

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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This study attempted to identify and quantify the causal relationships between tropical/mid-latitude precursors and ISM rainfall on sub-seasonal timescale using the recently developed causal effect statistical tools. This topic is interesting and important due to the importance of the ISM in climate system and the urgent need of quantification of causal effects. However, there're still some major issues in this manuscript that prevent the acceptance for publication. The biggest problem is that the authors sometimes directly follow previous works' definition and analysis procedure without their own physical thinking.

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Major comments:

1. First if focusing on sub-seasonal variability, time filter is first needed to be applied to every dataset: either conducting band-pass filter like the MJO index or using similar time filter process in DW2007. Hypotheses in DW2005 and DW2007 are based on interannual and intraseasonal variability separately, whereas the results obtained in this study is based on weekly data that mix both sub-seasonal and interannual signals, which is very confusing. For example, the pattern in Fig.2d strongly resembles that in DW2005, which is probably due to the dominance of the interannual signal in the unfiltered time series. If the results with filtered data are largely different from that with unfiltered data, the authors' original idea of validating the hypothesis of interannual ISM-CGT relationships (from DW2005) may not be appropriate for this sub-seasonal study.
2. The definition of CGTI is confusing (lines 221). The CGT pattern (or index) defined in DW2005 is on interannual timescale, which is different from the intraseasonal Eurasian wave train pattern presented in DW2007. This study used the exact same region with the interannual CGTI used in DW2005, but the reason for why this region can also be used to define sub-seasonal CGT is not clarified. Again, maybe after filtering, the Z200 pattern does not resemble the CGT pattern and thus the index may not be called as "CGTI".
3. For the EOFs. First, how much of the total variance is explained by each EOF mode, and a test is needed to check whether the EOF modes are well separated from each other (refer to North et al. 1982 and DW2007). Then, comparing Fig. 2c and 2a/b, the Z200 pattern correlated with MT rainfall does not resemble well with the leading two EOF modes (although pattern correlation coefficient is statistically significant, the locations of centers are quite different), indicating this Z200 pattern is not the major mode of global Z200 variability. A suggestion is just focusing on the Eurasian sector (not necessary to link with circumglobal pattern) to conduct EOF, to find key region to define index, and to get wave train pattern.

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4. The “PCMCI algorithm and numerical example” section in SI should be added to the Methods in the main text (or replace the original long descriptions). Using equations is much clearer than the original wordy section 2.2 and 2.3. The definition and calculation of the path coefficient (or beta coefficient – unify the term please) should also be added to the Methods section.

5. In Figs.3,5,6,7, what’s the meaning of the magnitude of the autocorrelation coefficients? Why can the strength of causal links be expressed by autocorrelation coefficients (lines 257-258)? There’s no explanation of these autocorrelation coefficients in the main text.

6. There’s a causal arrow from NAO to CGTI in Fig.3, but Fig.4a does not show a NAO-like pattern that is correlated with CGTI. Is NAO a true precursor of the author-defined CGTI?

7. In Fig.5, what’s the physical meaning of the arrows from CGTI back to H1 and from L1 back to H1? Statistically, there may have these backward lead-lag correlations, but physically, I don’t understand how the change of CGTI/L1 can backward “cause” the change of H1/L1. If these backward arrows are spurious, how can we believe that the other arrows are true causal relations (considering that the authors claimed that their approach can remove the spurious correlations)?

Specific comments:

1. Considering MJO is not a tropical teleconnection, the title of this article may be changed to “Tropical and mid-latitude factors interacting...” (or some other word, not to use “teleconnection”).

2. When first using an acronym, a complete spelling should be written before this acronym. E.g., in line 146, “The PC-MCI algorithm” should be changed to “The Peter and Clark – Momentary Conditional Independence (PC-MCI) algorithm”.

3. Line 120: Simply explain why choose this box region to define the MT region (e.g.

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due to relatively large standard deviation from Fig. 1a). Considering in DW2005 and DW2007, their selected ISM region is to the northwest of this MT region, this again stresses that directly using the hypothesis from these two papers may not be appropriate.

4. Line 420 and Table S2: The explanation of ACE and ACS are not very clear. How large of the magnitude is significant (important)? If ACE and ACS are important expression of the causal effects, Table S2 can be added to the main text. The description of Table S2 writes “actors presented in Fig.8” should be “in Fig.7”.

Reference: North, G. R., T. L. Bell, R. F. Cahalan, and F. J. Moeng, 1982: Sampling errors in the estimation of empirical orthogonal functions. *Mon. Wea. Rev.*, 110, 699–706

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