

Thank you very much for time dedicated to our manuscript. Please find below your comments repeated again, and our answers. With the help of your advices, we have prepared a restructured, rebalanced and easier readable version of our manuscript.

General comments:

1. The background of Arctic-midlatitude linkages and possible physical relationships between Arctic climate change and midlatitude weather and climate and the role of atmospheric teleconnections has to be described more detailed and sound.

We added the following segment to summarise possible physical mechanisms behind teleconnection and to reveal the background of Arctic - mid latitude linkages to our Introduction (the first sentence was also in the previous version):

“Several studies have demonstrated relationships between warming and/or ice decline, and mid-latitude weather and climate extremes (Handorf et al., 2015; Coumou et al., 2014; Tang et al., 2013; Petoukhov et al., 2013; Francis and Vavrus, 2012; Petoukhov and Semenov, 2010). Others have analysed whether these associations are statistically and/or physically robust (Hassanzadeh et al., 2014; Screen et al 2014; Barnes et al 2014; Screen and Simmonds 2013, 2014; Barnes 2013), while some investigations suggest that the apparent associations may have their origin, in part, in remote influences (Perlwitz et al., 2015; Sato et al., 2014; Peings and Magnusdottir 2014; Screen et al., 2012; Petoukhov and Semenov 2010).”

The relationship between AA and weather extremes and/or persistent weather patterns in mid-latitudes are mostly explained with Arctic and North Atlantic anomalous circulation regimes, waviness and strength of jet stream (Vavrus et al., 2017; Francis and Skific, 2015; Overland et al., 2015; Barnes and Screen, 2015; Francis and Vavrus, 2015; Coumou et al., 2014; Tang et al., 2013; Petoukhov et al., 2013; Francis and Vavrus, 2012). Common supposition is that sea ice declines are primarily responsible for amplified Arctic tropospheric warming. This conjecture is central to a hypothesis in which Arctic sea ice loss forms the beginning link of a causal chain that includes weaker westerlies in mid latitudes, more persistent and amplified mid latitude waves, and more extreme weather (Perlwitz et al., 2015). On the other hand Sun et al. (2016) brought out that neither sea ice loss nor anthropogenic forcing overall yield the winter cold extremes and persistence in mid-latitudes. Arctic warming over the Barents–Kara Seas and its impacts on the mid-latitude circulations have been widely discussed (Dobricic et al., 2016; Semenov and Latif, 2015; Kug et al., 2015; Sato et al., 2014). Another particular regional warm core (Screen and Simmonds, 2010) is East Siberian–Chukchi Seas which is related to severe winters over North America (Kug et al., 2015; Lee et al., 2015). Screen and Simmonds (2010) brought out also the third particular regional warm core – northeast Canada and Greenland which has been less investigated. Wu et al., (2013) focused on winter SIC west of Greenland, including the Labrador Sea, Davis Strait, Baffin Bay, and Hudson Bay and found that winter SIC west of Greenland is a possible precursor for summer atmospheric circulation and rainfall anomalies over northern Eurasia. If we look at the regions in mid-

latitudes then potential Arctic connections in Europe are less clear than with North America and Asia (Overland et al., 2015).

2. All analysis are based on linear correlation analysis. To make inferences about correlations, the test of the Nullhypothesis of no correlation has been performed only. I think, this need to be expanded by, at least, including non-parametric approaches not relying on normally distribution, taking into account the reduction of degrees of freedom due to autocorrelation and also by estimating the confidence intervals of the correlation coefficients. Furthermore, Wallace and Gutzler (1981) introduced a stronger criterion than that of statistical significance to make inferences about teleconnections, namely reproducibility, which should be used here, too. Furthermore, the authors have to be careful not to overstate the results of the simple correlation analyses and have to be aware that correlation does not mean causation.

We agree, that our real data may not fulfil all condition thoroughly that are preconditions for linear correlations, especially linearity and normality. Still, as we are seeking not exact numbers but rather general patters, small violation of normal distribution assumptions should not have considerable effect. Also – as we use mostly seasonal mean values – central limit theorem also gives us credit to assume that the data is at least in some extent normally distributed.

We agree, that correlation does not mean causation always as covariability between two different data can happen without reason. Still, we discuss in the paper only correlations that are larger than ± 0.5 . For two random 37-elements dataset there is possibility to have such big correlation 0.2%, so only 2 cases of 1000.

For reproducibility, we run over all calculations using ERA-interim data, and saw that these two models show similar correlation patterns. We believe that our results using linear correlations are sufficiently confidential for our conclusions even without suggested Wallace and Gutzler (1981) method that would demand totally new calculations.

3. Having in mind the position of the centers of action of the teleconnection patterns over the North-Atlantic-Eurasian region, I suggest to include the analysis of statistical relationships with the Scandinavian and East Atlantic/West Russia patterns.

We added explanations (also for Scandinavian and East Atlantic/West Russia patterns) our choices of indices based on geographical position of the centres of action of the teleconnection patterns in data paragraph (see the segment 1 beneath);

Segment 1:

“The teleconnection indices we applied in our analyses were chosen according to the possible influence due to the geographical position of the centres of action of the teleconnection patterns over the North-Atlantic-Eurasian region. The following indices were chosen: 1) The North Atlantic Oscillation (NAO), which is the dominant mode of atmospheric variability in the North Atlantic sector throughout the year (Barnston and Livezey, 1987); 2) The Arctic Oscillation (AO), which is usually defined as the first EOF of the mean sea level pressure field in the Northern Hemisphere (Ambaum et al., 2001); 3) The Scandinavian Pattern (SCA), which consists of a primary circulation centre over Scandinavia, with two other weaker centres of action with the opposite sign, one over the north eastern Atlantic and the other over central Siberia to the southwest of Lake Baikal (Bueh and Nakamura, 2007); 4) The East Atlantic Pattern (EA), which consists of a north-south dipole of anomaly centres spanning the North Atlantic from east to west (Barnston and Livezey, 1987); 5) The East Atlantic/West

Russia Pattern (EA/WR), which consists of four main anomaly centres: Europe, northern China, central North Atlantic and north of the Caspian Sea; 6) The Polar/ Eurasia Pattern (PEU) consists of height anomalies over the polar region, and opposite anomalies over northern China and Mongolia.; 7) Additionally, Pacific Decadal Oscillation (PDO), which is the dominant year-round pattern of monthly North Pacific sea surface temperature (SST) variability was included. Although its geographical centres are far from the Baltic Sea region, Uotila et al (2015) found that PDO correlated significantly with the ice concentration and temperature of Baltic Sea. All indices were downloaded from the NOAA-CPC database (<http://www.cpc.noaa.gov>).”

4. The analysis should be extended by including other reanalysis. The authors themselves are experts in evaluating reanalysis data over the Arctic (Jakobson, E., et al, GRL, 2012). The same issue has been studied by Lindsay et al., JC, 2014). Based on these evaluations I suggest, that at least ERA-Interim should be studied for comparison.

We repeated the analysis with ERA-Interim. The results were resembled sufficiently in main points in the study region, although there we some discrepancies during summer season in the Central Arctic region. The dissimilarities are mentioned in the manuscript.

Specific comments:

(1) Check the spelling of 'Arctic Amplification' throughout the manuscript.

Corrected

(2) Check the spelling of 'indices' throughout the manuscript.

Corrected

(3) L57: What is meant by 'cold period'

We added the explanation to the brackets: (NDJFM).

(4) L59: 'overall warming. Over which period?

We added a period and a reference as follows:

After 1980s there has been significant temperature increase in the Baltic Sea region (BACC II, 2015).

(5) L62-65: Please, give references for these statements.

We added the reference (BACC II, 2015).

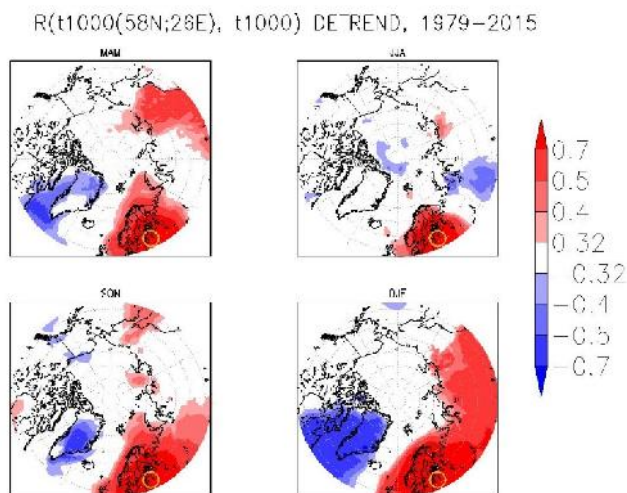
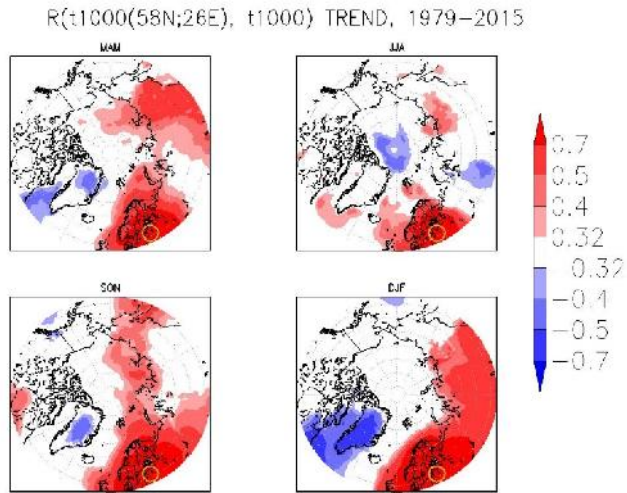
(6) L69: reference Lehmann et al., 2011 is not included in the list of references.

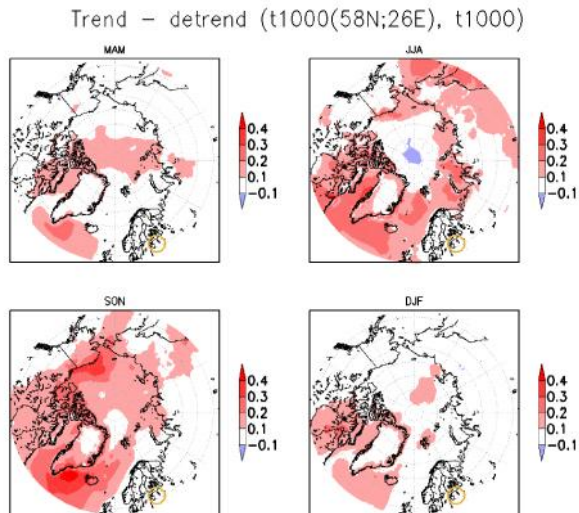
Corrected

(7) L107-108: I have some doubts, that detrending changes the correlations only slightly given only an area averaged value. I would like to see the correlation maps instead.

Here are temperature at 1000 hPa seasonal correlations: first without detrending, second after detrending and third is first figure minus second one. Without trends in temperature, negative correlations with TP would be slightly stronger in Greenland-Labrador area. Our sentence "differences between the areal averages of correlations were up to 0.02 in both directions" is indeed a bit misleading, we replaced it with "detrending did not change general patterns of correlations with TP, only intensified negative correlation in the Greenland region".

In the upgraded version, we show only results without detrending, to focus connections that are present in our world that is influenced by global climate change trends.





(8) Throughout the manuscript, do not call a correlation coefficient of 0.5 as strong, it explains only 25% of variance.

By the meaning of "how much one parameter variance is controlled by the other one", 25-50% is indeed not very strong. At the same time, by the meaning of "how certain we can be that there is connection between two parameters", the probabilities for 0.5 and 0.7 are 99.8% and 99.998% correspondingly, that is quite strong. Even correlations exceeding ± 0.32 are significant at 95% confidence level, so for correlation above ± 0.5 we needed stronger name than "significant".

(9) L237: Though I think the results of the study are valuable, they are not very surprising nor spectacular. Please, be more cautious with your formulation.

We tried to be more cautious with our formulations and replaced "spectacular" with "important".

(10) Fig.2 to 6: Do not include the shading levels below the 95% significance level.

We initially added 68% shading level to mark regions with still quite high possibility for the connections. We upgraded the new version to have first shading level at 95% as you suggested.

Thank you once more for your trouble!

Sincerely yours,

Liisi Jakobson
 Erko Jakobson
 Piia Post
 Jaak Jaagus

References (If we use in our answers references that are already given in our article then we will not give the reference here again):

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