

Interactive comment on “Intermittency of Arctic-midlatitude teleconnections: stratospheric pathway between autumn sea ice and the winter NAO” by P. Y. F. Siew et al.

Anonymous Referee #2

Received and published: 12 December 2019

General Comments:

The present manuscript discusses the potential link between Barents and Kara sea ice in autumn and the late winter NAO via the so-called "stratospheric pathway. By applying a causal effect network (CEN) approach, the authors focus on the intermittency of the pathway and discuss the role of both sub-seasonal and synoptic processes. The manuscript is well written and structured and the results are presented in a balanced way and discussed in the context of the relevant literature. Overall, I find the analysis a valuable contribution to the ongoing debate about Arctic-mid-latitude linkages, highlighting the importance of different time-scales and potential non-stationarities as

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well as the need for statistical concepts to deal with these challenges. I do have some comments which I think would improve the manuscript. In particular, the significance testing of the CEN analysis is not optimal and the regression analysis seems to lack some consistency with the CEN analysis. The results of the “Synoptic linkages and interactions across times scales” section on the other hand deserve more weight in my opinion as they are a novel attempt to explain non-stationarities of the stratospheric pathway and might be an important step towards reconciling model and observation analyses.

Specific Comments:

1) CEN method

I am well familiar with the applied method and am certainly convinced by its advantages and thus also pleased to see it being used. Nevertheless, I think that the authors do not apply it an optimal way. Since Kretschmer et al. (2016) and Runge et al. (2014) there have been several advancements to this approach as discussed in more detail in Runge et al. (2019, which was published after the author’s submission) and Kretschmer et al (2018, npj) and Runge et al. (2018, Chaos). In particular the authors do not adequately account for the issue of multiple testing and false positives:

They only test links found in their step 1 (the correlation analysis) and then use partial correlation tests to check if the correlation from step 1 can be explained by confounders. Due to the involved multiple testing (in step 2) of links, the significance level alpha cannot be interpreted as the false positive rate. The authors, however, use this alpha level for further interpretations of their results. Note that this can be overcome by considering their step 1+2 only as a “condition selection step” and then testing each possible link again using partial correlations with the conditions identified before (as described in detail in Runge et al. 2019). Not only does this yield in more statistical interpretability but, furthermore, this can also lead to higher detection power. The reason is that a true link A->B can be overlooked because correlations are zero (see

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their step 1) but conditioning out the influence of a common driver C (with opposite effects on A and B) reveals the actual signal (which might also affect the intermittency statistics). In this context, also note that estimating this condition set for each variable can be done by using different and rather liberal alpha levels (considered as a hyperparameter or a significance “threshold”) and the “optimal” set can then be chosen, for example, based on the Akaike information criterion (AIC), as described in Runge et al. 2019.

Further, the authors should be aware that the overall resulting network is also subject to the “field significance” issue (see e.g. Wilks 2017, BAMS). This means that just by chance, some of the detected significant links will be false positives. This can also be addressed by applying false discovery rate corrections to the overall network. This issue might not be too relevant for the monthly CEN which only consists of few links, but might be an issue for the synoptic-scales CENs.

That said, I don’t expect the authors to include all these novelties in their approach. Nevertheless, these issues should be discussed properly and the parts where referred to significance should be adopted. Further, I don’t agree with their statement in I 136 that testing each link again in step 2 of the analysis in Kretschmer et al (2016) is “somewhat arbitrary”. On the contrast, it is one way to deal with multiple testing problem as described above. Maybe the authors also want to consider comparing their results with the pcmci implementation provided here <https://github.com/jakobrunge/tigramite/>.

2) Strength of the pathway

When estimating the influence on NAO variability, the regression analysis seems somehow contradictory to the previous analysis. Why would one include the whole pathway if one believes that it represents an indirect chain of links? More precisely, if, for instance, the influence of BK SIC on SPV is via Urals then, in theory, the whole information is already contained in Urals and adding BK SIC in the regression should not provide additional information. Following the logic of a network

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approach, the causal effect from A→B is the sum over the products of link coefficients along all possible paths between the two variables (see for example here: https://github.com/jakobrunge/tigramite/blob/master/tutorials/tigramite_tutorial_causal_effects_mediation.ipynb)

Also, it seems obvious that adding more regressors to the regression analysis also increases its r^2 ? Is the analysis performed for all years? Wouldn't it make sense to use a similar bootstrap approach or at least some leave-k-out cross validation?

As stated before, a more interesting analysis could be an attempt to quantify the contribution of sea ice to the NAO via the tropospheric and via the stratospheric pathways alone. This recent paper might be an interesting source of inspiration how to approach this using the GEN approach:

Saggiaro, E. and Shepherd, T. G. (2019) Quantifying the timescale and strength of Southern Hemisphere intra-seasonal stratosphere-troposphere coupling. *Geophysical Research Letters*. ISSN 0094-8276

3) Intermittency

Addressing the intermittency of the Arctic – NAO link is very interesting and important given that it might provide an explanation why models and observation studies don't seem to agree on this topic. It is well known that not all extremely weak polar vortex states (or SSWs) affect tropospheric circulation (e.g. Karpechko et al. 2017, Runde et al. 2016). Do I understand the authors correctly that sea ice variability might also contribute to this downward intermittency (for example due to sub-seasonal-synoptic interactions)? I think it would make sense to highlight the intermittency of both the upward and the downward coupling mechanisms separately (with the upward part representing somehow the potential of sea ice to influence the NAO).

In my opinion the section “Synoptic linkages and interactions across times scales” is the most novel contribution to the rather large body of literature on this topic and deserves

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to be highlighted even more. In this context it would be great if the difference between autumn/early and late winter linkages could be discussed in more detail. Which season of sea ice loss is most relevant for the stratospheric pathway, which for the tropospheric pathway. Is it possible to quantify how much of the intermittency is explained by things such as ENSO or synoptic variability?

Please also consider discussing/citing this recent paper: E Tyrllis, E Manzini, J Bader, J Ukita, N Hisahi, D Matei, Ural blocking driving extreme Arctic sea-ice loss, cold eurasia and stratospheric vortex weakening in autumn and early winter 2016-2017, Journal of Geophysical Research: Atmospheres 124, 11313-11329

Technical comments:

L3-4: not so obvious to me if really leading to a transition to –NAO (e.g. Karpechko et al 2017) L4: The Causal Effect Network. . . L32: Kretschmer et al. 2016 does not use lagged correlations L46: do these studies also consider BK sea ice? There is a lot of evidence that sea ice in the Pacific sector leads to a strengthening. L63: Maybe state which linkage you mean exactly. L189: 10,000 L 203: Why only those where it appears and not all? Fig. 3: switching the axes would make it more intuitive. L273-277: very interesting thoughts but should rather be moved to discussion?

References:

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