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Earth System Dynamics
Discussions

Interactive comment on "Tropical and mid-latitude teleconnections interacting with the Indian summer monsoon rainfall: A Theory-Guided Causal Effect Network approach" *by* Giorgia Di Capua et al.

Anonymous Referee #2

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The paper addresses an interesting topic from a network point of view, confirming previous results with this new data-driven learning methodology. However, I think that the authors should make more clear which is the added value of this methodology (with respect to using more traditional methodologies) and highlight which results are different from what was already known. In particular, the determination of the causality of the links seems to be the most important strength of the method, but this is not sufficiently stated. In addition, I find that the selection of some parameters (weekly time-span, CGTI region, 1 week time-lag, precipitation only over MT and not over all

the Monsoon area, etc.) are poorly justified and some discussion on how the results are modified with a different selection is necessary. Finally, some word on why the linear framework is adequate for the study if these mechanisms is missing.

Major questions/comments:

1. Which is the added value of the methodology with respect of using just correlations and partial correlations? I think that probably the values of the links could be reproduced just with more simple statistics but the causality determination is the highlight of the proposed methodology. This should be more clearly emphasized and showing the values of the correlations / partial correlations among all the selected variables (MT precipitation, MJO2, PC2, CGTI, etc.) is advised for comparison.

2. Which is the time-decay of the normalized causal effect among the different links? The authors only mention the results for 1-week lag but I think it's interesting to comment on how the intensity f the connections decays (or does not) with time.

3. How do the results change when seasonal or monthly time-scales are selected? As mentioned, the original DW2005 hypothesis was originally defined for the seasonal/monthly time-scales bu this time-scales are avoided in the current manuscript. Why? How do the results change?

4. The DW2005 is the base for this study, however it is only briefly discussed. I suggest to include a whole paragraph of the Introduction to discuss their findings more deeply. Also, a deeper discussion of DW2007 is missed. Those 2 studies are mentioned but together with other studies and, thus, their relevance and main results are difficult to identify.

5. The authors focus on mid-latitude and tropical links to the Indian Mosnoon. Can the authors identify any high-latitude link with the MT precipitation?

6. Why is only the MT region selected? Which are the results when selecting, for example the maximum precipitation over western India or eastern of 87E? How about

selecting the precipitation over all the Indian Monsoon region?

7. In all the network figures it would be useful to have the numbers indicating the path coefficient and auto-corr. path coefficient over the arrows and inside the circles, respectively. Absence of arrows indicates 0 path. Coefficient?

8. Why linearity is a good framework here? Please cite works to justify this.

Minor comments:

The terms “mid-latitudes” and “extra-tropics” are used indistinctly. This is problematic and in “extra-tropics” the high-latitudes are also included. Please only use 1 term to avoid confusing the reader.

line 125: space missing “)algorithm”

line 129: space missing “:It should”

line 130: what is an actor? A variable?

line 133: what is near-linear? Define.

line 134: PC algorithm means PC-MICI algorithm or other? Also the name is confusing as you used the term for Principal component before

line 149: which are the 2 conditions? More, generally: which are the n-conditions?

line 150: “parents contained in P^{n}_i” is n the same as before? I guess not, change for m

line 161: why is only tau=1 selected? Justify, why not look at other taus?

Line 176-180: why is the correction needed?

Line 183: how is “circumglobal wave train” defined?

Line 186: why is the NAO influence included in this subsection? The title only talks about ISM and circumglobal wave train

fig 1: panel c: add in the title that it’s over the MT region. Y-axis should be 10 to 10. Xaxis: it would be easier if it indicates the years

line 197: why start on the 2rd week? Justify and only show fig 1c starting at this time.

Line 199: justify why max lag 1 week is selected

line 243: typo “llink”

line 248-249: why the uncertainty in r? isn’t it just the pattern correlation number? Please explain.

Line 246-255: re-arrange paragraph to talk first about EOF1 and then about EOF2. Also, mention definition of eurasian sector the first time it appers.

Line 261-266: in Precipitation there is no signal over western Europe

line 269: Careful! How can you compare variables of different magnitude????!! you can’t say the precipitation is weaker than temperature.

Fig 2: CGTI has not been defined up to this point in the text. Why not show als lag + -1? seems important later. Why show temperature anomalies when it’s not the focus of the paper? Panels e and f: subtitles are misleading, indicate it’s anomalies associated with extreme CGTI.

Line 227-241: AT this pont CGTI seems important to the paper, however it is not shown. I suggest to include its time-series. Also a justification on why such a small region is selected is needed.

Line 316: OLR1 is not indicated in the figure 4b section 3.3: why add MJO in this section when it is about internal feedbacks? The sub-title is misleading

fig 7: why is the network overlapped on a lon-lat map? No overlapping was done before. For example, why is EOF2 located over east Asia or W1 over west India?

Line 446-447: you only mention mid-latitudes even though later tropics and internal feedbacks are analysed.

Line 459: how does your results have implications for the interannual time-scales? I think this sentence is misleading.

Line 464-465: how does this pattern compare with the regression of MT precipitation on the Z200 field? Any substantial difference?

Line 486-488: is it possible to confirm this with your results?

Lines 502-508: why is the linear framework adequate for studying these mechanisms? Can you cite any modelling work implying linearity of these type of interactions?

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