

Referee 2

Our responses to your comments are marked in *italic* below.

I found this paper quite difficult to follow as I was unsure of many of the methods and therefore the results made little sense. I felt that most things were not clearly explained or explained using non-scientific language and the results were not very robust thus making it hard to understand the **relevance and novelty** of this study. I have listed both the major concerns I have with this manuscript and more detailed/minor comments below that.

Major comments

I have a few issues with the methods used:

What is the ridge regression? There is no description as to what this actually does and so is difficult to understand what this means in the results and what especially when its described to be able to explain cause of winter variability in Baltic Sea.

Thank you for paying attention to this. We added: “Ridge regression (Saleh et al., 2019) is a multiple-regression method developed for cases when there is a strong correlation between input parameters.”

For the Arctic area that you assessed, this reaches to as far as 50N. Most studies start the Arctic from **60 or 67.5N**. I think that this is especially important here because you are doing correlations of the Arctic from 50-90N to the Baltic Sea which is from 50-65N so essentially you correlate the Baltic sea area with itself when you are doing a whole Arctic correlation and so will therefore get high correlations in this region. I think it would be best to **stick to an Arctic area that is commonly used in other studies**.

We corrected the Arctic area to be north of 60N. In the calculations, we looked already only at areas north of 60N, so no new calculations were done; the previous definition was just because we wanted to include the TA on the map.

Due to the issues with the methods used I also have a few concerns regarding the results presented:

The description of the results were very confusing. Line 162 you say that the correlations with Svalbard T2M and ICEC are weaker, is this in the table because this is showing the correlation between TA T2M, PSL and PREC and, the IA T2M, PSL and ICEC so I am not sure how you are showing the correlation between Svalbard T2M and ICEC.

We wrote “correlations with T2m and ICEC”, not “correlations between T2m and ICEC”. The correlation in this paper is always between TA and Arctic (region); also, in the table

caption, it is written that “Seasonal correlations of control run 1800 years-long data between areal averages of testing area (TA, rows) and important area (IA, columns) parameters.”

Figure 3 is never referenced in the manuscript and although I assume that it refers to section 3.2 it but this is not made obvious. Within this section, from assessing figure 3, if this is the correct figure, I would say that there is only a slight weakening in the winter North Pole ICEC and TA T2m correlation.

Thank you for paying attention to this. Indeed – we had lost all citations to Figure 3 during the article writing process. The weakening at the North Pole is huge – from a range of 0.3-0.5 to less than 0.1 from 2060.

Why did you correlate seasons to previous months particularly when in the lines 220-222 you state that a monthly correlation will have a weaker influence on the next seasonal average, why not then do solely seasonal lagged correlations or monthly lagged correlations?

Initially, we tried doing seasonal lagged correlations, but these were even weaker than the monthly ones. We didn't do monthly lagged correlations as we were interested in seasonal averages, not monthly ones.

Finally you mention that you analyse Z500 and U10 and yet this is not shown anywhere in the manuscript. I advise either add figures to show this or remove the reference to analysing this completely.

We added to the result a conclusion that “TA parameters correlations with Z500 or U10 were weaker than with T2m, SIC and PREC and were not included in the following analysis.”

Minor/detailed comments

Line 17: Unsure of what you mean about how teleconnection patterns did not show remarkable developments with ongoing climate change, particularly the phrase ‘remarkable developments’

We reworded the sentence: “During ongoing climate change, the teleconnection patterns did not show remarkable changes by the end of the 21st century.”

Line 36: There have been many studies that have discussed Arctic amplification and the causes behind it that should be referenced here.

We added a paragraph: The faster warming in the Arctic compared to the global mean, a phenomenon known as Arctic amplification (AA), is a result of interacting processes: sea ice loss and surface albedo feedback (Lainé et al., 2016; Yoshimori et al., 2014; Serreze et al., 2009; Screen and Simmonds 2010), changes in longwave and/or temperature feedbacks (Dai and Jenkins 2023; Jenkins and Dai 2021; Duan et al., 2019; Pithan and Mauritzen 2014; Lu and Cai 2009), cloud changes (Taylor et al., 2022; Boeke and Taylor 2018; Taylor et al., 2015; Taylor et al., 2013; Francis and Hunter 2006; Vavrus 2004), intraseasonal cycling of heat (Clark et al., 2021; Bintanja and Krikken 2016; Bintanja and Linden 2013), and

poleward energy transport (Sang et al., 2022; Spielhagen et al., 2011). However, the relative weight of these different factors is still under debate (Taylor et al., 2022; Dai et al., 2019).

Line 39: What is meant by; ‘AA is expected to be related to further changes that affect mid-latitudes’

The idea is that AA will not only stay in the Arctic but will also influence mid-latitude climate change.

Line 54: ‘Possibility to glance in to the future’ this is not accurate (or scientific enough) as to what scientists do when assessing the impact of Arctic on mid-latitudes. We assess the impact of Arctic on mid-latitudes to improve accuracy for forecasting or climate projections, we cannot glance in to the future but give a scenario based prediction of what may occur.

We removed this paragraph.

Lines 54- 62: I do not understand the relevance of this section. I suggest either removing it or being explicit about why the reader needs to know about the pace of climate change.

We removed this paragraph.

Line 66: This sounds like you have multiple models but with the CESM-LE you have one model that has multiple members.

There are several large ensembles with individual models and yes – we use only one of them.

Line 101: Can you give references to the classification of the AO, NAO and in particular the Barents Oscillation as it is less well known than the other two.

The definitions are given L102 – L103. „NAO is defined as EOF–1 of seasonal SLP anomalies for 20–80N, 80W–40E, BO as EOF–2 of seasonal SLP anomalies for 30–90N, 90W–90E, and AO as EOF–1 of seasonal geopotential anomalies for 20–90N.“

Line 104: Can you be more explicit here in your methods as to how the ‘correlations with and without the effect of teleconnection indices’ were analysed

We used partial correlation, defined on lines 94-97. For the controlling third variable, we used teleconnection indices.

Line 109: ‘were clearly weaker than with the remaining regions’ feels like there is a word(s) missing here.

We reworded the sentence: “Initially, we also looked at the Chukchi Sea and Canada Basin regions, but the correlations between climate parameters between TA and these regions were clearly weaker“.

Line 123: What do you mean by teleconnection transformations?

We reworded the sentence: “ To analyse the strength and shape changes in teleconnections during climate change, we looked at 20-year periods of the ensemble simulation from 1980 to 2100. “

Line 126: ‘weaker correlations are supposedly not important’ – yes stronger correlations are more important

We reworded the sentence: “In order to make the connections clearer, we do not show weaker correlations than ± 0.1 , though they were still statistically significant.”

Line 137: This line is very confusing, please edit.

We reworded the sentence: “The climatic variables of separate areas are usually dependent, but the strength of the correlation depends on the distance and concrete variable“.

Line 147: Is row 1 or 2 the one that is not controlled by NAO and what is the sign of the strong teleconnection in the Atlantic to the east of Iceland, in my view the strongest correlations are off the coast of North America.

The first row is the regular correlation. The second row is the partial correlation with the controlling factor NAO, which means that the second row would be the correlation when there would be no variability in NAO. The strong teleconnection in the Atlantic at the region around 60N, 30W is negative.

Table 2: Why did you separate East and West Greenland

We separated them because there are clear differences in some parameters, e.g. SLP and PREC in winter.

Lines 181-185: I am not sure which figure you are referencing here, can you add this.

These figures were not added. We clarified this by “Local correlations (at the same spot, no figures shown) are necessary ... “.

Line 202: The correlation becomes negligible off the coast of Siberia rather than fades away and Barents sea is not off the coast of Siberia but Russia. Also a positive correlation is found in the East Siberian Sea in 2080-2100 showing that not all correlation fades away.

New text here: “The negative correlation between T2m and SIC in the control run in the coastal areas of Russia becomes negligible after 2040, except in the Barents Sea, where it strengthens. A positive correlation is found in the East Siberian Sea in 2080-2100 (Figure 3)“.

Line 217-218: What rate are you talking about here and what is meant by incoming parameters?

Here, we use the rate as the percentage of the total variability that a parameter from TA can describe. The Word “incoming“ is indeed accidental; we removed it from the article.

Line 218-219: Revise this line starting ‘It turned out that quite common...’ as I am not sure what is meant in particular what is quite common?

We reworded the sentence: "There were several variables the connection worked also contrariwise, where the values ..."

Line 224: What is meant by CESM-LE self-consistent database?

We agree the expression is confusing. We reworded the sentence: „The advantage of this study is the length of the stationary 1800-year-long CESM-LE control database“

Line 225: These correlations are very weak, plus this value differs from the one stated in the methods, are these values supposed to be different?

The idea in presenting these values is to show that for such long timeseries, even quite weak correlations are statistically significant. Here, the stated value is for a database with 1800 elements. In the methods, there was also the value for 800 element database added.

Line 230: Are there no more up to date papers than Hildebrandsson which is 109 years old?

We moved the paragraph to the introduction and added newer references:

Zhuo, W., Yao, Y., Luo, D., Simmonds, I., Huang, F. 2023. The key atmospheric drivers linking regional Arctic amplification with East Asian cold extremes, Atmospheric Research, Volume 283, 2023, 106557, ISSN 0169-8095, <https://doi.org/10.1016/j.atmosres.2022.106557>.

Deng, K., Yang, S., Ting, M., Lin, A., & Wang, Z. (2018). An intensified mode of variability modulating the summer heatwaves in eastern Europe and Northern China. Geophysical Research Letters, 45, 11, 361–11, 369. <https://doi.org/10.1029/2018GL079836>

Line 236: What differences in the model parameters and different periods from CESM-LE complicated a comparison with the Jacokson et al. 2017 paper? A lot of studies compare reanalysis and historical CESM simulations.

It is quite common to do a comparison between reanalysis and climate model data. If you want to repeat analyses compiled for reanalysis with climate model data, then your choices are limited with the previous work. In our 2017 paper, we did not analyse 2 m temperature but 1000 hPa level T2m and its vertical profile, but in this paper, we were mostly interested in 2 m temperature.

Line 237: The comparable T2m – comparable to what?

Changed to “The comparison of T2m in the present study ...”

Line 253: What is natural SAT variability?

We added an explanation to the natural variability: "... related to natural variability (the variation that humans do not cause) of ..."

Line 260. This sentence is confusing, please reword.

Changed to “Changes in the TA T2m correlations with ICEC in the Arctic concur with negative trends in ICEC and positive trends in T2m in TA.”

Line 262: I am not sure what you mean here, can you reword.

Changed to “The strongest correlation between TA and Arctic region parameters was T2m in TA and T2m in the Greenland region.”

Line 273: what local factors are you talking about?

We replaced it with “local meteorological factors“ (likewise Iguchi et al., 2018; Chen et al., 2017 used the term), which means that the global circulation influence is smaller.