
Reviewer 2

General comments:

Arctic sea ice is one of the most affected components by climate change. Understanding its evolution over the last 40 years is key to prepare for further amplification of Arctic warming. As such, physical variable can be used to examine the different behaviors of sea ice. The present study addresses the following question:

What insights can be gained about sea ice behavior trends through the application of clustering techniques? In this work, the authors relied on sea ice concentration (SIC) seasonal cycle to identify 4 sea ice behaviors instead of the classical approach of splitting the Arctic based on geographical regions. The study spams from 1979 to 2023, with daily values averaged as 5-day mean and SIC are from passive microwave satellite observations. The four optimal clusters can change over time and are identified as: open ocean, permanent sea-ice, partial winter freezing and full winter freezing. The authors show the long term changes of the 4 types of seasonal cycle of SIC. They also introduce transitions from one behavior to another, which are either stabilization (typically any ice regime to open ocean) and destabilization (typically permanent ice to any winter freezing regime).

The paper is well structured and clear, which makes it pleasing to read. The context and method are thoughtfully described. The results are clearly explained, properly analyzed. Especially the section 3.4.2. which shows a very interesting analysis.

In my opinion, this is a great paper which could be improved by discussing the limitations of this study in section 4. Some additions can be added to the text for clarification.

Thank you very much. We have added some text in several sections for clarification, in particular for discussing the limitations of our study.

In the following pages, I address several points that requires the authors' attention and I hope they will help improving the present manuscript:

The SIC product is presented, yet no limitations nor assumptions made to obtain SIC are presented. Is there any reason to pick this product compared to another SIC product? I believe 1-2 sentences of this topic would bring perspective to the text and remind the reader that this dataset differs from reality. Either in section 2.1 or section 4. Is the product consistent over the 40 years? What is the uncertainty of the measure?

Thank you. To make the most of our data driven model, we need the largest amount of data. To the best of our knowledge, there are two satellite data products having daily SIC starting in 1979: NSIDC and OSI SAF (EUMETSAT). We used NSIDC as it is commonly used for climate studies while OSI SAF is commonly used for operational studies.

We now say in section 2.1:" Measurement uncertainties are highest at low SIC, where satellite signals are often influenced more by atmospheric and surface conditions—such as clouds, water vapor, melt on the ice surface, and changes in the character of the snow and ice surface—than by the actual presence of ice. "

At several occasions (l.216, l.378, l.422, l.600, l.621, l.658), the authors write 'sea-ice seasonal cycle'. In my understanding, the seasonal cycle can refer to different variables such as concentration, thickness, albedo, etc. For sake of clarity, I suggest either:
- add a sentence stating that throughout the manuscript, sea-ice will always relate to concentration,
- add 'concentration' to each instance of term 'seasonal cycle'.

Thanks. We now say: "In this paper, we determine Arctic regions based on statistically different sea-ice concentration seasonal cycles, and describe Arctic changes through the time evolving borders. We identify for the first time spatio-temporal regions of the Arctic based on the variability of the seasonal cycle of Arctic sea-ice concentration"

And in the methodology section 2.1, we now say: "Throughout the manuscript, sea-ice will always relate to concentration."

The authors uses the clustering analysis to identify sea-ice precursors for one given point (3.1, L. 325-328). While I find this analysis interesting, I believe using the term 'predictor' is misleading. To my understanding, there is no prediction in this manuscript; the dataset is entirely based on past data. Therefore, the clustering outputs do not predict the behavior of sea-ice, but indicate general behavior for a given grid cell. I do agree that the clustering relies heavily on the start of melting and freezing periods. Using the term 'indicator' would remove this potential confusing.

Thank you very much. We agree. We have changed the term predictor to indicator. Also we substantially pushed further the analysis.

We now say:

"... Therefore, it seems that, for ice-free conditions in summer, the first date of freezing is a good predictor for the appearance of full ice conditions in the next winter.

However, this suggestion relies solely on the shape of the four types of seasonal cycles but to properly quantify this, the spread must be taken into account. Figure S2 displays the spread of the seasonal cycle by plotting the quantiles 0.1, 0.5 and 0.9 of each cluster. To verify our hypothesis on sea-ice predictors, we account for the spread of the date of retreat and date of advance for each cluster. To do so, we

calculate the first date of retreat (the first date after the maximum SIC that is below 0.9) for each seasonal cycle experiencing fully ice covered conditions (having at least one value above 0.99 during the year). We also calculate the first date of advance (the first date after the minimum SIC that is above 0.1) for each seasonal cycle experiencing ice-free conditions (having at least one value below 0.01 during the year). For these calculations, seasonal cycles have been temporally filtered using a 15 days sliding window in order to get rid of short-term dynamical ice events, as done in Lebrun et al., (2019). To circumvent the effect of the discontinuity between 31 December and 1 January, we define the origin of time in May for the calculation of the date of advance. We then label each first date of retreat and first date of advance for each seasonal cycle with its corresponding cluster according to our clustering analysis (Figure 4a).

The normalized probability over each cluster of the first date of retreat and first date of advance at each date is shown Figure 5. This figure also displays the total number of the first date of retreat and the first date of advance of all clusters for each date. If the first date of retreat occurs between January and April, there is around 95% of chance to belong to either the open-ocean cluster, the partial winter-freezing cluster or full winter freezing cluster, which all present ice-free duration in the following summer. However, this situation did not often occur, as the total first date of retreat happening in this period is unlikely (solely 5% of first date of retreat for all clusters). The first date of retreat is more likely to occur between the beginning of April and August, as within this period around 90% of the total date of retreat for all clusters exist. A first date of retreats in early June has solely around 10% of chance to belong to the permanent sea-ice cluster which do not present ice-free conditions in summer while a first date of retreat in early July has around 70% of chance to belong to the full winter-freezing cluster which shows ice-free conditions in summer.

The first date of advance is more likely to occur between the beginning of August until the beginning of January, as within this period around 90% of the total date of advance for all clusters exist. A first date of advance in early September has around 95% of chance to belong to the full winter freezing cluster which present fully ice covered condition in the following winter, while a first date of advance in early

November has around 80% of chance to belong to the partial winter freezing or open ocean clusters which do not show fully ice covered conditions in the following winter. Therefore, this simple model suggests that the first date of retreat could be a good predictor for ice-free conditions the following summer and the first date of advance a good predictor for fully ice cover conditions the following winter. “

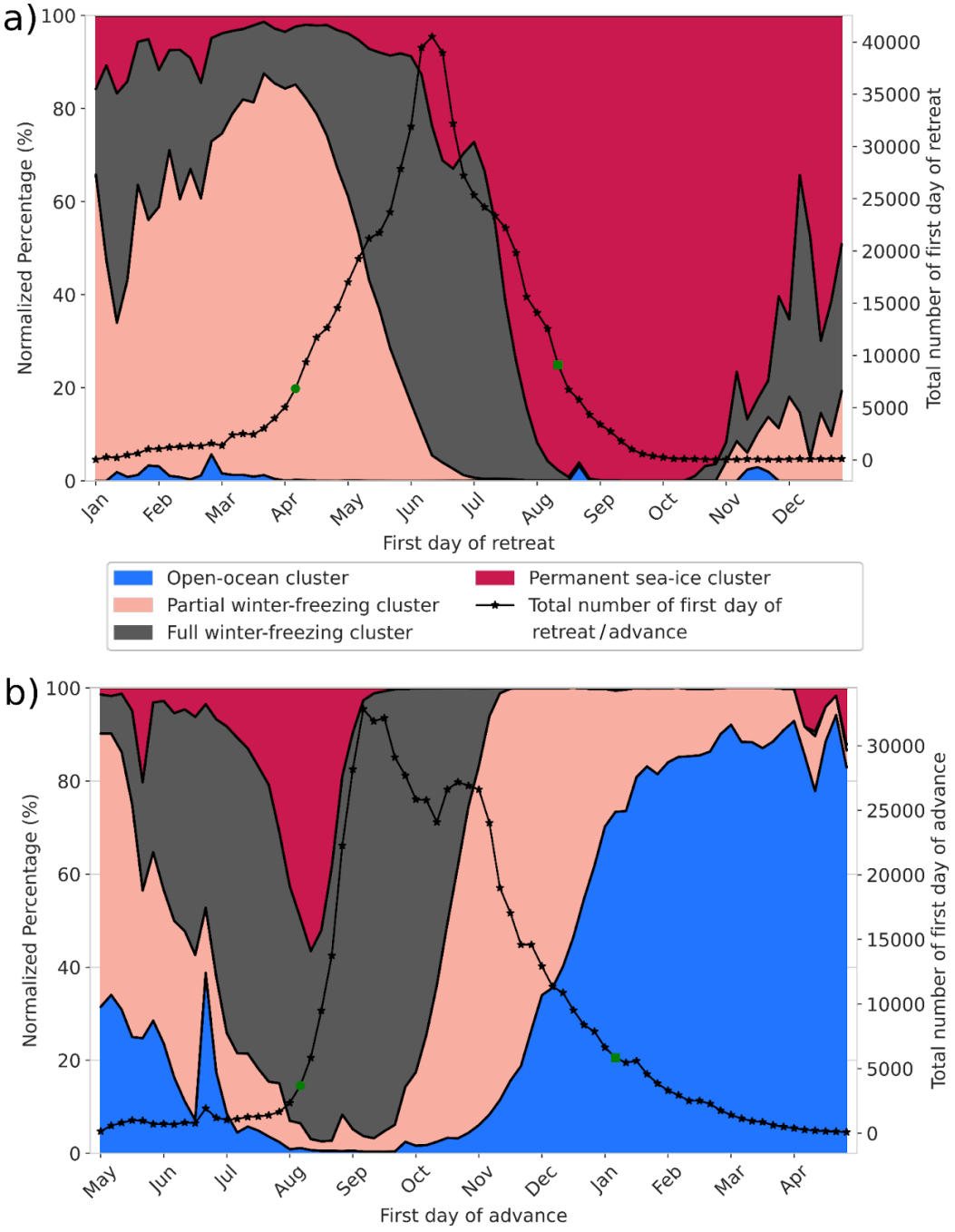

Figure 5: Normalized probability of the first date of retreat (panel a) and first date of advance (panel b) for each cluster. The solid lines with star markers are the total number of first dates of retreat and first dates of advance for each date. The

green circle markers (start date) and green square markers (end date) cover the shortest period where around 90% of the first date of retreat, respectively the first date of advance, for all clusters occurs."

Figure 10 appears before Figure 9 in the text. Figure 9 is actually not cited in the text directly. I would
recommend swapping order of Figure 10 and 9, and adding a citation of former Figure 9 in the paragraph between l. 554-562, where the "star" and "triangle" examples are described.

We have reorganized the figure number and now say:"Figure 10 illustrates how we define the stabilization and destabilization labels."

Overall, the section "conclusion and discussion" present the final results and compare them with the literature. This comparison is satisfactory, however it lacks discussion on the limitation of this study regarding the data and the method.

Thank you very much. We have added a new paragraph in the discussion . We now say: "A limitation of the study is the fact that the method accounts solely for the area between the centroid and the seasonal cycles to define the clusters, meaning that there is no constraint to have the same maximum and minimum to belong to one cluster. However, if the shift of minimum or maximum is large, the area will largely increase which prevents having a large discrepancy between the maximum and minimum of the seasonal cycles and their respective centroids. Another limitation of this study is that sea-ice dynamics are analysed using SIC 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. "

Need for clarifications:
On l.128, the authors state that 'usual descriptors… do not account/consider for the full seasonal cycle' of sea ice. I find this statement unclear (also in the abstract., l.40). Are the cited studies only considering part of seasonal cycle (by choice, lake of data, lack of mean) ? Are they using SIC as well or another variable which could be partially unavailable (such as thickness during melting period) ? Why SIE or type of sea-ice can not consider the full sea-ice seasonal cycle (also L.131)?

These studies do not directly consider the full sea-ice concentration seasonal cycle, in comparison to our study that directly uses the shape of the seasonal cycle in the calculation to analyze Arctic changes. However, these other methods highlighted changes in the shape of the sea-ice seasonal cycle, in an indirect way.

We say: "These three ways of describing the variations in Arctic SIC (trend of SIE, sea-ice age, 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, being maximum in late summer (Fox-Kemper et al., 2021 in IPCC, their Figure 9.13; Meier and Stroeve, 2022), (ii) Arctic sea ice has shifted to younger ice between 1979 and 2018 (IPCC, 2019) and (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). Here, in this paper, we describe the evolution of the Arctic by delimiting spatio-temporal regions having a common type of seasonal cycle. “

L. 116: ‘do not consider changes in sea-ice features’. “Features” is used several times throughout the manuscript (l. 637), and I think concrete examples of features should be given by the authors at the first reference. By the end of the manuscript, I understand that such ‘feature’ means the duration of ice season, or duration of open ocean. At my first reading, it could also have been the sea ice thickness distribution or other properties of sea ice which are not tackled here.

We now say:”The major drawback of our approach resides in the exact grid point quantification of the real seasonal cycle features (such as ice-free duration)”.

L. 317: When writing the dates of melting and freeze up, the authors mention first the permanent, followed by full winter-freezing and partial winter-freezing. However, throughout the paragraph, the seasonal cycles are presented in a different order: open-ocean, partial winter-freezing, full winter-freezing, and permanent ice clusters.
For consistency, I recommend keeping the same order as initially mentioned.

In the new version, we have removed this paragraph. We now say:”Considering a given location fully ice-covered in a given winter, our clustering results suggest that when the sea ice starts to melt in April, the seasonal cycle belongs to the full winter-freezing cluster and be ice-free the next summer. In contrast, when the melting starts one month later (in May) the seasonal cycle belongs to the permanent sea-ice cluster and the considered location will not be ice-free in summer. Besides, the freezing date for areas free of ice could differentiate between the partial winter-freezing and full winter freezing clusters and subsequently predict full ice conditions in the following winter. In our clustering, a freezing starting in October totally freezes in winter which is not the case if the freezing starts in November, having a maximum of about 70% SIC in March. Therefore, it seems that, for ice-free conditions in summer, the first date of advance is a good indicator for full ice conditions in the next winter. “

L. 635: “The major limit of our approach”: I find that this major limitation is explained relatively shortly. In addition, I do not really understand this sentence. Some rephrasing or additional explanation are necessary. Do you mean to say that a lot of

different seasonal cycle of SIC (grid cells) are reduced to one single seasonal cycle through clustering?

Yes exactly. We now employ “drawback” instead of “limit” and wrote one specific paragraph for the limitation of our study.

We now say:”A limitation of the study is the fact that the method accounts solely for the area between the centroid and the seasonal cycles to define the clusters, meaning that there is no constraint to have the same maximum and minimum to belong to one cluster. However, if the shift of minimum or maximum is large, the area will largely increase which prevents having a large discrepancy between the maximum and minimum of the seasonal cycles and their respective centroids. Another limitation of this study is that sea-ice dynamics are analysed using SIC 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. “

Specific comments:
L. 136: citation from Lebrun et al. (2019) is missing.
Done.
L. 173: “for the first time”. Is this really the first time? This is quite surprising! The authors did a great job in showing the potential of such a method.

We do not say that it is the first time that the method is used.

We say:”In this paper, we identify for the first time spatio-temporal regions of the Arctic based on the natural variability of the seasonal cycle of Arctic sea-ice.”

Few sentences are very long: I could suggest rephrasing them: L. 177, l. 301.

We have shortened L177 and now say:”In section 3, we first analyze the clustering outputs of the Arctic sea-ice seasonal cycle (3.1), then examine the probability to belong to each cluster (3.2), and finally investigate the regime stability and transition (3.3).”

For L 301, we say:“They exhibit the expected physical behavior that, due to the thermal inertia of the ice and indirect processes involving the ocean and atmosphere, the maximum sea-ice coverage (in March) follows the minimum solar insolation by a lag of around 3 months, and the minimum sea-ice coverage (in September) occurs around 3 months after the maximum solar insolation (Parkinson et al. 1987). “

We agree that the sentence is long but we think it is clear enough.

L. 234: ‘influencing’. As influence could be positive, I had to keep reading that this influence was undesired, regardless of it being beneficial or not. Maybe use ‘biasing’.

We choose impacting as different realization gives different results. We now say: ”The initialization of centroids coordinates using k-means++ concept (the first

centroid is chosen randomly, the second is the farthest-away, the third the farthest-away of the first and second, and so on) has been tested and is partly impacting our results."

L. 238-239: Quantiles are given both with % (33%, 66%) and as float (0.25, 0.50). Please, pick one way and stick with it. I would advise for %, as it is used again later in the manuscript.

Thank you. We now use the "float" way everywhere in the manuscript.

L. 248: At the end of "Mahalanobis distance to constrain the clustering with physical information.", I was expecting to find the definition of Mahalanobis distance which is L.255. Please, consider putting the definition as soon as possible.

Thank you. We now say straight forward that it uses the correlation matrix:" we choose to use the Mahalanobis distance (using the correlation matrix) to constrain the clustering with physical information."

L. 249: Parenthesis is not closed. "(as shown in Figure 2 by…"

Thank you. Done.

L. 301: "They exhibit the expected physical behavior that, …maximum solar insolation (Parkinson et al. 1987)." Although I see what the authors want to express, this sentence appears difficult to read and cumbersome. Please, rephrase and make this sentence lighter.

We say:"They exhibit the expected physical behavior that, due to the thermal inertia of the ice and indirect processes involving the ocean and atmosphere, the maximum sea-ice coverage (in March) follows the minimum solar insolation by a lag of around 3 months, and the minimum sea-ice coverage (in September) occurs around 3 months after the maximum solar insolation (Parkinson et al. 1987). "

We agree that the sentence is long but we think it is clear enough.

L. 423: "(from no ice to 70% SIC for the partial winter freezing clusters and to 100% SIC for the full winter freezing cluster)". I find this information relevant and I think it would fit better in an addition sentence than in parenthesis.

We think parenthesis are a good choice to keep the sentence concise.

L. 476: "and to a smaller extent, of the full winter-freezing cluster." What about the decrease in partial winter freezing? It could also be compensated by a gain in full winter-freezing?

Thank you. 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 the partial winter-freezing cluster (0.5 % per decade). ”

Figure 8: There are common markers between the subplots. While the open-ocean category can share the same marker and color, the square and diamond markers indicate different categories in both subplots. I would recommend changing the markers in subplot b) to eliminate any possible confusion (especially if printed in black and white). Additionally, adding a vertical line at the year 2000 can enhance the graphical readability.

We think we can easily refer to the legend for the markers of each subplot to not be confused. Also we have vertical lines every 5 years, which leads to a vertical line for the year 1999 and 2003. We think adding a line for 2000 will overload the graphic.

L. 498: “) The area of the MIZ” > “t”

Done.

L. 508: It would be interesting to add one sentence about the supposed reason why the sea-ice loss signature is only visible in the permanent sea-ice cluster. I expected to find this in the discussion section but did not see it.

We now use this paragraph to compare our clustering to the MIZ categorization. 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. “

We think further interpretation on why the signature is only seen in the permanent sea-ice cluster is out of the scope of this work.

L. 547: “As shown Figure 10” > “in” missing

We now say :”As shown in Figure…”

L. 612: “the area between the Central Arctic and the open-ocean does not” > I suggest adding “permanent ice” for clarity. “ the area between the permanent ice Central Arctic and the open-ocean does not”

We have removed this sentence in the new version.

L.652: “The year of loss in the likelihood to belong to the permanent sea-ice shows” is heavy to read for my tastes

We now say:”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, especially in coastal regions of the Pacific side of the Arctic, leading to a shortening of the seasonal cycle. “