WCD-2021-18

Identification, characteristics, and dynamics of Arctic extreme seasons

Response to the Reviewers' comments by Katharina Hartmuth, Maxi Boettcher, Heini Wernli, and Lukas Papritz

We thank both reviewers for their additional, helpful comments. We address each comment point by point below. The reviewers' comments are given in blue and our responses in black.

Please note, that we always refer to the lines in the **updated, revised manuscript** (document without track changes). Figures in the reply document are referred to as "Fig. R1", etc., and figures in the revised manuscript are referred to as "Fig. 1", etc. We supplement this document with a latexdiff-pdf showing changes from the first to the second revised manuscript.

Reviewer 1

General comments:

The authors evaluate the atmospheric conditions during anomalously extreme seasons in the Arctic. This is performed using a regional principal component (PC) analysis (PCA) from ERA5 data of the first two PCs of all seasons from 1979-2018. The PCA uses six key surface variables and divided spatially into 9 Arctic sub-regions subjectively chosen based on climatological sea ice conditions in either the Nordic Seas, Kara and Barents Seas, and the rest of the Arctic. Results identify 2-3 extreme seasons for each season and in each sub-region. The PCA applied here provides a quantification of how anomalous a season is relative to another season, which variables contribute most to the extreme conditions of the respective season, and how consistent those conditions are during those particular seasons. The authors then choose two extreme or anomalous seasons in the Kara-Barents sea during winter (DJF) and one extreme DJF season over the "High Arctic" to further investigate the synoptic weather conditions that were occurring. The chosen seasons are picked based on their orthogonal, yet anomalous or extreme, projections onto the PCs, as well as their diverse processes.

This research nicely demonstrates how PCs can be used to identify seasonal anomalies and extremes in certain regions of the Arctic. It furthermore demonstrates how to use that information to provide an expectation of how an extreme season was characterized with regard to of one of the six variables and how consistent those conditions were. It is certainly a notable method to identify extreme seasons that might be worth analyzing in further detail at shorter time and space scales if desired. I thank the authors for considering and addressing my major concerns from the previous version. I note that in the previous version of the manuscript, the sign convention for the surface energy budget was not stated (and I thank the authors for adding that in the latest revision), which has now allowed me to comment on the surface energy budget in this version with the new insight. Overall the arguments are much clearer and this manuscript will make a positive contribution and be published once a few remaining issues are addressed. Specifically:

1) I appreciate that the authors added correlation coefficient values and respective p-values in Tables S1 and S2. The corresponding text still needs some refinement, however. Since they are performing a statistical significance test, they can choose a p-value (say 0.05 or 0.01) to be a threshold as a "statistically significant correlation" a priori. This would clarify some of the statements, as sometimes I am not sure whether they mean that the correlation coefficient value is high (subjective) or whether they mean that the p value is low (objective since the correlation in that case is statistically significant). For example, on line 243, do they mean there is no statistically significant correlation in the summer? That would make most sense to me, given the high p-values. The magnitudes of the coefficients are also relatively low, but it is subjective to make conclusions based off that, alone. Generally, the lower p-values correspond

to the higher coefficients, so it shouldn't change most of the conclusions to refine the wording whenever the word "correlation" is used.

Thank you very much for this comment, this is indeed a good point as we mostly defined correlation rather subjectively via the correlation coefficient. However, we agree that it would be better to use p-values for a more objective assessment. Thus, we now use a p-value threshold of 0.05 to define a correlation as "statistically significant". We changed the wording about correlations throughout the manuscript to clarify our statements.

2) The story for the DJF 2011/12 case (Section 5.1) is very interesting but does not quite seem to make complete sense to me as described here. It is argued that the consistently warm temperatures are because of the repeated passage of cyclones from the Nordic Seas. Yet, the authors point out that overall, cyclone frequency was below climatology, and there were frequent blocking episodes. However, precipitation was one of the relatively lowest seasons over KBM with perhaps a few episodic precipitation events (i.e., Fig. 5h). Of course, blocking episodes favor warmer temperatures, and SLP was higher than average for much of the time when temperatures were above average and when there was blocking (Figure 9a,e, and colormaps). But then how does the consistently lower SIC come into the story? Note that the September 2011 sea ice extent was the second lowest since 1979 up to this point, and much of this was in the Kara and Barents Seas, so there could very well have been a new surface forcing in that region. Indeed it seems at a glance like this may not have an impact in DJF, because of the strongly positive and consistent E∗s (Fig. 5l) and H∗S (Figure 7b). Presumably cloud cover was anomalously low, though this could easily be checked by looking at whether R∗L has the same sign as E∗s and H∗S. Perhaps though, there was preconditioning in the autumn (October - November), which if there was anomalously more open water instead of ice, then there could be anomalously high upward sensible heat fluxes, which lead to warmer temperatures and higher tropospheric thicknesses by the time DJF started leading to forcings that favored patterns for blocking. Can this be ruled out? Figure 1 shows that in the two months before DJF 2011/2012, surface skin temperature, 2-m air temperature, and 850 hPa air temperature anomalies all had similar patterns to DJF in the lower troposphere before the blocking pattern arose, supporting a preconditioning by the surface conditions.

Thank you for these detailed remarks, we appreciate the amount of thought which has been put into this comment. It is indeed true that there has been a slight preconditioning in terms of sea ice during autumn 2011, as the sea ice formed later than usual, leading to a negative sea ice anomaly especially in October (which is evident in the anomaly patterns for skin temperature and 2m temperature, which match the shift in the sea ice edge). This is also evident from Fig. 13 in the manuscript, although it shows that the preconditioning for DJF 2011/12 was much weaker compared to DJF 2016/17. In addition, as the sea ice coverage in the Kara and Barents Seas reached values close to normal at the end of November 2011 (see Fig. R1d), surface preconditioning presumably plays a smaller role in determining the unusualness of this winter.

Regarding the surface fluxes during December 2011, E_S, H_S and R_L show a consistently positive anomaly, which speaks against the argument of low cloud cover leading to the positive E_S anomaly. An analysis of the total cloud cover anomaly during this period shows no significant anomalies during this period.

Addressing the possible preconditioning of the increased blocking frequency in DJF due to the reduced sea ice coverage during October, we can show in Fig. R1c that there have not been consistently anomalous upward sensible heat fluxes in the Kara and Barents Seas in autumn. We think that such an increase in upward H_S would need to occur consistently over a longer period to explain the increased number of blocks during DJF, especially considering the reduction in upward H_S during December. Of course, we cannot completely rule out the role of autumn heat fluxes for winter blocks, but in our opinion this scenario is highly unlikely for DJF 2011/12.

Regarding the initial question "why has DJF 2011/12 been so warm?", it is surely a combination of different synoptic situations and dynamical processes that led to such a strong warm anomaly rather than one specific process. We slightly adapted the text such that this result is emphasized more strongly and further encourage the reader to have a look at the supplementary animation, which shows the relevance of a variety of synoptic processes throughout this winter.

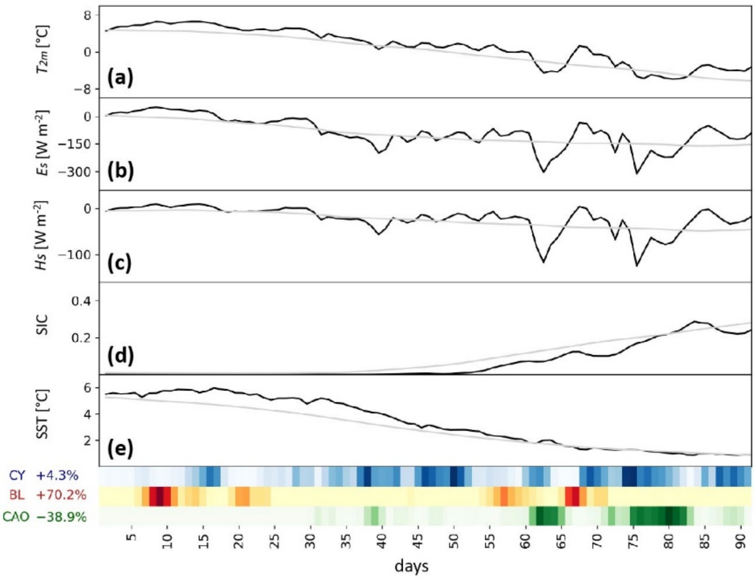

Figure R1: Time series of daily-mean (a) T_{2m} (in °C), (b) E_S (in W m^{-2}), (c) H_S (in W m^{-2}), (d) SIC, and (e) SST (in °C) averaged in the region of the Kara and Barents Seas in SON 2011 (black lines). The transient climatology is shown by grey lines.

3) In the DJF 2016/17 case, is it that an extreme $E*s$ anomaly occurs when there is reduced SIC and a marine CAO? The cyclones reduce the SIC in KBM, and the passage of the cyclone is then followed by a CAO. For example, the extreme negative anomaly in $E*s$ between days 30- 35 (Figure 10b). There was a cyclone around day 25 (Figure 10e, Figure S7) that reduced SIC. After the cyclone passed, there was a strong CAO at nearly the same time as the negative $E*s$ (Compare Figure 10b with Figure 10 CAO heatmap) and negative $T*2m$ (Figure 10a). Right now, the wording of the text in lines 449-462 makes it seem like the CAO is just an additional factor, but it seems like it may be the critical factor in order to lead to the overall magnitude/rank in $E*s$ since lower sea ice alone would not necessarily do so. Also, the cyclone paths may be important such that there are frontal passages that promote CAOs in their wake in the right regions. Furthermore, if this is the story, I might consider choosing one of these marine CAO cases to highlight in Figure 11 instead of the current warm temperature anomaly case, esp. since overall $T*2m$ was not ranked as highly (Table 2). I think this then flows better for the text on lines 481-486 where SIC, $T*2m$, marine CAOs, and $E*s$ are readdressed and furthermore when preconditioning is discussed thereafter on lines 488-506.

Thank you for these very helpful remarks. In DJF 2016/17, the CAOs are indeed a critical factor, leading to strong anomalies in the surface fluxes and thus, strongly influencing this seasons' rank in E_S. As you point out, their occurrence in the wake of cyclones is particularly relevant, as the cyclones often cause a retreat of the sea ice edge (e.g., shown in Fig. S7), favoring strongly increased surface fluxes during the subsequent CAO. The cyclone paths do play a very important role as can be seen when comparing this winter with the winter 2011/12 in the same region, when most of the time the cyclones' cold sector does not reach the region of the Kara and Barents Seas, hampering the formation of strong upward surface fluxes. We adapted the text in section 5.2 to clarify the importance of this link between cyclones and CAOs.

We further adapted Fig. 11, reproduced here as Fig. R2, such that it now shows three different timesteps, including day 62, which features a CAO case in the Kara and Barents Seas. For this timestep we added a fourth panel displaying the daily-mean E_S anomaly, which is strongly negative in the region of the CAO. Thus, this figure does now not only highlight the variety of synoptic processes occurring throughout this season, but further emphasizes the importance of the link between cyclones and CAOs for the unusualness of this winter season.

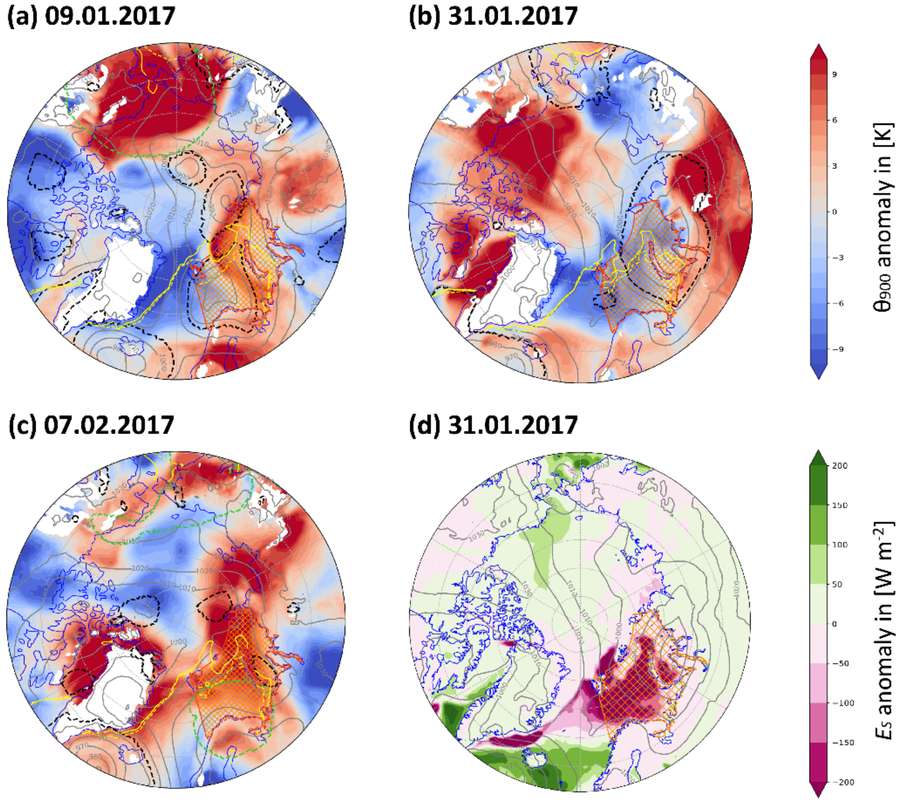

Figure R2: Synoptic situation on (a) 09 January (day 40), (b) 31 January (day 62), and (c) 07 February 2017 (day 69). Daily anomaly of potential temperature at 900 hPa (θ_{900}; K, color). Sea-level pressure (SLP, grey contours, in intervals of 10 hPa), sea ice edge (SIC=0.5, solid yellow line), climatological sea ice edge (SIC_{clim}=0.5, dashed yellow line), cyclone mask (dashed black contour) and blocking mask (dashed green contour) at 00 UTC on the considered days. Daily E_S anomaly (E_S*; W m^{-2}, color) on day 62 is shown in (d). The region of the Kara and Barents Seas is marked by orange hatching.

Other specific comments:

1) Line 19: normal → either “average” or “typical”

Changed “normal” to “average”.

2) Line 88: Recent studies emphasized the importance of polar anticyclones and blocking events in the High Arctic for what? Insert into sentence.

We mention this already in the second part of the sentence. For clarification we rephrased the line slightly such that it now reads:

“Recent studies emphasized the importance of polar anticyclones and blocking events in the High Arctic for driving subsidence-induced adiabatic warming, leading to anomalies in surface temperature and net surface radiation which cause increased sea ice melting.”

3) Lines 90-93: Burt et al. (2016) also discuss this positive warming feedback, and additionally show a possible regional implication.

We added a reference to Burt et al. (2016).

4) Line 145: Since the CAOs here can only be identified over water, insert "marine" in front of "cold air outbreaks."

Line 139 now reads "Further, we define marine cold air outbreaks (CAOs) based on …".

5) Lines 299-300: How exactly can we see from Figures 5 and 6 that the driest years have non-zero precipitation amounts? What does it mean that it is less evident? Is this just a result that is not shown and therefore not evident from the plots?

Thank you for these questions. First, we can see from Figs. 5e-h and 6e-h that there is no season with a continuously negative precipitation anomaly, which would be the case if the marker of that season was located on the outer-left dashed grey line. Thus, for each season, the seasonal-mean absolute anomaly $\overline{|P^*|}$ is always larger than the seasonal-mean anomaly $\overline{P^*}$, indicating that both, positive and negative daily P anomalies (P^*) exist. This would not be the case if precipitation was zero throughout the season, resulting in either no or a negative P^*.

Regarding your second and third question, we think it is remarkable that there are no seasons without at least a few days with above-average precipitation. This is not something we have calculated, but we just simply think that this is not obvious per se. (We also changed "less evident" to "less obvious", which better reflects what we intend to say.)

6) Lines 346-347: In sub-regions ARI and ARM, I do not understand why the positive correlation between R∗L and T∗2m emphasizes the importance of clouds. For example, in the summer, when it is cloudier, it is cooler due to less shortwave radiation reaching the surface, so T∗2m is more negative. Clouds increase the downwelling longwave radiation, thus making R∗L more positive according to the sign convention.

Thanks for pointing this out. As we have not looked in detail at these correlations (and the processes that possibly cause them) and only made an assumption, we removed this statement.

7) Line 369: continuous → consistent

Here, we refer to our definition of a season with a "continuous anomaly" (see line 266ff.). However, as this result changed slightly with our correction of the calculation of seasonal-mean absolute anomalies (see corrigendum to the first revised manuscript), this statement does not appear anymore.

8) Line 371: What does "equally strong" mean when comparing two different variables?

Thank you for the remark, this is indeed not a very objective comparison, as the "equally" only refers to a subjective evaluation of the distance between outlier and other seasons in Fig. 6a and 6i. However, as this is not the case anymore for the sub-regions KBM and KBS, we changed the sentence such that it now reads "In summer, all extreme seasons are characterized by a strong T_{2m} outlier (Fig. 6a,c,d), which co-occur with E_S outliers in ARI and KBM (Fig. 6i,k)."

9) Line 395: Can't we see that the largest T∗2m over the 39-year period from Figure 5b,d instead of Figure 7b,d, where we can only see the principal component values, which are a combination of variables, correct?

Thanks a lot for spotting this typo! We want to refer to Fig. 5b and d here obviously, as the PCA biplots only show a combination of anomalies and no actual seasonal-mean anomalies.

10) Line 401: Insert T_{2m}^{*} in front of “values” to make it less ambiguous exactly what values are being referred to, esp. since T_{2m}^{*} was just referred to beforehand.

We changed the line such that it now reads “In the whole region, during December, T_{2m} values are continuously around +6 K above climatology, …”.

11) Line 407: What is meant by “strongly correlated” exactly? Is there a correlation coefficient being referred to or is this meant to be a visual comparison between panels a and b in Figure 9?

Yes, this is a visual comparison between both panels. We calculated the actual values of the correlation coefficient (0.66) and the p-value (1.16e-12), which indicate that the correlation between daily-mean T_{2m} and E_S is indeed statistically significant.

12) Line 532: How much of the warming was caused by subsidence vs. horizontal advection? The trajectories show descending air parcels, but I cannot see how much warming would be caused by that in this case.

Thank you for this question. For each warm event, we can classify the trajectories based on their thermodynamic development in terms of temperature (T) and potential temperature (θ). If the trajectories experience an overall increase in T but decrease in θ, we define them as subsiding air masses (affected by radiative cooling). If, however, the changes in T and θ have the same sign (negative, e.g., in the case of long-range transport from lower latitudes or positive, e.g., transport of very cold air masses over a relatively warm ocean), we assign them to horizontal transport (for more detailed information about this method see, e.g., Binder et al., 2017 and Papritz, 2020).

For the cases shown in supplementary Fig. S8 the share of subsiding/horizontally transported air parcels that reach the region of a warm anomaly is about 37%/61% in January and 58%/41% in February. Thus, we conclude that horizontal transport dominates the warm event in January, whereas in February, subsiding air contributes more to the warm surface temperature anomaly. However, these numbers do not reflect how much of the anomaly value has been caused by which process, they only reflect the percentage share of the two transport categories.

13) Lines 539-543: Up until this point, preconditioning referred to the state of the sea ice at the beginning of the season in question (i.e., Question 4 on line 117). The discussion here and Figure 13 suggests a different (yearly) time scale, i.e., that the sea ice state from previous winters could be a preconditioning for the current winter. The shorter-term preconditioning may be more important in the 2011/2012 case, given that the September 2011 sea ice extent minimum was the second lowest extent on record up to that point.

Thank you for mentioning this point. We agree that there are most probably different types and/or definitions of preconditioning. As the ERA5 dataset is not suitable to do a statistical analysis, it is one aim of our current research project to use CESM data to statistically analyze and classify different types of preconditioning leading to extreme seasons.

14) Lines 666-667: Is the CESM large ensemble capable of representing the synoptic processes described in this paper?

It is one aim of our current studies to verify, if climate models such as the CESM large ensemble can represent extreme seasons and the processes leading to them sufficiently well.

Technical corrections:

1) Line 372: don’t → do not

Changed as suggested.

2) Line 401: Remove “about”

Changed as suggested.

3) Line 405: we ↔ here

Changed as suggested.

4) Line 488: Remove “also” before “preconditioning”

Changed as suggested.

5) Line 545: Remove “want to”

Changed as suggested.

References

Burt, M. A., D. A. Randall, and M. D. Branson, 2016: Dark warming. J. Climate, 29 (2), 705–719

Reviewer 2

I am very pleased with the work the authors have done revising the paper. I was particularly concerned that not all selected seasons showed a significant anomaly in any variable, however, revised Table 2 reveals that extreme seasons were ranked either the first or second in at least one of the variables (with an exception for MAM 1990 for NOI, however, that season was extreme in ARI).

Interestingly, extreme winter seasons show strong Es anomalies, while T2m is hardly anomalous. On the other hand, in summer the leading factor is T2m, while in transition seasons the dominant variable is Es again with higher uncertainty though. I am not sure how this can be explained - perhaps, increased standard deviation of Es /T2m during colder / warmer seasons, respectively. It would be good to see a comment on this and also on the reversibility of this relationship, i.e., if extreme winter seasons are those ranked high in Es anomalies (and T2m for summer)?

Thanks a lot for this comment, this is indeed a remarkable difference between extreme Arctic summers and winters, respectively and certainly an interesting observation. However, we would not yet dare to firmly state that there is such a relationship because of the few seasons we used for this analysis. We are currently working on the analysis of Arctic extreme seasons in the CESM climate model, and it is one of our goals to generalize these results based on ERA5 with the model data.

I appreciate extra work done on DJF2016/17 and 2012/2013 seasons, that showed subsidence in the former season and inversion in the low troposphere in the latter season. That was very convincing.

New Figures 4 and 3 look much better.

I think the information on explained variance by the first two modes given in reply to R3 can be added to the manuscript, e.g., by mentioning that the first two PCs explain over XX% of variance.

Please have a look at line 298 of the manuscript, where we mention that, depending on the region and sub-region, PC1 and PC2 "usually explain about 80%-90% of the total variance".

Minor suggestions (lines in the manuscript with tracked changes):

l.142: is there a threshold on a displacement of atmospheric blocks? I think the term 'blocking' implies some stationarity, while the term 'track' usually suggests propagation. In this case the 'track' must mean duration rather than propagation; perhaps, the wording can be changed to be less confusing.

There is indeed a threshold for the displacement of atmospheric blocks. We define them as spatial objects with a negative PV anomaly exceeding −1.3 pvu, which show a spatial overlap of at least 70% between each 6-hourly time step. Thus, these features are quasi-stationary and can, to a certain extent, propagate with time, which is why we speak of a "track" in this case.

L. 535: I'd say 'depends on their location within the regions of interest'

Thank you for this suggestion. However, we would like to keep the line as it is ("the impact of cyclones on surface anomalies depends critically on their track relative to the region of interest"), as it seems that not only the cyclones' position *within* a region is important, but also *next to it*. For example, in the case of the winter 2011/12 it is important that the cyclones do not move *through* the region of the Kara-Barents Seas but get stationary in the Nordic Seas (and thus only partly reach the region of interest).

l.545: Maybe 'Having analysed two anomalous winters in the Kara and Barents Seas, in this section we present an anomalous winter in the High Arctic to highlight the difference in dynamical processes leading to extreme seasons in various parts of the Arctic'. I think it may be not only the sea ice conditions that are different between those regions, but also the location and, hence, accessibility for midlatitude synoptic systems.

Thank you for the suggestion. We changed the sentence such that it now reads:

"After analysing two anomalous winters in the Kara and Barents Seas, we now discuss an anomalous Arctic winter in the High Arctic to better understand the different dynamical processes leading to such seasons in Arctic regions with distinct surface conditions."

l.563: There is an interesting relationship with the stratospheric vortex. The timing suggests that SSW may have been caused by anomalous tropospheric conditions in late December and then in January the stratospheric anomaly helped create anomalous tropospheric conditions. (this is just a comment)

Thank you for this comment, this is indeed an interesting fact to think about. Coy and Pawson (2015) have shown that the strong vertical wave activity flux from the troposphere to the stratosphere, which led to the preconditioning and the actual splitting of the polar vortex in DJF 2012/13, has been driven by anomalous upper-tropospheric flow conditions. The formation of a high-latitude cutoff high from a strong tropospheric ridge (and subsequent transition into a lower-stratospheric high) as well as a strong tropospheric storm developing over the North Atlantic seem to have played an important role in preconditioning the SSW. Studies which analyzed previous SSW events over the North Pole (e.g., Coy et al., 2009; Harada et al., 2010) also emphasized the importance of significant synoptic-scale events in the upper troposphere for the occurrence of SSW events.

At the same time, it is maybe not surprising, that such strong events in the stratosphere can also strongly influence the tropospheric conditions below due to stratosphere-troposphere coupling. Several studies showed the impacts of SSWs on tropospheric conditions, causing for example extreme cold air outbreaks (Matthias and Kretschmer, 2020). Butler et al. (2017, see Fig. R3) could show that a positive surface pressure anomaly over the Arctic concomitant with a negative surface temperature anomaly (such as we found it for DJF 2012/13) is typical following strong SSWs:

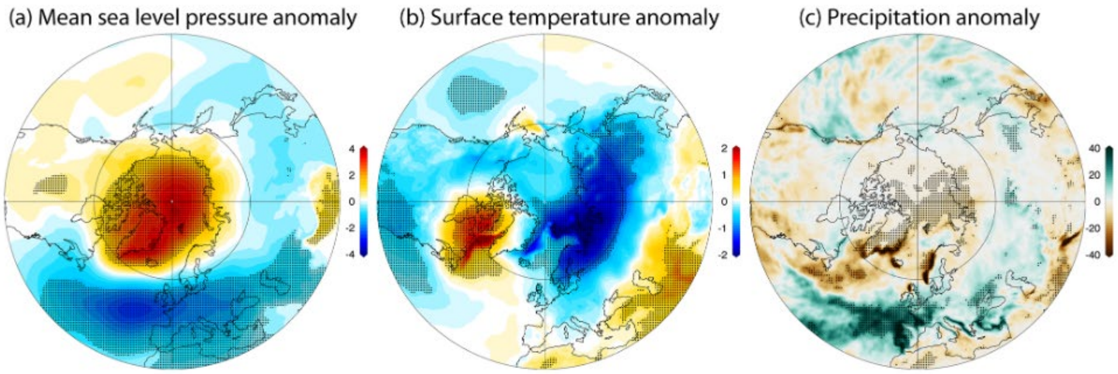

Figure R3: Composites of the 60 days after historical SSWs in the JRA-55 reanalysis for (a) mean sea level pressure anomalies (hPa), (b) surface temperature anomalies (K), and (c) precipitation anomalies (mm). The stippling indicates regions that are significantly different from the climatology at the 95% level. Figure from Butler et al. (2017).

l. 623: remove ‘We can show that’ (if you do this then I’d replace ‘find’ with ‘show’ in l.625)

Thank you, changed as suggested.

l.627: I am not sure I understand how multivariate approach helps with different regions with respect to the SIE. The regions have been defined prior to application of the multivariate approach, so even if a single variable was used, you could still do an analysis for various regions depending on the ice coverage.

The multivariate approach allows us to directly compare the unusualness of seasons in one sub-region and between different sub-regions (and thus different surface conditions) via the definition of an “anomaly magnitude”, which is based on the Mahalanobis distance d_M in each PCA biplot. This anomaly magnitude combines the anomalies of the six different precursor variables into one measure for unusualness. As different variables dominate the unusualness of a season for varying surface conditions, this method allows us to better compare the different sub-regions. For example, in winter, variations in H_S seem to be strongly determining the anomaly magnitude over ice, whereas over the open ocean, variations in P seem to be more important. Our approach allows us to compare the unusualness of seasons independently of the parameter(s) which caused this unusualness and thus also independently of the predominant surface conditions in a specific region.

A few times ‘indicative for’: I am not sure if this is correct, I’d say ‘indicative of’. Please check.

Thank you for this comment, we changed “indicative for” to “indicative of”.

l. 254-255: I think it is worth mentioning that smaller P over the KB seas is probably due to reduced moisture availability compared to the Nordic seas. In both geographics regions, |P|-bar increases over the open-water surface relative to mixed sea-ice conditions.

Thank you for this remark, we extended the concerning sentence such that it now reads “… a larger variability of P can be observed in the Nordic Seas compared to the Kara and Barents Seas, probably due to reduced moisture availability in the latter region.”

l.259: I suggest adding 'in **summer** seasons'. It took me a while to understand this sentence as I initially thought that 'season**s**' referred to JJA and DJF.

Changed as suggested.

l. 281: I am not sure if the word 'continuous' is a good choice. I think you meant that seasonal-mean T2m anomaly showed a wide range of values.

We are not sure we understood your remark correctly. In line 280, we refer to our definition of a "continuous season" in terms of the seasonal substructure of daily anomalies of a particular variable, which we introduce in lines 266f: "Thus, we define seasons with $0.8 \leq \left|\frac{\overline{\chi^*}}{\overline{|\chi^*|}}\right| \leq 1$ as seasons with a "continuous" anomaly.". Several summer seasons fulfill this criterion, implicating that the daily anomalies throughout these seasons are more or less continuously either above or below average.

l. 281-286: I'd start with winter and then discuss summer.

We decided to keep the structure as it is, focusing on the separate discussion of different aspects such as the shape of the plot or the influence of surface conditions. Separating this discussion for summer and winter would lead to more text including more repetition. We also do not discuss each aspect for winter and summer separately, thus, we do not think that the text would benefit from a restructuring.

l. 284: perhaps, replace 'also' with 'even'.

Changed as suggested.

l. 285: I think that for winter, seasonal-mean anomalies in P are so small, that I am not even sure that saying dry or wet seasons is appropriate as the difference between wet and dry may be not statistically significant. In summer, though, there is a linear relationship between P-bar and |P|-bar, which is worth mentioning instead of 'the driest seasons feature some precipitation events' (|P|-bar values for drier seasons are smaller, as would be expected, meaning that precipitation events are either weaker or less frequent or both). Also, I'd remove or rephrase 'maybe less evident' - it is well evident from the plots.

Regarding the first part of your comment, maybe the different scales for the precipitation anomalies in Figs. 5 and 6 gave the wrong impression, that the seasonal-mean anomalies in winter are much smaller than in summer. To avoid further confusion, we adapted the scales for Fig. 5. As the anomalies are similar in size, we think it is appropriate to distinguish between dry and wet seasons also in winter.

We further adapted the text in lines 284f to stronger emphasize the linear relationship between $\overline{P^*}$ and $\overline{|P^*|}$ due to the skewness of P.

l. 294-296: add a reference to fig. S2l and o

We added references to supplementary figures in lines 281f and 289f, where they additionally support our statements.

l. 298: instead of fig. 6l, I'd mention fig. 6i

Changed as suggested.

References

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