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Potsdam, December 10, 2020

Dear Editor,

we have gratefully appreciated the interesting and helpful suggestions and the general positive feedback of the two anonymous reviewers regarding the presentation of our study.

In the following, we present a point-by-point response to the comments and remarks, with the comments of the reviewers shown in blue, italic font.

Reviewer 1

- *ll. 88-89: This is NCEP/NCAR - Reanalysis v1 - Kalnay et al 1996*

  In fact, most of our analyses have been based on data from the NCEP/NCAR Reanalysis v1. However, in the discussion section, we also used 200 hPa relative vorticity, 200 hPa as well as 850 hPa winds, and 500 hPa geopotential height from the NCEP-DOE Reanalysis v2 for Figure 6, which is why both datasets are mentioned here. We have clarified this in our revised manuscript.

- *1. ll. 98-100: It is not clear to me the difference between the two parameters. Could you please clarify, maybe with a figure?*
  2. l. 103: or you use a module here, or you have to mention before this formula that event m is preceding event l.
  3. ll. 104-105: I had to open Odenweller and Donner because this paragraph it is not very clear. Once comparison starts, I find some differences e.g. event coincidence rate can be precursor or trigger in the original paper. Formula (1) is trigger event coincidence rate. Please use the same symbols of the original paper (s").
  4. l. 107: anticipate this sentence before the formula 0 ¡ ... ¡ delta T. please use original symbols of the paper you are citing (s")
  5. ll. 112-113: since you are studying instantaneous non lagged event, why are you using tau in the above formulas?
  6. l. 121: please use original symbols of the paper you are citing (s")
  7. l. 125: this is just a question: being tau equals zero throughout all the manuscript, does it make sense to write it everywhere? It can be yes or no, but please motivate it.

  We thank the reviewer for these comments pointing out that we need to further clarify our methodology. We have rewritten this section with a special focus on the raised points. Indeed, regarding comment #7, we agree that we can omit the second ECA parameter τ, thereby possibly relieving the necessity of adding another schematic illustration to clarify comment #1. As for comment #3, we have however decided to stick to a simpler notation, since the original one (s") corresponded to a specific context in the mentioned reference and appears unnecessarily complicated within the method description of the present work.

- *ll. 113-115: here 3 appears a magic number. Could you please provide a more physical explanation? I don't know (just guessing - it should be justified): e.g. transport of moisture from point a to b can be maximum three days.*

  The reviewer is correct in that we motivate our choice based on the related atmospheric processes. We have included an explicit explanation of the underlying rational in our revised manuscript.

- *l. 199: maybe I missed it in the paper. What is ITCZ?*

  Thank you for this comment. We have indeed missed introducing the abbreviation ITCZ for the inter-tropical convergence zone. We have added this information to the manuscript.

- *ll. 206-208: I know where is Honshu... I wonder if all the readers of this journal know it. Could you please indicate it (with a point) in your map or use any coordinate?...*

  We thank the reviewer for this remark. We agree that a labelling of the islands and the Japanese Sea will be useful for following the discussions in our manuscript. Therefore, we have now included a physical map of the region with indications of the main geographical features discussed in the manuscript.

- *ll. 224: could you please identify somehow the double band in the figures? (maybe with two ellipses or anything you think may work better). This may help people with red-green colour blindness.*

  We have highlighted the corresponding structures in Fig. 1c and d of the revised manuscript.

  ll. 233-235: I have a question (just to clarify). Did the northern high degree band disappeared because of any physical phenomenon OR it is disappeared because the area that you take into account is too small?

  It is likely that the northern band disappears since the link distance pattern changes completely due to the seasonal reorganization of the atmospheric circulation. Unfortunately, since the TRMM data set is limited to a latitude below 50°N we unfortunately cannot check the possible effect of a larger study area in the context of the present study. However, further exploring the mechanisms described in our work as represented in other more global data sets presents an interesting direction for future research.

- *l. 246: Another question. Why mid-June/mid July? are these months more important than the other that you are studying? Do you want to capture a transition? the two bands* *are more evident? could you please add a sentence where you specify it? thanks*

We agree that we have to more explicitly clarify this choice in our manuscript. There are in fact two reasons behind. First, we have observed that the double band is most prominent in this period. Second, the cross-degree peaks exactly for the 30-days window covering the period between mid-June to mid-July. We have added a corresponding note to the manuscript.

- *ll. 294-296: is this approach similar to the one that was first used in one of the prof. Kurths paper? In case yes, I can't remember which one it was... could you please check?*

  It is not clear to us which paper and specific approach the reviewer is pointing to here. There have been various papers from the mentioned group using event synchronization strength (rather than ECA) based functional network analyses in the context of monsoon dynamics over India, South America and also East Asia. Some of them have also looked at synoptic situations occurring with the emergence of synchronized rainfall (e.g., Boers et al., Nature Comm., 2014). However, we are not aware of any work that has explicitly studied the synchronicity of heavy rainfall events in two distinct regions with the statistical approach that we have taken here.

- *l. 308: I'm not sure that anti-synchrony is equal to "extraordinary low event coincidence rate". I would rather use "asynchronous". a- stands for without — anti- stands for against. So for me anti-synchrony would be a negative synchrony (that can be also high)*

  Regarding this comment, we would like to clarify that we indeed did not mean "asynchronous" behavior (in the sense of absence of synchrony) but really some "negative synchrony" in the sense that during heavy rainfall in one of the regions, heavy rainfall would be suppressed in the other, i.e., would occur less frequently than expected just due to chance. (If phrased in terms of phase synchronization, this would correspond to anti-phase behavior. However, since we are arguing in terms of events here, such terminology does not easily apply.) We have clarified this point in our revised manuscript.

- *l. 319: this is very vague. Could you please specify what do you mean for particularly high activity? I mean in numbers.*

  Here, we refer to the event coincidence analysis regarding the number of events in the two regions. We have rephrased this sentence specifying the quantiles of event coincidence rates.

The identified typo in l. 351 has been corrected in the revised manuscript, too.

Reviewer 2

- *In my experience, TRMM data are biased, resulting in general an underestimation of the effective heavy rainfall amount. The choice to assume 90th percentile as threshold for identifying heavy rainfall, instead of higher ones, and the fact the authors work on rainfall occurrence of rain amount exceeding that threshold should not have significant effects on* *the proposed analysis, but a brief discussion about the reliability of TRMM data in the context of the study probably could be appropriate.*

We thank the reviewer for this useful comment. We acknowledge the presence of biases in the TRMM data, while still believing that it presents the most suitable data set for our study in terms of spatiotemporal resolution and coverage. As long as the biases at a given point in space are not time dependent, utilizing our approach should not be affected by those circumstances at all, since – as the reviewer emphasizes correctly – we are not interested in absolute rainfall sums. We have discussed this point in the revised version of our manuscript.

- *The paragraph 2.2 Event Coincidence Analysis (ECA) and 2.3 Functional Network analysis should be revisited because they are not sufficiently clear specially for people not familiar with the methodology proposed. For example, the variable sj in Eq.(1) is not defined. Furthermore it is not clear to me why for $\tau = 0$, Qij of Eq.(5) and Eq. (6)should be different.*

  We fully agree with this statement, which is also well in line with corresponding observations of reviewer #1. We have rewritten essential parts of the two mentioned sections to clarify the necessary methodological details and hopefully make them better accessible to readers that are not yet familiar with functional network analysis and event synchrony measures.

- *I am not so sure that ECA is in each case better than ESS. There are proposals that solve the drawbacks in Quiroga et al. 2002, see for example Conticello et al,2018, InternationalJournal of Climatology, 38(3), 1421-1437., or Conticello et al. 2020, WaterResources Research 56.4 (2020): e2019WR025598.*

  The reviewer is completely right in their comment. Indeed, we were not meant at all to emphasize here that ECA is superior to, or better than ESS. Rather, ECA does not experience the intrinsic problems of an uncorrected ESS in the presence of temporally clustered events that can indeed be corrected for by proper declustering. Some further more detailed discussion on the differences of climate network properties obtained with ESS and ECA as similarity measures can be found in a recent paper co-authored by some of us (Wolf et al., Chaos, 2020), with a follow-up study currently under review. As a result of those corresponding more systematic intercomparisons, we would like to emphasize that networks constructed based on both ESS and ECA indeed capture similar information, yet may substantially differ in their higher-order characteristics. We have attempted to clarify those points in the revised version of our manuscript.

- *3) If and eventually how the morphology of Japan, characterized by steep mountains 1500 to 3000 m. high, affects heavy rainfall spatiotemporal structure of the region examined, probably deserves some mention.*

  We agree that the elevation of the Japanese Alpes and other mountain ranges in the area of interest markedly affects the spatial patterns of heavy rainfall, which can also be partly observed in Fig. 1b. However, it is interesting to observe that the "most synchronized" heavy rainfall sequences occur over the open ocean and the Sea of Japan rather than over land. This could point to orographically induced heavy precipitation more often occurring along with smaller frontal systems or more localized convective rainfall in our study region and the considered season, rather than in a coherent large-scale spatially organized manner. On the other hand, elevation (along with the land–sea contrast) shapes atmospheric circulation in the study region and thereby intimately contributes to the existence of those circulation patterns that are finally responsible for the emergence of the reported double-band structure of synchronous heavy rainfall activity. We have added a short note on this observation to our revised manuscript.

On behalf of the authors,

Sincerely,

Frederik Wolf