Responses to the referees

In this document, we reply to all the review comments from Referee 1 and Referee 2. For clarity, our responses are in blue font and the review comments are in black.

Comments by referee 1

Authors examined the relationship between atmospheric large-scale circulation indices, twelve SOM patterns, and ice drift speed at three fixed transects. The results are novel. The manuscript was well written. I recommend minor revisions.

1. I suggest adding a map describing the Beaufort Gyre (BG), Transpolar Drift Stream (TDS) and Fram Strait (FR) and their transects. Because not all readers know those circulations.

We added the following map to the manuscript.

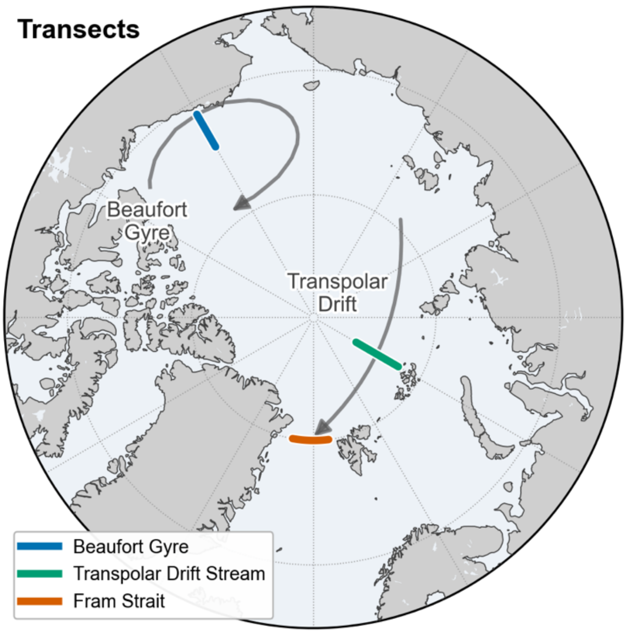

Figure 1: Locations of the transects and schematic representation of the Beaufort Gyre and Transpolar Drift Stream.

2. Please tell readers why authors choose 12 nodes ?

We selected 12 nodes based on previous experience (Nygård et al. 2021). They compared 2x3, 3x4, 4x5 SOM arrays qualitatively and quantitatively. They calculated variances of MSLP within each circulation type. The mean variance within a circulation type is notably reduced in the 3 × 4 SOM array (123 hPa^2) compared to the 2 × 3 SOM array (144 hPa^2), but the mean variance does not decrease at the same rate towards the 4 × 5 SOM array (120 hPa^2). As Nygård et al. (2021) summarize: “The size of the SOM array was subjectively selected to 3 × 4 to include the main features of regional circulation patterns and yet be sufficiently general to enable conceptualization of the results.”

3. Please plot the number of days for each node in winter and examine their trends. Based on the trend and sea ice motion through the transects of BG, TDS, and FR for each node, authors can calculated the contributions of 12 nodes to sea ice speed through the transects in winter. Please refer to Lee and Feldstein (2013).

We appreciate the idea, but in our case, there are no significant trends in occurrence of the SOM nodes, which is stated in Section 3.1 lines 157-158. The barplot below visualizes the occurrence of the nodes. The trends and statistics are show in the table below. The largest linear trends are about 0.2 days/year, but due to the large interannual variability none of the trends are significant at the 5% significance level ($p < 0.05$). However, the increasing occurrence on node 2 and node 12 is almost significant.

Futher, we are not able to link the winter average ice drift speed to the node occurrence. Our intrepretation is, that individual nodes lead to the anomalous drift speed in the time scale to days or weeks, but not in seasonal timescales.

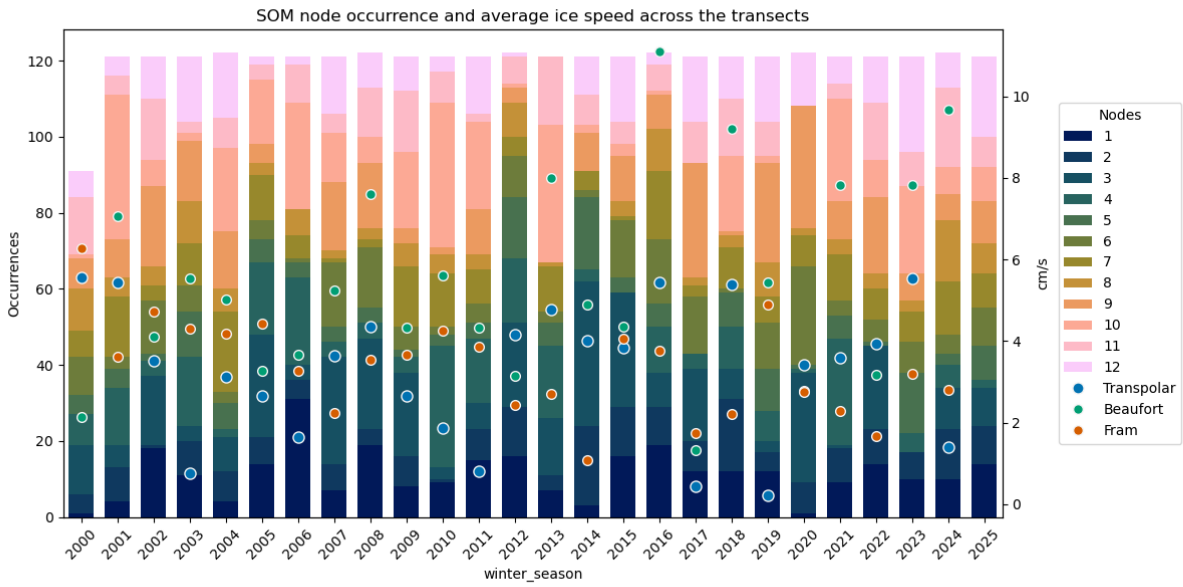

Figure 2: Node occurrence, i.e. numer of days belonging to each node, in winters (December-March) 2000-2025. The data for winter 2000 starts in Janyary, which explaing the lower number of occurrence. Coloured dots show the absolute value of winter average drift speed across the transect (scale on the right hand side of the plot).

Table 1: Slope denotes the linear trend in occurrence frequency (occurrences/year); positive values indicate increasing occurrence and negative values indicate decreasing occurrence over 2000-2025. P-value tests the null hypothesis of no trend, with $p < 0.05$considered statistically significant. R^2 represents the proportion of variance in occurrence explained by the linear trend, with higher values indicating a stronger linear relationship.

Node	Slope	p-value	R^2
1	0.018462	0.917118	0.000461
2	0.221197	0.064934	0.134886
3	-0.085470	0.763348	0.003849
4	-0.262564	0.272186	0.049997
5	0.076923	0.560243	0.014330
6	0.108034	0.525539	0.016999
7	-0.036239	0.798916	0.002757
8	-0.002735	0.978572	0.000031

9	0.154188	0.512645	0.018071
10	-0.292308	0.384511	0.031659
11	0.061880	0.667886	0.007801
12	0.301197	0.080942	0.121512

Lee, S., and S. B. Feldstein, 2013: Detecting ozone- and greenhouse gas–driven wind trends with observational data. Science, 339, 563–567, https://doi.org/10.1126/science.1225154.

Comments by referee 2

This manuscript explores the relationship between ice motion and large circulation patterns vs regional MSLP patterns with the aim to determine which has greater influence. The findings show that the regional MSLP patterns defined through SOM analysis, accounted for more of the ice motion than the large circulation modes such as AO, NAO, PNA and DA. I found the concept of this paper interesting particularly in the finding showing that using regional MSLP patterns were more informative in ice flow studies compared to using large scale circulation indices. However, I found the message of this paper was lost. I was confused at numerous points in the paper about the inclusion of some points that I didn't think were necessary nor added to the story and felt that the manuscript was disjointed. In particularly I feel that the main and most interesting results were not really highlighted clearly enough and that time should be taken to clean up how you describe and talk about the climate indices and regional MSLP patterns as I think that multiple terms were used for these throughout. I think these points and the ones below need to be addressed before this paper can be accepted for publication.

Response:

We are grateful for the constructive comments and thorough evaluation from the reviewer. These comments pointed out that your terminology was not always systematic, the motivation not as clear as we wished, and the conclusions not clearly formulated. We are happy to address these questions to improve the manuscript. Here, we address the comment point by point.

Wider points to be addressed:

- There are a couple of incorrect statements made I believe. Between lines 124-125 you state that Deser et al. (2000) applied rotated PA to 700 mb geopotential heights, however having checked this paper, I see no mention of this. They did apply EOF analysis to sea ice and then did a regression analysis to circulation patterns, but nowhere was 700 mb geopotential height ever mentioned. Furthermore upon checking the NOAA CPC where you got the NAO and PNA indices from, they actually apply the RPCA technique to monthly standardised 500mb heights between 20N-90N which is different to what is in this manuscript.

    - Here, we have indeed used the wrong reference. We should be referring to Barnston and Livezey (1987), who applied the rotated principal component analysis to to the 700 mb geopotential heights.
    - Due to technical reasons citations of Barnston and Livezey (1987) had been replaced by Deser et al. (2000) in several places. We have corrected this.
    - The NAO and PNA indices are indeed calculated based on the 500mb height, as pointed out by the reviewer. We have corrected this in the text.
- The paper I find confusing at times as you move between talking about the large scale circulation patterns and MSLP patterns which I think has to do with how they are referred to, for example you mention large-scale circulation indices, then finer scale circulation types, which are also referred to as regional MSLP, Arctic atmosphere circulation types. And throughout you mention pressure patterns which further adds to confusion as the indices are related to pressure patterns. I would choose a nomenclature for each process and stick to it throughout
    - This is a very good suggestion and we appreciate the objective comment on the terminology. We suggest the following nomenclature:
        - **Large-scale circulation indices** for NAO, AO, PNA and DA
        - **Weather types** or SOM nodes for the pressure pattern identified by the SOM-clustering
- I find the discussion and conclusions section confusing. It does not, in my view, follow a logical structure and feels a bit disjointed. I also feel that it does not comprehensively lay out the results and contextualise what they mean regarding using climate indices vs regional MSLP patterns to investigate ice flow.
    - We have clarified the text in the Discussion and Conclusion -section. We avoided adding new content, but reordered and reformulated the text to make it more clear.
- Proper contextualisation of where you research fills a gap is needed earlier. You place this almost at the end of the introduction when this would be much more important to state when going between the large-scale circulation indices to what you did (between lines 48-49).
    - The linkages between the large scale circulation (AO, DA, NAO etc) and Arctic is drift and ice export have been addressed in numerous studies. However, we want to show the clustering methods, like SOM, could give more insight in the driving mechanism of the ice motion.
- I would rethink some of the wording in the paper because I feel that you overstate that pressure gradients results in geostrophic wind that leads to ice motion as though this is a new concept, when in fact this is incredibly well known.
    - Thank you for mentionning this. We did not intend to give the impression that the connection between the geostrophic wind and ice motion would be a new finding. We rather wish that this fact would help in interpreting the results and in explaining why the large scale circulation indices are not always linked to the ice drift speed in the study locations. Also, the role of the geostophic wind in addressed already in the introduction (eg. line 37-38: “As a rule of thumb, the geostrophic wind explains approximately 50% of the ice drift speed variability (Colony and Thorndike, 1984).”).
- As you only have ice motion data from 2000-2023 and MSLP data from 2000-2025 would it not make sense to just use the same time period for both datasets especially considering there is only two years difference. I know that you use the same time frame for the regression analysis but I would argue it would be better to stick to the same time frame throughout.

- We considered this during the work. However, we wanted to use the most full dataset for the SOM analysis. This allows to have a more complete analysis in the possible trends in occurrence of weather types.

Detailed points:

L14: What do you mean by explanatory power?

Explanatory power refers to the squared correlation of the linear regression model (Figure 6). We changed "explanatory power" to "squared correlation R^2 ".

L26: Changes in wind? What changes do you mean here, is this climate change related or just wind variability?

We decided that the text should be more winter specific and changed the text accordingly. Olason and Notz (2014) concluded that in winter, the increased drift speed is mainly due to thinner ice rather than increased wind speed.

L28: In what way where the ice deformations 'more important' than expected, not sure why it is relevant to note this in general.

Here, we wanted to point out that dynamic ice deformations have been observed in the entire ice pack. Indeed, we don't focus on defromations in this study, but deformations are linked to thinning of the ice, as is the increased drift speed.

L34: 'Ice motion is essential for redistributing and exporting ice…' I find this an odd phrasing because ice motion actually does redistribute and exports ice, the word essential here is not needed.

True. We have reformulated the text like this: "Ice motion redistributes ice within the Arctic and exports ice from the Arctic." (Lines 32-33)

L49: " the Arctic atmosphere circulation types" are these universal circulation types or what does this actually mean?

As suggested above, we decided to use the term "weather types", when referring to the SOM-nodes.

L77-78: "The results highlight the locations of …." Not sure why you have included a result statement in your material and methods section.

We have removed this statement.

L81-82: 'and show the spatial correlation between ice motion and atmospheric pressure' this can lead to confusion because you use ice motion data from polar pathfinder but this line makes it appear that both were from ERA5. I would remove it completely as you calculate the correlation across two different datasets.

Good point. We now specify, that the ice motion is from the Polar Pathfinder dataset.

L82: “Retrieved daily means of 1 hourly data” was this just MSLP?

Yes, this applies for the ERA5 MSLP data. We didn’t use other ERA5 variables in the final manuscript.

L100: Where in the mid-latitudes are the centres of action for the AO?

The AO has one center of action in the central Arctic and one a zonally symmetric ring-like pattern in the mid-latitudes. We added the word “ring-like” in our description. The zonal symmetricity means, that the pressure pattern is uniform around the latitude circle, here around the mid-latitudes.

L102: You mention the stratospheric connection of the AO but this is not talked about again anywhere in the paper so I am not sure if it is required to mention this in the paper.

True. This is not strictly necessary for the paper, but we kept it in the text to highligt that the near-surface and stratospheric phenomena are connected. If the reviewers/editors wish, we can remove this.

L106: Do you mean you used the AO index from NOAA CPC?

Yes, we used AO, NAO and PNA from NOAA CPC.

L125: Do you mean you used the NAO and PNA indices from NOAA CPC, and what do you mean daily values were calculated, daily values of what?

We used the dailys values of AO, NAO and PNA from NOAA CPC. We removed the “Daily values were calculated following Barnston and Livezey (1987)”, to make it clear that we did not do these calculations.

L147: Figure X? Which figure are you referencing?

We added a new Figure 1 to show the locations of the transects (suggested also by Reviewer 1).

L162: What do you mean that the circulation types show increasing cyclonic activity? As this is a SOM not a temporal analysis of the circulation I am not clear what you mean by increasing?

Here, “increasing” was not the best choice of word. We meant that the SOM-nodes in the upper right corner have typically lower MSLP in the Atlantic region than the other nodes. The lower MSLP is a signature of cyclones. We removed “increased cyclonic activity” and wrote “lower MSLP” instead.

L167: I am unclear what you mean in this following sentence “The features of the pressure fields are reflected in the average circulation index on each node”

We replaced this with a simpler statement: “The weather types are linked to the average large-scale circulation indices. ”

L168: Again you referencing decreasing MSLP and I am not sure what you mean by this.

Here, we are trying to describle that the weather types with high NAO tend to have low MSLP in the North Atlantic, as expected. We have reformulated to text to make it more clear.

L170: “Circulation types towards the right..” Do you mean the last/right column of figure 2?

Yes. We specified that we mean the right column, and the two lowest rows of the central column.

L187-188: “We observed that the ice drift speed across the transect can be over three times faster when the circulation type is favourable” is this for all three regions, do you have actual data to show this and what is meant by favourable for each of the three transects?

We modified the text to be more specific: “We observed that the ice drift speed across the transect can be over three times faster when the weather type has a low pressure on the left-hand side of the ice flow, or a high pressure on the right-hand side”

L223-226: This whole section currently states the ice flows faster when there is a strong pressure gradient, but this is a very obvious statement but in its current format reads as though this a surprising or new finding. I would suggest rewording.

Also left-hand side and right-hand side of what? This is used multiple times throughout the manuscript and you need to be a lot clearer about what you mean

The line numbering has shifted a bit, but we believe that this comment refers to the section that starts with “The heatmaps in Figure 5 show average drift speeds[…]”, line 220-225 currently. We think that this is a relevant result that quantifies the drift speeds and MSLP gradients across the transects. However, we modified the text to avoid the impression of presenting basic knowledge as new findings.

Figure 5: How do these drift speeds compare to drift speeds under observed motion, in other words how significant is this ice motion in regards the real world, do these constitute fast ice motion speeds? Also are any of these significant?

The drift speed value in Figure 6 (previously 5) are Polar Pathfinder ice motion values selected at the transect location and averaged for each weather type (or Node). In that sense, these values are real world values. Spreen et al., (2020) report ice drift speed of about 9.7 km/day based on satellite data, and 19.2 km/day based on Acustic Doppler Current Profiler (ADCP) (observation period September 2012- June 2014) in the Fram Strait. We consider that our results are well in line with this and the fast nodes are significantly faster that the average drift.

L232-240: This section seemingly compares the figure 4a ice drift speeds to the circulations patterns in the modes in figures 1 and 2, however it is never really clear which figure (1 or 2) that I should be comparing to when you discuss the pressure patterns, please point to which specific figure you are referring to. Furthermore, I don’t see any reference to figure 5b of the MSLP gradients (in particular their actual values) and what is most interesting here is that the largest gradient in TDS doesn’t actually correlate to the largest ice flow and yet this is not really mentioned. I would either make more reference to this figure or remove.

In this section, we referred to Figures 2 and 3 (previously 1 and 2), because Figure 2 shows the actual MSLP, and Figure 3 the MSLP anomaly. We now spesify this in the text. We added also references to Figure 6b (previously 5b), as suggested.

At TDS, the large MSLP gradient doesn't clearly correlate with the drift speed. We believe that this is related to fixed location of the TDS-transect that we use in this study. As TDS in not confined by any geographic barrier, it's locations and intensity are free to evolve depending on the atmospheric (and oceanic) forcing. A better methodology would track to location and intensity of TDS explicitely.

We discuss this later in the manuscript, eg. lines 294-300: "Unlike the other two transects, the TDS is not confined by any geographic barrier, thus defining a fixed location for the TDS transect is arbitrary […].

L235-237: I was expecting more discussion about the relevance of noting the different average index values for the large-scale circulation indices either here or in the discussion, as it is interesting to note their different phases and yet fast ice motion.

We hope the other modifications in the Discussion and conclusion now address better this point.

L286-289: This section is confusing. You state that a pressure dipole was not necessary to drive fast flow, but what is your definition of a pressure dipole? Or are you saying that either a high or low pressure anomaly can drive fast ice flow? However, line 289 says that the pressure gradient drives the geostrophic wind which almost negates the previous point and a high/low pressure coupling is important (which is my interpretation of a pressure dipole).

By pressure dipole we meant a situation where a low pressure anomaly on the left hand side **and** a high pressure anomaly on the right hand side of the drift occur at the same time. We added this information to the text, lines 287-288. However, one center of action, ie. either a low **or** a high pressure, is sufficient to create the necessary pressure gradient. We hope that the text is more clear now.

L297-298: Is this line on the positive feedback loop important to note if so can you elucidate more about this statement.

Here, we refer to the positive feedback loop described in the previous sentence: "Climate change has reduced the winter sea ice cover in the Barents Sea, creating a pathway for increased cyclonicity in the region, that favours sea ice export from the Arctic." However, the reference to Smedsrud is of lesser importance, thus we desided to remove it.

L299-305: This feels more like an opening paragraph for this section especially considering you have already been discussing the nodes from the SOM.

We modified the text slightly.

L310: You claim that you defined the TDS as a drift pattern that brings ice towards the Fram Strait, however I am sure that this has been well defined before. Can you explain more what you mean here?

In general, the TDS is defined as a major Arctic Ocean surface circulation pathway that transports sea ice and freshwater from the Siberian shelves across the Arctic Basin toward Fram Strait and the North Atlantic. However, in reality the location and direction of the TDS vary. We explain this more in detail now in lines 310-314.

L312: You state at times the TDS was reversed and ice motion was intensive, what do you mean by intensive, have you a numeric value for this?

Here, we refer to cases where the ice flow at the TDS location had an important meridional, or poleward, component, as in nodes 1, 2 and 3. Figure 4 shows that the ice velocity exceded 8km/d in in the transpolar drift region, but the direction of the flow was not normal to our transect. This also points out a limitation of using fixed transect to study the TDS, as the drift's location and direction change according to the atmospheric forcing.

Spreen, G., de Steur, L., Divine, D., Gerland, S., Hansen, E., Kwok, R., 2020. Arctic Sea Ice Volume Export Through Fram Strait From 1992 to 2014. Journal of Geophysical Research: Oceans 125, e2019JC016039. https://doi.org/10.1029/2019JC016039