Answers to review

Arctic regional changes revealed by clustering of sea-ice observations

Amélie Simon[1,2], Pierre Tandeo[1,3], Florian Sévellec[2,3], Camille Lique[2]

[1] IMT Atlantique, Lab-STICC, UMR CNRS 6285, 29238, Brest, France
[2] Univ Brest CNRS Ifremer IRD, Laboratoire d'Océanographie Physique et Spatiale (LOPS), Brest, France
[3] ODYSSEY Team-Project, INRIA CNRS, Brest, France

Reviewer 1

This review is co-signed by François Massonnet and Noé Pirlet (UCLouvain)

Summary.

The authors use an unsupervised machine learning method, namely k-means, to identify regimes of Arctic sea ice concentration (SIC) variability based on the seasonal cycle. They use Mahalanobis distances instead of classical Euclidean distance, to account for the correlation between the months, and the initialization based on equal separation of quantiles for the centroids, to avoid random aspects in the clustering algorithm. They report the mean state and variability / trends of the sea ice state when categorized with this approach.

Novelty.
The work provides interesting insights on the dynamics of Arctic sea ice and is a good topic for a journal like The Cryosphere.

Positioning.
There have been early studies of clustering that are not cited, namely Fuckar et al. (2016) and Lukovich and Barber (2007). The authors should also cite Raphael and Hobbs (2016), since it is an (Antarctic) study that defined regions based on the behavior of sea ice concentration.

Thank you very much for these very interesting and relevant articles.

We have now added in the introduction:
“Using an ocean-sea ice general circulation model, Fuckar et al. (2016) performed a k-means cluster analysis on pan-Arctic detrended sea-ice thickness and found that the associated binary time series of cluster occurrences exhibit predominant interannual persistence with mean timescale of about 2 years.”

“A statistical regionalization method based on observed SIC has been proposed for Antarctica. Raphael and Hobbs, (2014) isolates regions around Antarctica by using sea ice extent decorrelation length scale and variance. The resulting five sectors exhibit distinct times of sea-ice advance and retreat. Their methodology does not account for the temporal evolution of the sectors.”

And in the discussion:
“Besides, by the use of the Silhouette coefficient, we found the Arctic is best described with a number of clusters of 3 (the open-ocean has been added afterward). This number has also been found by Fuckar et al., (2016) using a suite of indices (Krzanowski-Lai, Calinski-Harabasz, Duda-Hart J index, Ratkowsky-Lance, Ball-Hall, point-biserial, gap statistic, McClain-Rao, tau and scatter-distance index) onto detrended sea-ice thickness of an ocean-sea ice general circulation model.”

“In contrast with Fuckar et al., (2016) that calculated time series of occurrences of clusters based on the resemblance of the pan-Arctic pattern, our probabilistic method defines a time series of probability of occurrence of each cluster at the grid cell scale. This enables us to study the spatial evolution of the cluster areas, and therefore define spatio-temporal regions that share a common feature (in our case sea-ice seasonal cycle).”

“Also, Lukovich and Barber (2007) examination of spatial coherence in SIC anomalies indicates that maximum SIC anomalies prevail near the Kara Sea, Beaufort Sea, and Chukchi Sea regions during late summer/early fall from 1979 to 2004. All these studies are consistent with our results showing a decrease in probability for the permanent sea-ice cluster of about 3.1% per decade, …”

Methodological questions

Currently our main obstacle to understanding the research is methodological. Our impression is that the manuscript would deserve to have a better description of the details of the methods of clustering, because as such, we would feel unable to reproduce the results due to many missing details. Since we are unsure about several methodological aspects, we do not comment much on the science itself yet – maybe at the next iteration of the review. Below, we identify several places where we think an improvement could be made.

• L. 224: if we understand correctly, there are as many such matrices of size 73 by 1 123 710, as there are years (45), is that correct? So the k-means clustering is applied on all years individually, which allows producing time series. We think it would be good to mention that already here.

The 123710 includes all the seasonal cycles for all grid cells and all years within the same matrix. We reformulate to be clearer: “The input data of our clustering are all the seasonal cycles including every considered grid cell and every year.”

To help the reader, we have also produced a schematic of the input data for the clustering Figure 2a.

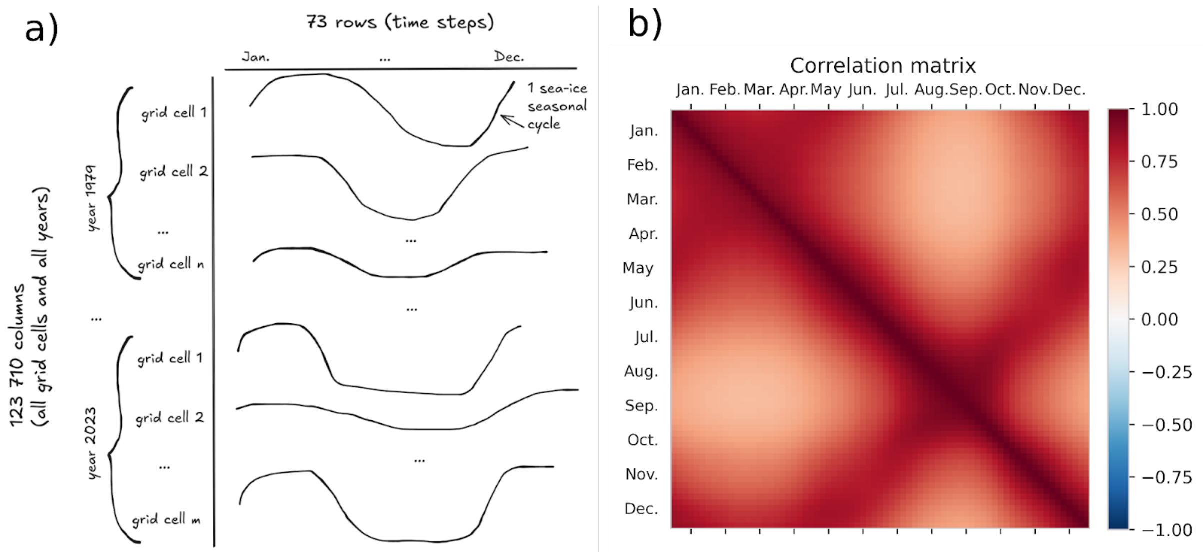


Figure 2: Schematic of the matrix input data for the k-means clustering (panel a) and correlation matrix of the 5-day mean sea-ice concentration for all non-zero sea-ice seasonal cycle above 55°N (panel b)

- L. 229: “It is an iterative method that minimizes a cost function being the sum of the squared distance between each seasonal cycle and its nearest cluster center (also called centroid)”. Here, it would be useful to write (“in a sense to be defined later”) after “distance” because it took us some time to understand how a 73-time frame seasonal cycle could be located in the state space. Also, it could be mentioned here that at each iteration, the coordinates of the clusters are updated.

We now say: “It is an iterative method that minimizes a cost function being the sum of the squared distance (distance in a sense that would be defined later) between each seasonal cycle and its nearest cluster center (also called centroid). At each iteration, the coordinates of the centroids are updated.

- L. 225-229. We feel that a methodological figure could help here, showing how the SIC fields are arranged in a matrix, how this matrix represents a series of points in a 73-dimensional space (you can work in 2-D for illustrations), and how the centroids evolve at each iteration, before a remapping is done in physical space. The Cryosphere is a journal where methods from data science can appear new to some readers.

To help the reader, we have also produced a schematic of the input data for the clustering Figure 2, panel a.

We also now provide the first centroids for the clustering involving 2 to 4 clusters Figure 3, panel b.

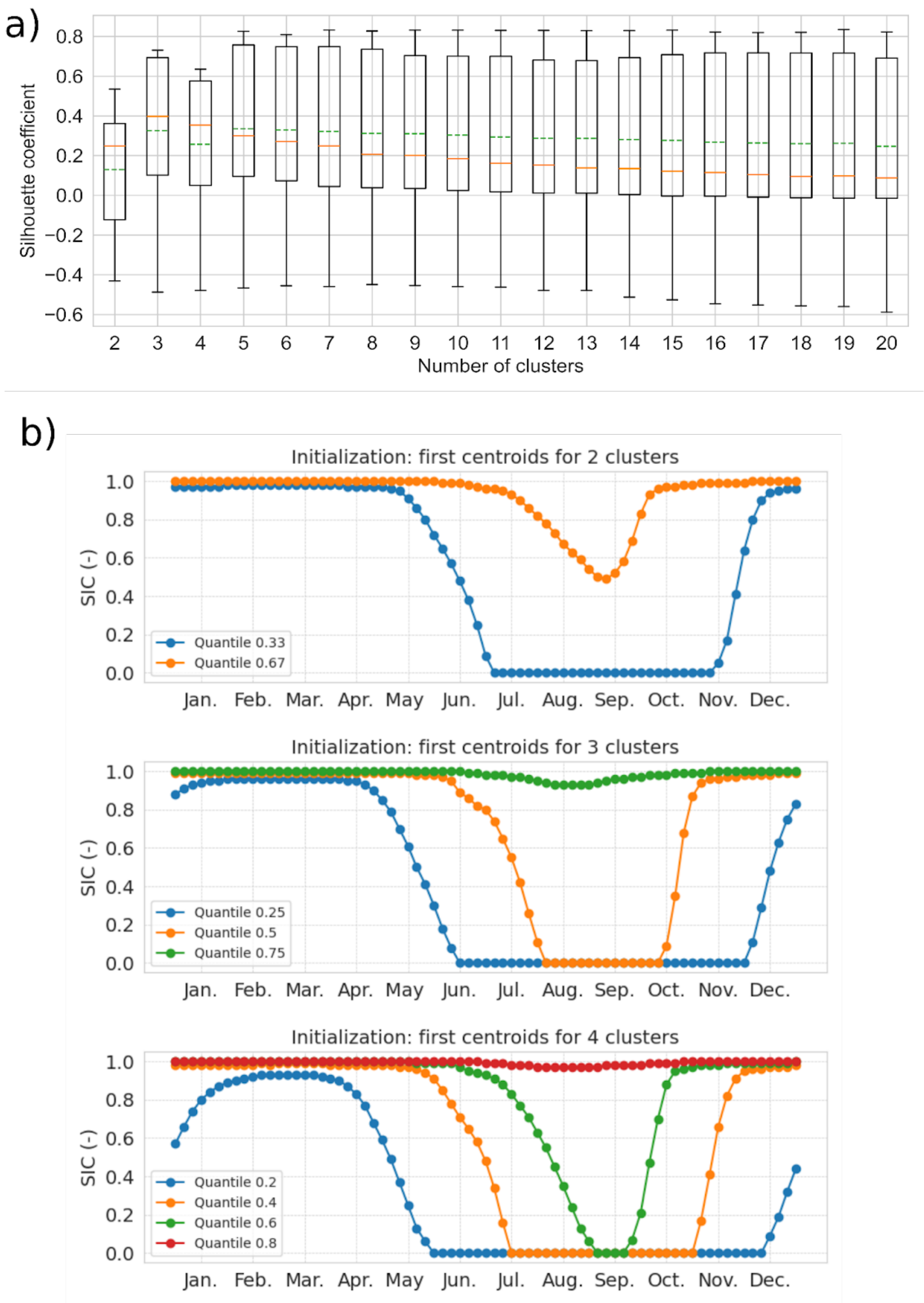


Figure 3: Boxplot of the Silhouette coefficient for a number of clusters from 2 to 20. The box extends from the first quartile (0.25) to the third quartile (0.75) of the

Silhouette coefficient. The whiskers indicate the 1st and 99th percentiles. The green-dashed and orange-solid lines indicate the mean and median values, respectively (panel a). Equal quantile separation initialization: centroids of the first iteration of the clustering for a number of cluster of 2, 3 and 4 (panel b)

• L. 234-236. Has the method of equal quantile separation been tested elsewhere (if so, please cite a relevant study) or is it something that the authors are proposing in this study?

We found a reference on that matter (see e.g. Jambudi and Gandhi, 2022). We now say: “The strategy of initialization based on quantiles has been investigated for synthetic and real dataset and has shown a faster convergence compared to Random and Kmeans++ initialization techniques (Jambudi and Gandhi, 2022). ”

Jambudi T, Gandhi S (2022) An Effective Initialization Method Based on Quartiles for the K-means Algorithm. Indian Journal of Science and Technology 15(35): 1712-1721. https://doi.org/10.17485/IJST/v15i35.714

• L. 234-236. It is not entirely clear how quantiles are calculated on data that has 73 dimensions (and what “quantile separation”, line 235, really means)? We understand that it is possible to compute the distance between any two pairs of points by applying the Mahanabis distance on the two 73-long vectors; do the authors then sort all mutual distances and define the quantiles based on that? If so, how do they revert to one centroid given that the distance involves two points? This method is not described in a way that would allow reproducing the results. Please clarify this part.

The quantile is solely for the initialization of the centroid's coordinates and is classically computed without using Mahalanobis distance. Instead of initializing randomly the first coordinates of the centroids, we fix it to be the quantiles. The next iteration does not account for quantiles.

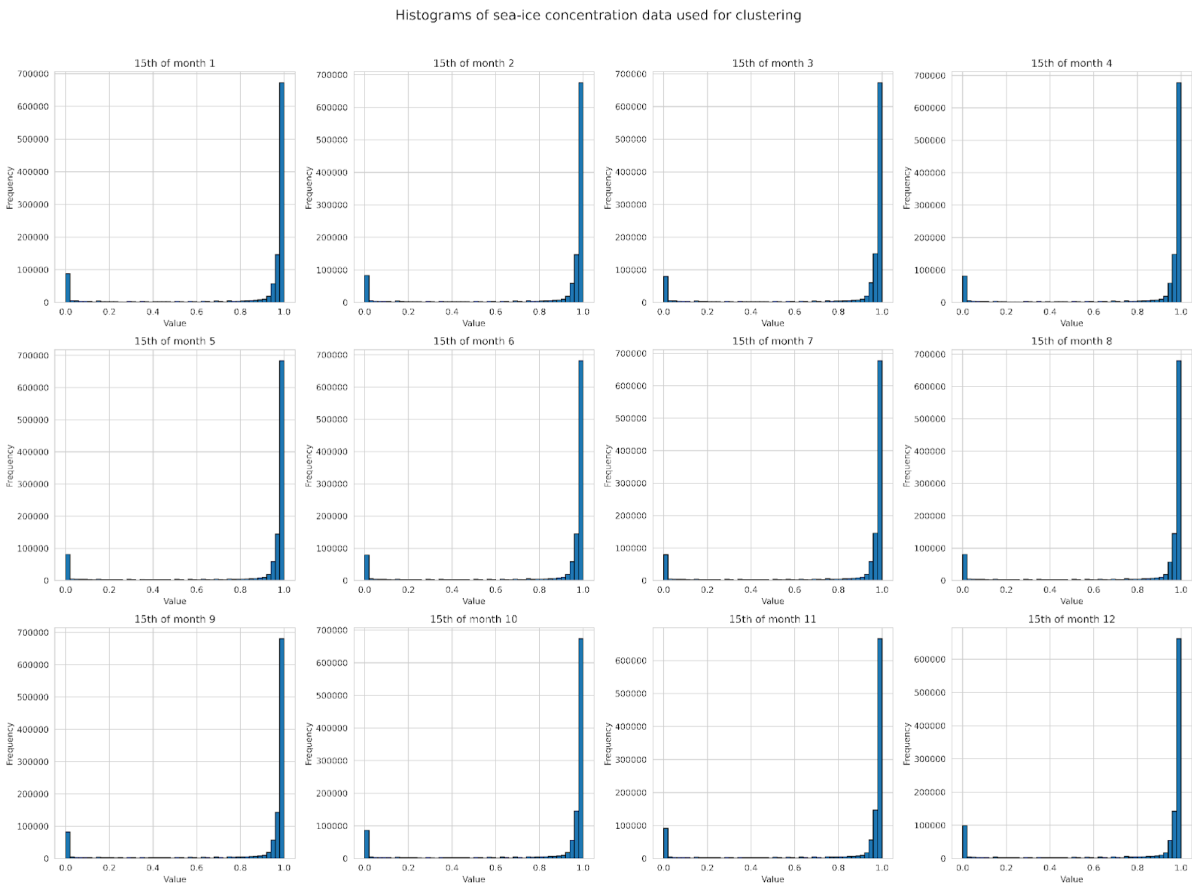
We have added Figure 3, panel b to make it more clear.

• L. 246-260. It is not entirely clear how the correlations of Fig. 2 are calculated. If we consider, for example, the entry that connects the first 5 days of February and the first 5 days of June, then what exactly is calculated? Do the authors first average SIC in space and then compute correlation over all pairs of these averages on the 45 years? Do the authors compute correlation over space (in that case, do they stack the 45 years on each other, effectively producing two vectors of 45 times 1 123 710 length? Also isn’t there an issue with using correlation here, since most points are either at 0 or at 1 ? A scatter plot would reveal something very different from what a correlation aims to capture from a standard cloud ofpoints. Can the authors elaborate a bit on these two points? Could the authors show the histograms of SIC > 55°N for the 15th of each month, for example?

Thank you. We correlate the matrix of Figure 2, meaning we do not average anything, we directly correlate all non-zero seasonal cycles in time and space.

We have now more explicitly explained the calculation: “The correlation matrix is computed for all nonzero seasonal cycles for the period 1979-2023 above 55 °N. It is calculated from the matrix of shape (73, 1123710), having 1123710 value of SIC for 73 dates.”

As suggested, we have plotted the histogram of SIC for the 15th of each month that even if it shows a lot of 0 and 1, shows some nuances. Even if not ideal, we think that the correlation is acceptable.



• Fig. 2: the Figure caption is too short, but instead should allow reproducing the figure unambiguously.

We now say in the caption of Figure 2: “Correlation matrix of the 5-day mean sea-ice concentration for all non-zero sea-ice seasonal cycle above 55°N”

• L. 246-260. These correlations are computed without deseasonalizing nor detrending the data, is that correct? When one applies other data reduction methods like PCA/EOFs, the forced variability is removed first – is it the case Here?

The concept is different from PCA/EOF technique, as we do not work with time series but with seasonal cycles. Also, usually the PCA/EOF are used to assess mode of variability, so trends are removed. Here we want to describe the trend, but through changes in regions having typical seasonal cycles. The two approaches are therefore different. In our case, we don't detrend the data.

• L. 264: what does "normalizing by the correlation matrix" mean here? Divide entry-wise by each element? If multiplying by the inverse of this matrix, then use this phrase – one cannot "divide" by a matrix. Also in L. 256 we read "normalized by the inverse of the correlation matrix" while L. 264 "we read "normalized by the correlation matrix". Could the authors clarify this?

Thank you. We now say: " we do not normalize the distance by the inverse of the covariance matrix (as usually done for the Mahalanobis distance) but by the inverse of the correlation matrix"

• L. 373: please explain how M_cor is constructed (related to our other comment on the corresponding figure).

M_cor is the correlation matrix calculated from all seasonal cycles (all years and all grid points). We now say: "The correlation matrix is computed for all nonzero seasonal cycles for the period 1979-2023 above 55 °N. It is calculated from the matrix of shape (73, 1123710), having 1123710 value of SIC for 73 dates".

We have modified this part, as we no longer used the Mahalanobis for the calculation of probability. We use the Euclidean distance for that. We still use the Mahalanobis for the clustering though. We now say:"The Mahalanobis norm, deriving from a symmetric operator, effectively rotates the original physical phase space (here., date of the annual cycle) to align with the data's natural directions—linear combinations of the physical time axis. This transformation allows centroid detection in a space that reflects the intrinsic structure of the data. Therefore, using the Mahalanobis distance helps the clustering algorithm to follow the direction of the correlation and capture the elongated shapes of clusters. When calculating the probability to belong to one cluster, we do not need to work with the data's natural directions, but rather work in the original physical time space. Therefore we use Euclidean distance for the calculation of probability and the Mahalanobis for the clustering."

• L. 379-382: So there the authors used fuzzy k-means but then in the previous section, it was not fuzzy ? This is not very clear, and calls for a better justification of the transition between these two sections. Also, is there an objective criterion to prefer fuzzy k-means over crisp assignments? In general, how do we measure whether the k-means did a good job or not (is it with the Silhouette metric, and if so, what is a "good" clustering)?

We now say: “This means that we use a “fuzzy” k-means clustering where the assignment is soft (each data point can be a member of multiple clusters) in contrast to a hard or crisp assignment (each data point is assigned to a single cluster; Jain et al., 2010).”

Presentation

• L. 73 : We think « sea-ice » with a dash should be used when it is used as an adjective; when used as a noun, it should be « sea ice »

Thank you. We have modified accordingly.

• L. 140-157: on the regionalization: it is not entirely clear why this paragraph is here. As we understand, no regionalization is required since the k-means method picks the optimal clusters which can then be used to defined physically-relevant regions. We would propose to move this paragraph to the discussion, since the regionalization is more an outcome of the work than a pre-requisite.

We have made it more clear that we have a double objective: “We determine Arctic regions based on statistically different sea-ice seasonal cycles, and describe Arctic changes through the time evolving borders of the regions characterized by these seasonal cycles.”

• L. 158-172: the emphasis on regionalization for this paragraph (which follows another paragraph on regionalization) shifts somewhat the research question from the initial goal (understanding the physical regimes) to another goal (defining objective geographic boundaries). We would propose to focus the work on the former, and to discuss later in the text how the k-means can also be seen as a way to provide physically-based regions based on distinct sea ice dynamics. Of course, identifying spatial clusters and delimiting regions are two tasks that go hand in hand, but to us the main research question is not always clear.

We have made it more clear that we have a double objective: “we determine Arctic regions based on statistically different sea-ice seasonal cycles, and describe Arctic changes through the time evolving borders.”

• L. 200 to 202: If 5-day mean shows similar results than for 1 day why not keeping 1 day as temporal resolution ? Could the authors precise their reasons for using 5-day mean instead of 1 day?

As the computation is quite long, we keep it to 5-days, as it gives similar results as daily data. We have added the following figure in the supplementary to demonstrate this.

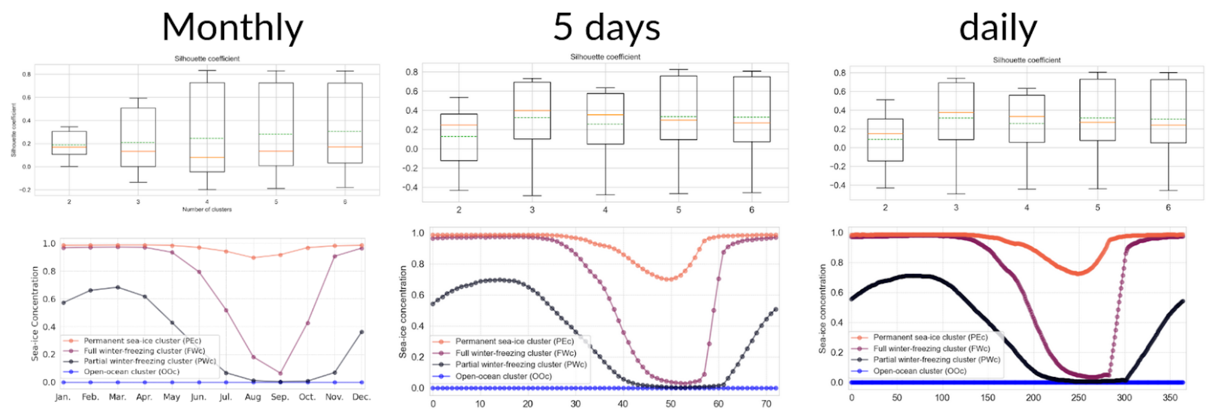


Figure S1: Comparison between monthly (left), 5-day (middle) and daily temporal resolution (right). The Silhouette coefficient for a number of clusters from 2 to 6 (top row), the four types of seasonal cycles (middle row) and a map of the four labels (stable, stabilization, unstable, and destabilization) used to describe the evolution of Arctic clusters based on sea-ice seasonal cycles (bottom row). In the top row, the box extends from the first quartile (0.25) to the third quartile (0.75) of the Silhouette coefficient. The whiskers indicate the 1st and 99th percentiles. The green-dashed and orange-solid lines indicate the mean and median values, respectively.

• Fig. 9 is never cited

Fig 9 is now Fig 10. We now say: "Figure 10 illustrates how we define the stabilization and destabilization labels."

• On printed sheets of paper, the figures do not render very well.

We have improved the quality (by using eps instead of png in inkscape).

• Line 358 to 359. The transition from the section 3.1 to 3.2 is quite enigmatic. Section 3.1 seems to deal with deterministic clustering where each point is assigned to a cluster while section 3.2 deals with probabilistic clustering. We would propose to put the explanation of lines 362-363 at the end of the section 3.1 to make a smoother transition

As suggested, we introduced a smoother transition for the sections by naming the reason for introducing the probabilities: "As a given seasonal cycle can be in between two or more seasonal cycle centroids, we introduce the probability to belong to one cluster in the next section."

• Line 392: Could the authors explain why they chose to divide the period into 3 sub-periods of 15 years ? It seems a bit arbitrary and it's hard to understand where the authors are going with this sub-time division. Could the authors add a bit of context to introduce that choice?

We have 45 years of data. 45 is divided by 15 and 3 rows are a good compromise for the size of the figures.

• Line 456 to 459 : We find it unclear whether the probabilities of belonging to a cluster given in this paragraph are the average of the probabilities over all the years or just one year? Looking at Figure 7, it seems that this is for 1979. If that's the case, why take one year rather than the average? Could the authors clarify This?

Good point, we have now calculated the average and say: “The probability of belonging to the open-ocean cluster is around 40%, to the permanent sea-ice cluster is around 29% and to the full winter-freezing cluster is around 18 % and to the partial winter-freezing cluster is around 13% (Figure 7).”

• Line 511. We are unsure about what the authors are trying to imply/measure by defining “stable”, “stabilization”, “unstable” or “destabilization” regimes. In particular, they should relate those regimes to something known from the literature or give more interpretation because this notion and its application come somewhat out of the blue.

We propose these new regimes to quantify the stability and transition of the Arctic sea-ice seasonal cycles and refine the description in several parts of the text.

Minor comments

• L. 43 « optimal » should be accompanied by « (in the sense of statistical dissimilarity) » or something like that, because otherwise this word can be wrongly interpreted.

We now say: “Without providing prior information, this data-driven method shows that the Arctic is best described by four types of seasonal cycles ...”

• L. 45. The use of the phrase “ice-free conditions” should be taken with care here and throughout the manuscript, since ice-free has a well-defined meaning in the climate projection literature (namely, sea ice extent < 1 million km2). What exactly is ice-free in the context of this study? We also propose to add a horizontal line on Fig. 4a to represent the chosen threshold, in order to quickly see when the seasonal cycle shows ice-free conditions.

Thank you. We now clearly define that in this study ice-free conditions occur when SIC < 0.15. To not overload the Figure 4a, we say it in the abstract :”two clusters showing ice-free conditions (SIC < 0.15)”

and in the main text :”We refer to ice-free conditions when SIC is below 0.15.”

• l. 43-45: When reading this part of the abstract, it sounds like the authors are re-discovering two regimes that are well-known (open-ocean and permanent ice) and two intermediate regimes where ice is present seasonally. Since the work goes deeper than re-inventing these regimes, we would propose to describe the clusters with more physical interpretation and to highlight what the k-means analyses have allowed to do, that the human eye is not able to see (i.e., what is the added value of an algorithm that can deal with large amounts of data in a high dimension space)

Thank you. We now say: “Without providing prior information, this data-driven method shows that the Arctic is best described by four types of seasonal cycles: ...”

• l. 50 likelihood reduction à likely reduction? Or do the authors mean that the likelihood of this regime in the Canadian side has reduced? Maybe likelihood should be changed by “probability of occurrence” to make things clearer?

We now say: “The pan-Arctic probability to belong to the permanent sea-ice seasonal cycle has decreased by 3.1 %/decade which is compensated with an increase of probability to belong to the open-ocean cluster (1.6 % per decade), the full winter freezing cluster (1.1 % per decade) and to a smaller extent to the partial winter-freezing cluster (0.5 % per decade)“

• L. 52. You mean that spatial redistributions occur within the four clusters ?

Yes, exactly. We now say “spatial redistributions”

• L. 53-55 : it is grammatically strange to write that a “sea” is stabilizing. It is the state characterized by a given cluster that becomes less frequent, in a given region/sea.

We explicit what that means by saying: “From the Beaufort to the Kara Seas, the southern parts have stabilized (experiencing a new typical seasonal cycle, corresponding to the full winter-freezing cluster) and the northern part have destabilized (losing their typical permanent sea-ice seasonal cycle).”

• L. 54-55: be consistent grammatically: « have destabilized » ... « have stabilized »...?

Thank you. We have modified it.

• L. 113 : % relative to what ?

It is relative to the period 1979 to 2018. We say: ”the September SIE exhibits a decreasing trend of - 12.8 ± 2.3% per decade over the period 1979 to 2018”.

• L. 116 : maybe « and do not consider changes in the underlying processes” ?

We want to emphasize that, on top of analyzing if there is sea-ice or not, changes can be made within the sea-ice regime. We say: “However, trends of SIA or SIE only inform about changes in regime from ice to open-ocean and do not consider changes in sea-ice features.”

• L. 125: it would be worth mentioning that these studies have highlighted an asymmetry in the trend of retreat vs advance (see Lebrun et al. In particular).

Indeed, the current manuscript also reveals an asymmetry in the seasonal cycle, so there is a nice connection to be made here.

Exactly. We say that in the following paragraph: “(iii) the trend of later ice advance is expected to eventually double that of earlier retreat over this century, shifting the ice-free season into autumn (Lebrun et al., 2019)”

• L. 128 it is true that these studies do not inform on the sea ice dynamics including melt and growth behaviours, but does the present study do so, given that it does not seek to study the mass balance terms nor the time derivatives of sea ice Concentration?

The melt and growing behaviors are taken into account in our analysis through diagnostics of three different shapes of sea-ice. It includes the time derivative of SIC. But we agree that dynamics could not be fully accounted for without considering the thickness.

We add in the limitation:”Another limitation of this study is that sea-ice dynamics are analysed using sea-ice concentration, rather than sea-ice volume (which would better represent sea-ice behaviour, including growth and melting), due to the lack of robust and long-term sea-ice thickness data.”

• L. 131-132. The statement that previous studies have not directly considered the full sea ice seasonal cycle, is not entirely correct. For example, previous studies looking at SIE report the seasonality of trends, of the mean state, of the variability. But they consider each point of the season separately, while in the present study the seasonality is accounted implicitly for while producing the clusters, through the correlation matrix.

Yes, previous studies have reported the seasonality of trends, which we state in the same sentence by saying: “These three ways of describing the variations in Arctic SIC (trend of SIE, type of sea-ice, ice-free duration), without considering directly the full sea-ice seasonal cycle, have nonetheless highlighted changes in the shape of the sea-ice seasonal cycle: (i) the trend in SIE depends on the season…”. However, we think that these studies have not accounted directly for the full seasonal cycle in the diagnostics.
We also add in the limitation:”The major drawback of our approach resides in the exact grid point quantification of the real seasonal cycle features, as we gather grid cells within a type represented by a single seasonal cycle (the centroid). However, considering the full seasonal cycle gives useful information, as its derivative gives the period of melting and growth. Therefore, the two diagnostics complement each other nicely.“

• L. 216 a “non-zero” seasonal cycle could be interpreted differently by different readers. We assume you mean “having at least a non-zero value for SIC throughout the year” ?

We now say: “We consider all oceanic grid cells above 55°N having a non-zero sea-ice seasonal cycle (having at least a non-zero value for SIC throughout the year)”

• Line 298: “one of the two outputs” leads the reader to wonder what the other output might be, and it is not displayed in the same paragraph. It is only introduced at line 347. Please mention that the other output (the connection of each grid point to a cluster) is studied later.

We have reorganized this paragraph to introduce directly the two outputs. We now say: “The clustering method connects each seasonal cycle to a given cluster (Figure 4a) and provides the centroids of each cluster (Figure 4b).“

• L. 302: follows à lags?

Yes, we now say: “follows the minimum solar insolation by a lag of around 3 months”

• L. 339 : apparition à appearance ?
We have corrected it. Thank you.

• Fig. 4 : the caption is not sufficient to understand what is plotted. Panel (a) shows the average concentration over each cluster, is that correct? How is the time dimension dealt with here? Are the average SIC determined for each cluster, then averaged in time?

Panel a shows the centroid of the clustering method. These centroids are seasonal cycles. It is the output of the clustering method.

We now say in the caption: “Four types of seasonal cycles (output of the clustering method, called centroids)”

• Fig. 4b. have the authors plotted these maps for iconic years like 2007 (big ice arch between the Greenland and the Kara Sea or 2012 (absolute minimum)? That would be interesting to see to what extent the method captures the physics of those events.

We now comment on these two years as follows: “ Also, looking at the years with marked extremes in September sea ice extent, (2007, 2012, 2016 and 2020; see introduction), the MIZ categorization shows a transfer of area between the packed ice and the MIZ. In our clustering vision, 2007, 2012 and 2020 show a transfer of area between the permanent sea-ice cluster and full winter-freezing cluster while 2016 show a transfer of area between the full winter-freezing and the partial winter freezing, reflecting different dynamical changes in the sea-ice seasonal cycles. Therefore, our clustering analysis presents a more detailed description of the MIZ category. “

• Line 370 centroids à centroid

We have corrected it. Thank you.

• Fig. 5: From a quick look, one could argue that the method is not really able to sort the data, as all four clusters have ~25% probability near the ice edge. We were both surprised to see so little variability in the maps, as they seem to have quite homogenous spatial distributions of probabilities.

Thank you very much. We have now decided to calculate the probability using the euclidean distance, which better splits the regions (we still use mahalanobis for the clustering).

We now say: “The Mahalanobis norm, deriving from a symmetric operator, effectively rotates the original physical phase space (here, date of the annual cycle) to align with the data's natural directions—linear combinations of the physical time axis. This transformation allows centroid detection in a space that reflects the intrinsic structure of the data. Therefore, using the Mahalanobis distance helps the clustering algorithm to follow the direction of the correlation and capture the elongated shapes of clusters. When calculating the probability to belong to one cluster, we do not need to work with the data’s natural directions, but rather work in the original physical time space. Therefore we use Euclidean distance for the calculation of probability and the Mahalanobis for the clustering.”

The new Figure

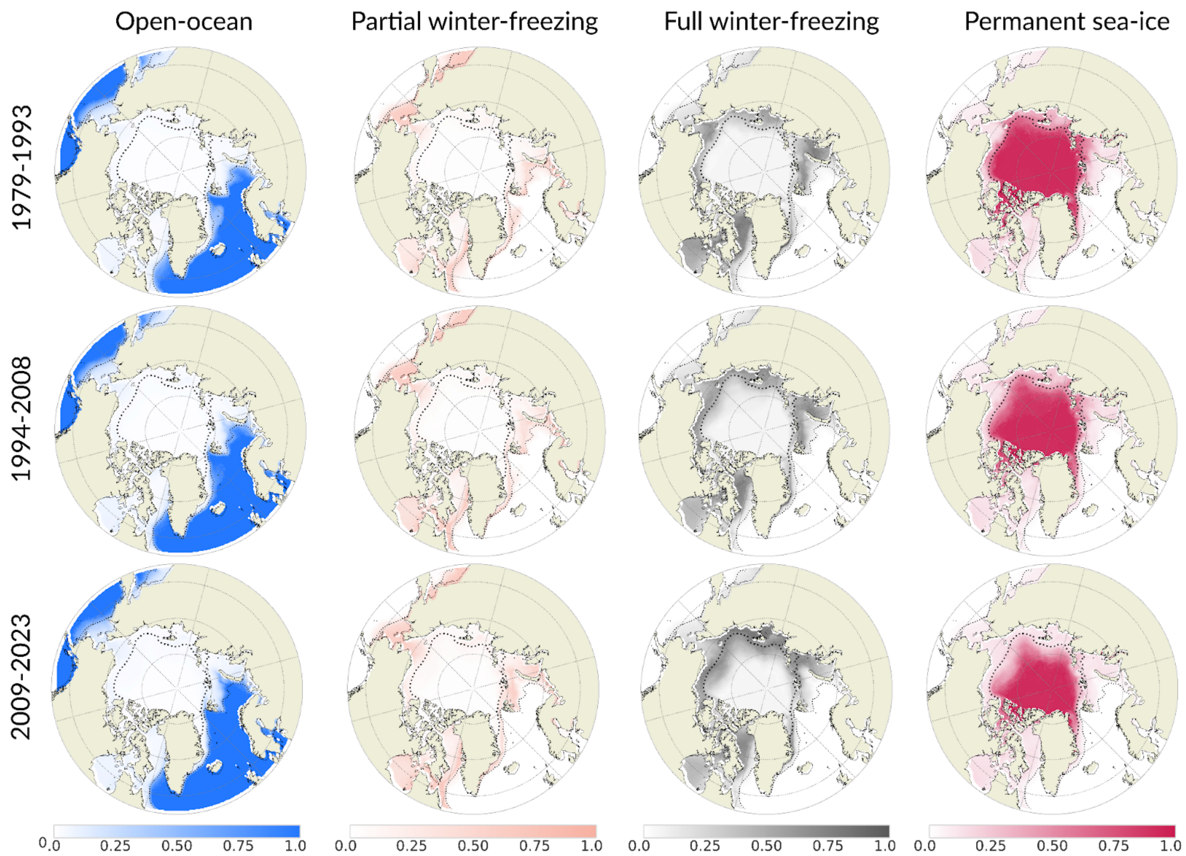


Figure 8: Map of the probability of each cluster: open-ocean (first column), partial winter-freezing (second column), full winter-freezing (third column) and permanent sea-ice (fourth column). Rows correspond to three periods of 15 years: 1979-1993 (top row), 1994-2008 (middle row) and 2009-2023 (bottom row). The dotted thin and thick lines are the mean SIC of 0.15 and 0.8 for the period 1979-2023, respectively. The circle sitting over the north pole is the pole hole (see section 2.1).

• Fig. 7. How is the “total probability” calculated here? Each spatial point has been assigned a probability, are these probabilities then averaged in space? Does it make physical sense to average probabilities in space?

The total probability is calculated by assigning to each spatial point, four probabilities (one for each cluster) and then summing for each cluster its associated probability over the whole domain. We do not average in space, we sum.

We say:” We call the total probability, Pt, the normalized area weighted probability over all grid cells. We sum, for each year, the probability weighted by the area of each grid cell over all grid cells divided by the sum of the probability weighted by the area of each grid cell over all clusters and all grid cells.

• Fig. 8: y-label units should be m2 not km2

Thank you very much. We have corrected it.

• Fig. 8: please repeat in the caption how Open-ocean, MIZ and Packed ice are Defined.

Good idea. For clarity, we now say in the caption: ”Figure 8: (a) Time series of the total area covered by each of the four clusters. (b) Times series of the area covered by three categories: packed ice (0.8 < SIC < 1), the Marginal Ice Zone (MIZ; 0.15 < SIC < 0.8) and the open-ocean (SIC< 0.15).

• Fig. 8: how do we explain that there is much less interannual variability in the (b) panel (that is, hand-made clustering) compared to (a) (k-means)?

Thank you so much. Thanks to your comment we have noticed a bug when plotting the old Figure 8b. We have corrected it and the new figure has a similar interannual variability for panel a and b.

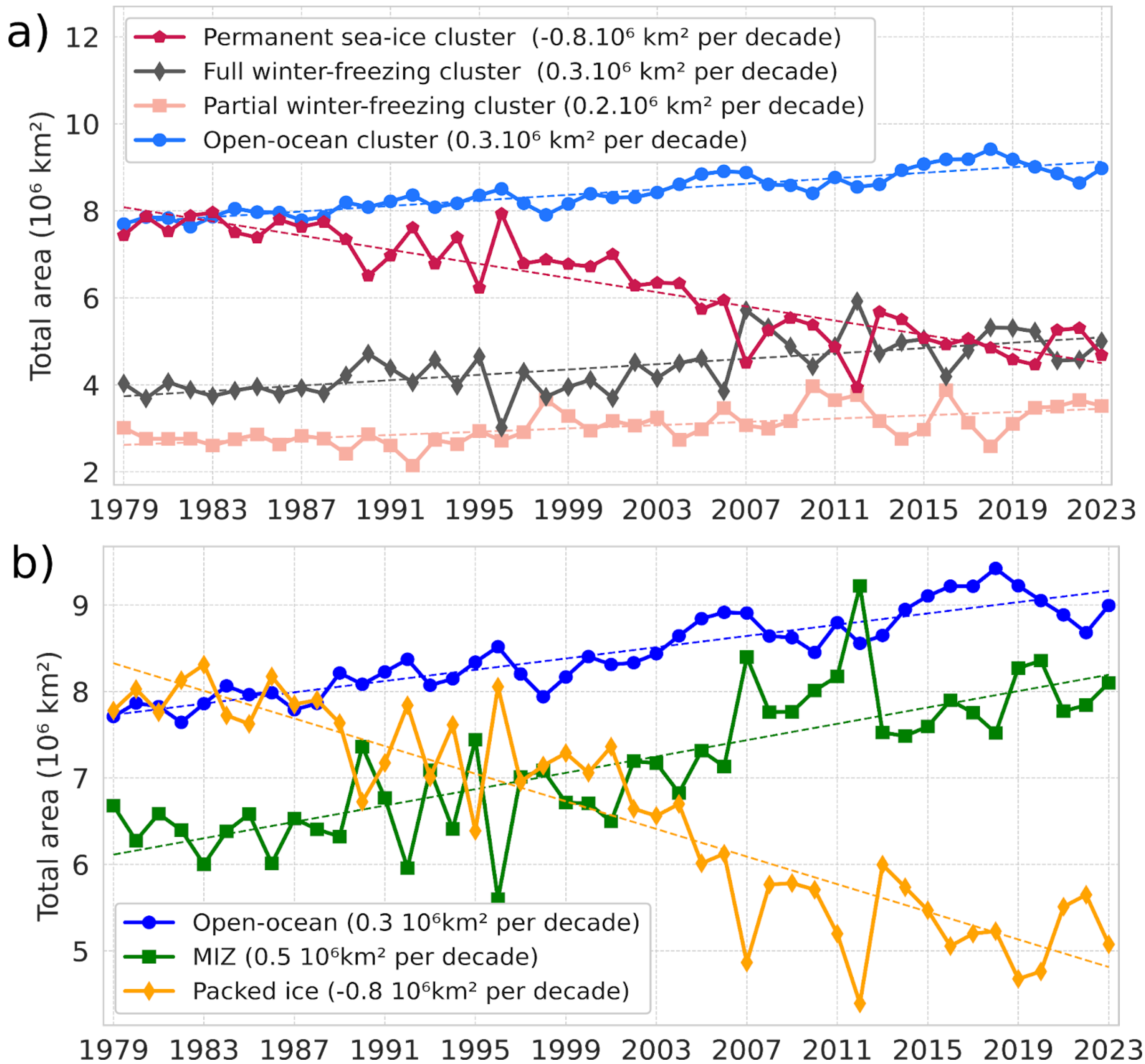


Figure 6: (a) Time series of the total area covered by each of the four clusters. (b) Times series of the area covered by three categories: packed ice (0.8 < SIC < 1), the Marginal Ice Zone (MIZ; 0.15 < SIC < 0.8) and the open-ocean (SIC< 0.15). All curves show a significant linear trend with a p-value less than 0.05 using a Wald Test with a t-distribution.

• Lines 505-509: it would be good to interpret the fact that 2007 is only visible for the permanent cluster, in terms of existing literature that studied this event. In fact, we would argue that the signature is also visible in the full winter-freezing cluster, in "negative". That is, the 2007 event seem to be interpretable as a strong drop of perennial ice but also as a strong surge of the full winter freezing cluster. Why is it so? We would have expected to see a surge of the open water cluster in 2007, not the winter cluster.

Thanks. We now say:"Also, looking at the years with marked extremes in September sea ice extent, (2007, 2012, 2016 and 2020; see introduction), the MIZ categorization shows a transfer of area between the packed ice and the MIZ. In our clustering vision, 2007, 2012 and 2020 show a transfer of area between the permanent sea-ice cluster and full winter-freezing cluster while 2016 show a transfer of area between the full winter-freezing and the partial winter freezing, reflecting different dynamical changes in the sea-ice seasonal cycles. Therefore, our clustering analysis presents a more detailed description of the MIZ category. "

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