

The linkage between the warm Arctic and mid-latitude weather and climate is a hot topic for cryosphere research community and for this reason, I see this study is interesting and worth to be noticed as a scientific publication. The manuscript is well structured, and the objectives of this study are clear. The content fits well the scope of ACP.

I recommend this manuscript to be published in ACP. However, I see there are some aspects scientifically and technically that still need further improvement for better clarity of this manuscript, I hope authors can make corresponding revisions based on my comments below:

1 Title: “Revisiting the trend in the occurrences of the “warm Arctic-cold Eurasian continent” temperature pattern” Why “revisit”? Have you (authors) done this before? Or are there other papers dealing with this matter before? if so, what are the scientific outcome from those existing studies?

We have not carried out previous research on the potential mechanisms for the trends of warm-Arctic-cold Eurasian per se, but there have been several other studies that are either directly or indirectly related to this specific topic. Two main conclusions regarding the forcing behind the trends stem from these studies. One conclusion is that the recent warm Arctic-cold continents pattern can be attributable to the Arctic sea ice loss (Inoue et al., 2012; Tang et al., 2013; Mori et al., 2014; Kug et al., 2015; Cohen et al., 2018; Mori et al., 2019); The others disputed sea ice loss as a driver for the trend (Blackport et al., 2019; Fyfe, 2019). Instead, they point to internal atmospheric variability and the Pacific and Atlantic SST oscillations as potential forcing behind the trends (Lee et al., 2011; Sato et al., 2014; Matsumura and Kosaka, 2019; Clark and Lee, 2019). Most of these previous studies and the two school of thought were mentioned in the Introduction. Our work, which took a different approach, confirmed the second school of thought. Because of these existing studies on this topic, we used the word ‘revisiting’ in the title of our manuscript.

2 To my understanding, SOM is a pure advanced statistical tool and there is nothing related to the physics, right? If this is the case, shall I say any results come from SOM have uncertainties because you need to pre-define SOM nodes and this procedure is a kind arbitrary, right? On top of it, as you pointed out in the abstract only 40% of the surface temperature trends are explained by SOM pre-defined nodes that fit to your pre-condition, i.e. warm Arctic-cold Eurasian continent. What I am trying to say is that for what kind of criteria you need to be satisfied before you can make a rebuts conclusion to say: “ok, there is a linkage” or “no, there isn’t a linkage”. This comment and “a kind of arbitrary” above come from your description on line 141-143.

SOM is an advanced statistical tool for pattern extraction. Although SOM is superior to some other existing pattern extraction tools such as EOF, it suffers from the same limitations as other statistical tools in identifying physical modes. That was why a large part of the manuscript was devoted to explain the existence of the patterns and their trends based on physical understanding of atmosphere and ocean dynamics that had been established from theoretical framework and/or from coupled ocean-atmosphere modeling. Yes, to use the SOM method, one has to pre-define SOM nodes and the procedure is not completely objective. A small grid (each node has larger frequency of occurrence) tends to miss transitions between the main patterns that are retained by a large grid. But an excessively large grid could

sidetrack the attention from the main variability patterns. Nevertheless, changing the grid from 3x3 to 4x4 or even larger would not change the main conclusion.

3 How sensitivity of the data source will impact the final result? In this study, you have applied ERA-Interim data. if you use other data resource, e.g. NCEP or MERRA, would be your conclusion changed entirely or partly? I am not asking to use these data sets to rerun SOM, but it would be nice to comment it at the end of this study.

We believe our results are not particularly sensitive to the specific large-scale reanalysis data source. We could have also used ERA5, or NCEP or MERRA and arrived at similar conclusions, although there might be some minor differences. We have added some comments on this point at the end of the study.

4 Authors focused on the impacts of the SST anomalies over North Pacific and Atlantic Oceans on the trend in the occurrences of the “warm Arctic cold Eurasian continent” temperature pattern. The influence of decreasing Arctic sea ice cannot be ignored.

You may consider to add discussions on the influence of sea ice to your pre-defined warm Arctic and cold Eurasian content.

We added some discussions on the influence of sea ice in the Conclusions and Discussions section.

There are a number of technical details need to be clarified:

a) Fig.1: All “percent” sum together is larger than 100%, please check.

Changed

b) Fig.2: The color bar refers to what? Contour color? what are the background (fingerprint like) information in each sub-plot? The text explanation for figure 2 (line 182 -185) and figure 2 presentation seems not match to each other. I suggest you remove unnecessary from the plot and only show what you have explained in the text so readers can understand better.

Both color bar and contour color refer to 500-hPa geopotential height anomalies. Dotted regions in each sub-plot indicate the above 95% confidence level.

We revised some of the discussion.

c) The comment above applied to at least Fig, 3, 4, 5 and 6.

In Figure 3-6, shaded and dotted regions indicate the above 95% confidence level.

d) “same as Figure2, but for,,,” This is not a good figure caption, please write clear with full information. For those surface fluxes, I think you need to explain the unit of the fluxes, are those daily accumulated fluxes?

We revised figure caption with details. The fluxes are daily accumulated fluxes, which are now explained in the caption and text.

e) The sea ice concentration figure needs more explanations, e.g. node information was missing; what was meant for positive and negative anomalous? is this also for winter season? how about summer season? Now I realized you actually only investigate winter season for

everything, if so, you need to say this explicitly in the beginning of the paper.

We added node information. The anomalous sea ice concentration is a composite result based on the occurrences of nodes. For example, the negative sea ice concentration corresponds to the spatial pattern of air temperature for node 1. In this paper, we only examine warm Arctic-cold continents pattern in boreal winter, which was mentioned in the first and second paragraph of the manuscript.

f) Fig.7 and 14: I have difficult to understand these figures? What we can learn from those figures? If you only tell the integrated total number of days for each node and compared with showing this figure, what we will missing up?

Figure 7 and 14 show the integrated total number of days for each node. In Figure 7 and 14, the numbers for nodes 1 and 4 are larger after 2000 than those prior to 2000. The opposite occurs for nodes 6 and 9. Figure 14 mainly show an interdecadal variability of the number. The trends in the number for nodes 1, 4, 6, and 9 are a fragment of the interdecadal variability. We added clarification in the discussion.

g) Fig. 12: “wave activity flux”: This need to be explained more in detail both here and in the text. 100m²/s, what is this? and in the caption:107 m²/s.

“vector 100m²/s” in the figure is figure legend of wave activity flux. The unit of stream function is m²/s and its magnitude is the product of the values in the figure and 10⁷. We have added explanation of wave activity flux in the discussion and in the figure caption with a reference.

h) Please mark the study area in corresponding figures 2-6, to help readers understand the mechanism impact more intuitively.

Marked

i) Table 3 is not mentioned in the article, and some problems of uppercase and lowercase letters (such as not show or Not show), please check them carefully.

Changed

j) The order of the nodes should be consistent in figures, 10-12.

Changed

k) Authors should increase some discussions about the application of statistical results in prediction of surface temperature Arctic cold Eurasian continent.

Added discussion

The results in this study are based on statistical analysis. Some numerical experiments may be considered in the further studies.

Added