Distribution and trends in Arctic sea ice age through spring 2011." Geophysical Research Letters 38.13 (2011).
- L47-50; This positive trend in the Bering Sea in winter is disappearing if more recent years are taken into account... Ah, this is stated in the conclusions, but is probably more appropriate to place here instead.
- The departures are also affected by antecedent sea ice conditions themselves. Please see the references given in the general comments above related to persistence.
- L79-82; This is not correct. "Ice-ocean model" implies that both the ice and ocean evolve freely as determined by the model (only the atmospheric forcing is required). Also, the statement ignores the fact that fully coupled models, which determine both the atmospheric and ocean/ice conditions prognostically, are now used more often for sea ice prediction. These models are also limited by the chaotic nature of the climate system, but this is typically accounted for by running ensembles. There is important literature on predictability using said models that isn't referenced or discussed here. Please see general comment above highlighting predictability literature.

- L83-84; It's difficult to see how this paper is an "extension" of Drobot 2003; while this study also focuses on the Beaufort and Chukchi Seas, and extends an analysis of statistical prediction skill in that region based on more recent observational data, the analysis done in Drobot 2003 (statistical prediction using multiple linear regression with many more predictors) is not repeated here.
- L88-92; The caution raised in the Drobot 2003 study I think definitely deserves attention given Arctic sea ice and Arctic climate have changed, as stated. Although this study distinguishes between prediction of sea ice coverage with and without the trend, as written it sounds like the authors will carry out an analysis similar to Drobot 2003, and determine the impact of the changing conditions on the statistical relationships found in that study, which is not what is done here.
- L95-96; This is incorrect. While the Drobot 2003 study didn't consider the effects of detrending as stated, Blanchard-Wrigglesworth et al 2011 did. Specifically they detrended the model projections of SIE by subtracting the ensemble mean at each point in time (removing the forced signal). Additionally, they detrended the SIE observations by subtracting the long-term linear trend.
- L97-100; I find this overview too vague and is broader in scope than what is actually done in the paper. I think it would be more accurate and helpful to the reader if the authors provided, in order of appearance in the paper, what will specifically be done.
- L102; "seasonal climatologies, persistence, and trend" of what? Should clarify that you are referring to these quantities for sea ice coverage itself.
- L104; "ice-ocean models" should be replaced with "dynamical models". Ice-ocean models are a specific subset of these, but fully-coupled models are used more often.

- L105; The Sea Ice Outlook was managed by the Sea Ice Prediction Network, and starting in 2018 became managed by the Sea Ice Prediction Network–Phase 2. The Sea Ice Outlook is not a previous name for Sea Ice Prediction Network. See https://www.arcus.org/sipn/sea-ice-outlook.

- L118; Suggest changing "maps" to "gridded records". Presumably what is being referred to here is data in digitized form and not the presentation of the data literally as maps.

- L124-127; Say the product name here. The product is described in the next sentence without actually saying explicitly that it is used.

- L154-155; While this is true, previous studies have shown that there are shorter persistence time scales for pan-Arctic SIE than for pan-Arctic SIA due to high frequency dynamical influences that change SIE, but not SIA (Blanchard-Wrigglesworth et al 2011; their Fig. 14). This should be mentioned here.

- L157ff; Was there any interpolation done to get the G10010 dataset onto the same grid used to define the MASIE sub-regions?

- L182; Somewhere early on in the methods section it should be stated what years are considered in the study.

- L191-192; Please clarify. I think what is meant is that the presence of a trend in a time series inflates forecast skill when using the anomaly correlation coefficient to assess that skill. It should be noted though that for distance metrics like the root mean square error, skill can actually be inflated by detrending the data if the two time series have different magnitudes of trends.

- L198-202; please refer to general comment on de-trending, but this paragraph would be a good place to reference choices made in other sea ice prediction studies.

- L209ff; As of now, I find the description of the fitting methodology a bit hard to follow. It could be more straight-forward both in terms of its description and implementation. If one thinks of the function as a piece-wise linear trend defined by

$$y = a_1 x + b_1, \quad x < x_b \tag{1}$$

and

$$y = a_2 x + b_2, \quad x > x_b \tag{2}$$

where x_b is the breakpoint year, continuity of y at x_b can be ensured by letting $b_2 = (a_1 - a_2)x_b + b_1$. Substituting b_2 into the equation yields 4 parameters to be estimated (a_1, b_1, a_2, x_b), which can be done using the 'curve_fit' function. This avoids having to also use both the 'curve_fit' and 'lin_regress' function, as the parameters found by 'curve_fit' can be used to describe the two lines completely. Regardless of which method is used, the equation(s) used in 'curve_fit' should be shown in the paper.

- L227-229; This was just stated on L207-208.

- L239ff; This is an interesting result, but is it necessary to include the break-point year for all calendar months from January-September in Figure 5? The piece-wise equation seems like a sensible approach to determine a breakpoint year in the summer months (except maybe in the central Arctic). However, for winter and spring months is it really the case that a two-piece linear trend yields a better mean squared error than a simple linear trend? If not, then the breakpoint year for those months won't carry any significant meaning other than that it's an emergent parameter you get from fitting the two-piece linear trend. Is that perhaps what is being seen in Fig. 5 for years before the 1990's and after the early 2000's? Breaking down the information in Fig. 5 for each month and region would make the results easier to interpret and more informative.

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- L250; Is the reason for showing Figure 6 not also to see what regions contribute the most explained variance to pan-Arctic SIE?
- L256-260; Why say the significance level for this specific autocorrelation value? Is it the maximum autocorrelation value for all of the samples considered?
- L260-262; This is the significance value when the sample has an autocorrelation of zero. Do the detrended time series have no autocorrelation? Also, this statement is true for most regions, but not all according to Fig. 6.
- L263-265; This is true when the trend is included, but not when the time series are detrended according to Fig 6. Please clarify which is being referred to here. Can the authors speculate at all why it is the East Siberian Sea and Laptev Sea explain the most synchronous interannual variance in pan-Arctic SIE? Is it the region that has the most interannual variability in September?
- L266-275; Is Figure 7 really necessary? It seems reasonable to consider the contributions of variability in different regions to variability in pan-Arctic SIE, but is there any physical basis for thinking that regions far away from the Beaufort Sea would show any explained variance in the BSI, apart from the trend? It's worthwhile to know how far from the Beaufort Sea variations matter, but those regions are shown in Fig. 9 already for lag 0.
- Are tables 1-4 necessary?

  For Tables 1 and 3, when the full time series are compared (non-detrended), the fact that there will be correlation between the different regions and the 2 predictands when both predictor and predictand contain trends is not really a surprise, is it? For Tables 2 and 4, the only relevant information to prediction of the BSI is limited to a few (expectantly nearby) regions, and for the most part to a short number of lag months. For those areas and lags where there is any

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explained variance over 10% , that information is already plotted in Figs. 8 and 9. Why not just say when describing Figs 8 and 9 that no explained variance values greater than 10% were found in other regions?

If an argument to keep the tables is made, the shading scheme should be explained.

- L284ff and Figure 8; I'm surprised to see the values of 0.41 and 0.05 for the Beaufort Sea in Fig. 8 (and identical values for the Canadian Archipelago in the tables 1, 2) for lag months January-April. How can this be if sea ice extent is not variable in the Beaufort Sea and the Canadian Archipelago in the winter?

- Figure 8; I think it makes sense to also show the autocorrelation for September pan-Arctic SIE (recognizing it is in Tables 1 and 2; see comment above). It would put the contributions of these other regions into context for prediction purposes. According to Table 2, Pan-Arctic SIE contributes at different lags contributes more to September SIE than any individual region. Should that result itself be mentioned as the most significant (albeit arguably expected) result in terms of prediction? This result should be compared against the studies mentioned in the general comments, but it seems to agree with them. Also, the lagged Laptev Sea SIE contributes more to September Pan-Arctic SIE than the Beaufort Sea, so why not show it too?

  A physical explanation for Laptev Sea can be found in Williams et al 2016:

  Williams, James, et al. "Dynamic preconditioning of the minimum September sea-ice extent." Journal of Climate 29.16 (2016): 5879-5891.

- L288-291; "...reaching zero by...". Near zero, not zero exactly. It's also odd that these are not zero exactly when ice covers these regions completely through April or May.

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- L319-320; Doesn't the BSI contain information from earlier months than September? That is, it's not a strictly September prediction metric.

- L336-338; Specify that this is a general finding; it's certainly not true for all "ice extents" considered.

- L338-340; "(and even years, given the multidecadal scales of the trends)"... While probably true, this isn't a direct conclusion/finding of this study.

- 342-345; The study being referenced here is based on six years of hindcasts which were created by averaging over many different techniques/models, and the results are not reflective of the predictive skill shown by several other prediction studies. To put the results of this paper into context, it should be stated that numerous other studies have shown higher skill than anomaly persistence forecasts (which is essentially the method used here). See references in the introduction of Bushuk et al 2018 (reference in general comments).

- L355; The influence of trend predictive skill is well recognized in the sea ice prediction community, and skill results based on detrending are commonly presented in sea ice prediction studies, so I'm not sure I see how challenge (1) follows from this particular study.

Technical corrections

- Figure 3: I think it would be easier to see and compare magnitudes of the anomalies and trends if Fig 3 were split into two sub-panels: one for the Beaufort and one for pan-Arctic SIE. The sub-panel for pan-Arctic SIE could have double y-axis (one for March, one for September) so that there isn't such a large vertical space between lines.

- L194-197; Sentence is worded awkwardly, specifically the "Because..., so ...".

- Figure 4: It looks like there is a kink in the piece-wise linear functions near the breakpoint. This shouln't be there.
- L255; "accrual".. what is accruing here? Why not just say "corresponding to correlations..."
- L257 and 258; typo? "a 60-year samples" should be "a 60-year sample"
- L296; Change to ..."when sea ice extent" for the Beaufort and East Siberian Seas are predictors. The seas themselves aren't predictors.
- L319-320; incomplete sentence
- L333-335; "(increases of trends)" not needed as it is explained what is meant by break-points in the second part of this sentence.