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Interactive comment on "Identification of Droughts and Heat Waves in Germany with Regional Climate Networks" *by* Gerd Schädler and Marcus Breil

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The authors of this paper present an interesting application of the relatively recent concept of functional climate network analysis to identifying and characterizing droughts and heat waves across Germany. Specifically, they employ a network construction to a subset of the gridded E-OBS data product for daily maximum temperatures and precipitation sums covering entire Germany plus a bit of the surrounding European land area. The presented application is novel in a few aspects: First, the network construction based on the distinction rainy/dry days as opposed to the more often employed selection of "heavy" precipitation days. Second, the focus on time windows of six or three months corresponding to a specific season instead of employing a running window analysis. Third, the consideration of a regional network constructed with a fixed

threshold to the bivariate statistical association measure, instead of a fixation of the edge density that has been more widely employed in recent regional studies. These aspects together provide interesting new insights, yet also call for extended justification and discussion, which I however see only partly provided in the present manuscript. As a consequence, I have a few comments that I would like to invite the authors to consider in revising their work prior to acceptance for publication in NPG.

General comments:

1. There is a vast body on complex network applications in climatology, so it is surprising that the authors essentially cite only some very old papers (Tsonis et al. 2006, Donges et al. 2009) instead of pointing the reader to more thorough recent overviews on the topic (like the review chapter by Donner et al. in the book "Nonlinear and Stochastic Climate Dynamics", 2017; or the book "Networks in Climate" by Dijkstra et al., 2019). In general, referencing the existing literature on climate networks needs to be considerably improved.

2. It is a neat idea to use the association measures between binary yes/no (rain/no rain) sequences describing the precipitation dynamics, yet this obviously throws away all information on precipitation strength with might be valuable in its own. Other works on precipitation based climate networks attempted different strategies – 1) focusing on the timing of locally extreme events only (cf. Malik et al. NPG 2010, Clim. Dyn. 2012; Boers et al. 2013- in a series of papers in GRL, Nature Comm., Clim. Dyn., and most recently Nature; Stolbova et al, NPG 2014, to mention only a few of them, not to request citing those excessively but just to bring the scale of associated publications to the authors' attention), 2) defining an alternative correlation measure replacing the zero precipitation points by the mean rainfall on the rainy days (Ciemer et al., Clim. Dyn., 2018) – I suggest to at least mention those methodological alternatives and motivate more clearly the setting followed in the present work. Regarding the latter, the authors mention the Hamming distance for binary series (p.2, l.14), but appear to use the product of the two sequences (yet ignoring the joint occurrence of no-rain days,

p.5, ll.3-4). This is somewhat confusing and should be clarified from the beginning.

3. As mentioned above, most recent climate network applications have fixed the edge density and let the correlation threshold vary with time, instead of vice versa, the reason being that many network characteristics are directly affected by changing edge densities. In this regard, it is not completely surprising that edge density and clustering coefficient provide rather similar results.

4. Page 3, ll.26-30: I disagree with the authors' interpretation of high values of the clustering coefficient indicating "strong collective behavior" – this is rather represented by a high edge density. The clustering coefficient focuses on transitive connectivity relationships and thereby rather describes the redundancy of connectivity. In a system with spatial autocorrelations implying the linkage probability being distance-dependent, it is likely that a higher edge density implies a slower decay of the spatial autocorrelation and, hence, a higher likelihood of denser (and therefore more transitive/clustered) regional connections. In general, one should keep in mind that the behavior of the clustering coefficient may change with the edge density (and associated with this, the shape/type of the degree distribution). This mutual dependence between the average local clustering coefficient and the degree distribution has been known in network theory for more than 20 years (cf. Barrat and Weigt, EPJB, 2000) and has led to an alternative definition of a global clustering measure often called network transitivity. Moreover, the implications for clustering properties in climate networks at a global scale have been discussed in great detail by Radebach et al., PRE, 2013. The latter paper showed that the behavior of the clustering coefficient under changing conditions can completely reverse if the edge density is varied, while the transitivity provided stable results. Their networks however contained about 10,000 nodes for edge densities of the order of 1% - notably the same order of magnitude as also used in the present work, yet with substantially smaller networks in terms of the number of nodes. It might be interesting (and possibly even relevant for the interpretation of the presented results) to see if similar effects also take place at the regional scale considered in the present work.

Taken together: some of the findings obtained in this study could be well interpreted in the light of the aforementioned references, while there are two questions remaining to me: 1) Why did the authors choose to consider a fixed correlation threshold instead of a fixed edge density? 2) Why did the authors use the mean local clustering coefficient instead of the global network transitivity, which would be less dependent on the edge density and degree distribution?

5. When you consider normalized network metrics accounting for their sample mean and standard deviation, it might look surprising that you only consider the cases >1 and disregard possible cases with normalized values <-1. This might be well justified by the fact that large-scale extreme situations are rather accompanied by elevated values of the network properties (e.g. the elevated transitivity values in global surface air temperature networks along with El Nino/La Nina found by Radebach et al., cited above). However, this aspect should be clarified for readers not familiar with the existing literature on climate network applications.

6. I am strongly concerned about the authors' interpretation of the found degree distributions. For example, on p.4, l.10, they report that they "would expect a network structure resembling a random network". I fundamentally disagree with this statement, especially on such relatively small regional scales. The climate system is always characterized by spatial autocorrelation, implying that the probability of linking two random nodes is not constant (as for a random network in the Erdös-Renyi sense), but depends on the spatial distance. The appropriate null model would therefore be a random geometric graph (e.g. Dall and Christensen, PRE, 2002), which has distinctively different features than an unconstrained random graph. As a result, the authors should carefully revise all statements indicating that a Poissonian degree distribution might provide a good benchmark; in fact, it cannot by construction in the present case.

7. In a similar spirit, the authors report that extreme years see a "more uniform" degree distribution (which I can accept without problems) and have "a heavy tail" (which I cannot accept without the authors showing that the tail is actually heavy, i.e., fol-

lows a power-law decay, which I can hardly believe to be the case due to the spatial constraints/boundaries of the constructed regional networks). Please provide corresponding evidence or rephrase.

8. It is interesting that the edge density for the whole network is commonly smaller than in the two subnetworks. Does this imply a reduced presence of North-South links connecting the northern and southern subdomains? I think it might be interesting to study this further, e.g., by using directional network properties like in Rheinwalt et al., Clim. Dyn., 2015, or Wolf et al., PRE, 2019. (But this is more a suggestion for follow-up works.)

9. Still related to the results summarized in Tab. 2: Can you add information on the spread among the extreme/normal years instead of showing only two examples that might reflect either an unbiased or a biased selection? Just the numbers as they stand now do hardly present relevant information in a proper statistical sense.

10. I recommend some improvements on the artwork (larger axis labels/ticks with proper symbols/words instead of cryptic abbreviations as labels). The figure captions should be self-explanatory (e.g., I do not find information about the meanings of the red lines in Fig. 9).

11. When comparing the network classifications with the EDI/EHI based classifications, I recommend to avoiding using the terminology of "false" classifications (maybe rather use "inconsistent"), since neither of the methods presents something that should be considered a "ground truth". Even EDI/EHI are not necessarily the "gold standard" to be used for benchmarking drought/heat wave years/seasons.

Minor suggestions:

- P.1, ll.7-9: "Metrics to identify extremes. . ." sounds a bit bold, I'd rather suggest some more explicit formulation like "Metrics to identify extraordinary network configurations. . . during years with extraordinary drought or heat conditions."

- P.2, ll.8-9: Is it really necessary that extreme events have such large spatial scales? I believe this depends on the type of extreme and its associated temporal scale – while being correct for seasonal-scale extremes discussed in this work, it might be questionable for some short-lived extremes like heavy rainfall due to almost stationary convective (thunderstorm) activity.

- P.2, l.13: Instead of "correlation measure", I suggest using the more general term "statistical association" measure. The Hamming distance is not a correlation measure in the usual statistical sense.

- P.4, l.2: "limit of a binomial distribution" should be clarified a bit, you probably refer to the fact that the probability distribution for each single link to exist is binomial.

- P.4, l.5: Mutual information is NOT(!) a network measure like the other mentioned quantities, but a different statistical association measure for constructing the networks.

- P.9, ll.21-22: Does this mean that you consider this finding mainly an effect of temporal rather than spatial autocorrelation?

- P.9, l.23: I would rather interpret lower clustering coefficients as indicative of (spatially) more "fragmented" connectivity structures, which would match with the statements regarding orography and land use.

- P.11, ll.4-5: I am somewhat surprised about this statement. To my best knowledge, the northernmost and especially northwestern part of Germany has a climate that I would hardly characterize as continental.

In summary, while the paper is interesting and well written (a few typos need to be corrected), I feel that the work is not sufficiently mature in the sense of being properly presented and interpreted in the context of the existing topically relevant literature. I therefore recommend the authors to perform a major revision along the lines of my above comments before further consideration of this work for final publication in NPG.

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