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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 #1

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This paper tackles the important subject of teleconnections to the Indian monsoon, mainly from the midlatitudes although also with tropical elements included such as that of intraseasonal variability. Given the monsoon provides the majority of water for more than a billion people, better understanding of its teleconnections gives hope for enhancing predictions. The work uses the novel CEN technique among others to diagnose the pathways between the monsoon, the circumglobal teleconnection (Ding and Wang region) and further across Eurasia at weekly time scales. The subject matter on dynamical teleconnections is suitable for the ESD journal.

The paper is an important contribution to the literature and deserves to be published after some minor alterations. The paper could do more to mention previous literature such as that of Rodwell and Hoskins, which is important in explaining the mechanism for the link between the monsoon, central Asia (including the Ding and Wang region) and further afield to the Mediterranean. In addition, the treatment of what the paper describes as the internal dynamics of the monsoon is confusing. The authors should perhaps re-examine this explanation or make a judgement as to whether this can be better described as part of the monsoon intraseasonal variability. Note that in boreal summer, it would be more appropriate to refer to the monsoon ISO/BSISO rather than the MJO. The paper generally has some very well produced figures. Specific comments can be found below.

Specific comments Line 35: It is not clear what is meant by the "ISM convective cell". Is this some sort of mesoscale convective system or an individual cloud? I suggest that some alternative terminology is found. (See also later on line 39.)

Line 36: "Periods with strong updraft lead to strong rainfall one week later". Please clarify if this means locally or acting at some distance along the teleconnection.

Line 37: In your statement, "internal ISM dynamics has the strongest CE of 0.5", what does this mean in the context of the similar statement earlier that explains the meaning of these value? Effectively you are saying (unless I am misinterpreting the purpose of the CE value), "A one standard deviation shift in the internal ISM dynamics causes a 0.5 standard deviation shift in ISM rainfall one week later". It is not clear what you are meaning by this.

Line 43: In the introduction it may be worth also citing the work of Stephan et al. (2019, https://doi.org/10.1175/JCLI-D-18-0405.1) who use the CEN technique to examine the CGT/SRP link to the ISM, albeit in the context of decadal variability.

Lines 63-64: In the statement, "...this thermodynamic perspective [cloud cover etc. acting to cool the surface] is useful to understand the quasi-biweekly variations of the

ISM elements locally", why is the quasi-biweekly time scale of particular interest? In a monsoon regime wouldn't we expect CAPE to build up and be destroyed much more regularly than this, e.g. on a daily basis, given that the surface forcing is strong and there is a plentiful supply of moisture?

Line 77: Rather like the earlier comment, what is meant by a "convective cell" of the MJO here?

Line 87: Regarding "downstream", careful to specify what is meant. Do you mean downstream with respect to the jet, i.e. further east? This seems to be in the same direction as given in the previous sentence, rather than "on the other hand" as the sentence starts.

Line 166 onwards: the methods in this section are explained well given the complex techniques involved and the referencing is done very well.

Figure 2 and others in the paper are very inventive and generally of very good quality.

Line 205: How are the "northern mid-latitudes" used for the EOF calculation defined?

Line 205: "lag = -1 week". It would be better to clarify exactly which variable is leading the other, to avoid ambiguity.

Line 224: That the first three EOFs are not mutually separable is somewhat of a mathematical interpretation, but what does it mean in terms of any physical explanation of the EOFs?

Line 232: Perhaps it would be a good idea to add a further physical interpretation of the Z200 leading MT rainfall by 1 week. Presumably this is a west to east propagation of the signal.

Line 247: Does the global scale mentioned here imply that the correlation was performed over all global gridpoints? It seems a bit excessive and could probably just be done over the hemisphere.

Line 275: Here (and also for the benefit of later), please clarify that the lags of 1-week act in the direction of the arrows (e.g. CGTI leads MT by one week, and then MT feeds back on CGTI one week later).

Line 281: Consistency with Ding and Wang is mentioned here, but ultimately this also supports Rodwell and Hoskins (1996) on monsoon-desert coupling (https://doi.org/10.1002%2Fqj.49712253408). This seems to be a surprising omission from the paper given that it helps explain the relationship between the monsoon and Ding & Wang region, and ultimately further afield to the Mediterranean. It would be good to discuss this work in the introduction and perhaps see also the works of Cherchi et al. (2014; https://doi.org/10.1175/JCLI-D-13-00530.1, which examines the issues in coupled models, and also see the discussion of the Rodwell and Hoskins mechanism in relation to Ding & Wang in Beverley et al. (2019; https://doi.org/10.1007/s00382-018-4371-4).

In figure 4, panel (d) seems rather pointless. Can a composite difference of ISM rainfall not be given based on the precursors illustrated in the earlier panels of the figure?

Line 326: What do the black contours represent in the figure?

Lines 330-332: It may be worth citing some earlier works linking NAO and monsoon, e.g. Goswami et al. (2006, https://doi.org/10.1029/2005GL024803).

Line 348: The "Himalayan plateau" is not appropriate as it does not exist. Do you mean the Himalayas or the Tibetan Plateau (or both)? Better to think of a more appropriate term.

Line 351-355: Rather than the "ITCZ", isn't an interpretation of this that the one week earlier than strong rain over the trough, we have rain further south over India, such that we have a northward propagation of the BSISO? The rainband looks slightly titled rather than entirely zonal. That would be why you see a causal relationship of MJO2 to W1.

Lines 369-370: There was an explanation on the use of OLR earlier, so it isn't needed again.

Line 372: Here and elsewhere we are referred to the internal dynamics of the monsoon. Is this not perhaps better explained as the intraseasonal variability, in other words related to the BSISO of active and break phases?

Lines 373-374: The stronger MT rainfall followed by weaker ascending motions one week later is probably consistent with a monsoon active phase moving into a break.

Line 377-380: The negative feedback described here is logical in the sense that convective rainfall acts to stabilize the atmospheric column (and destroy CAPE). But in the monsoon regime, CAPE will quickly be reinvigorated given the surface forcing and good availability of moisture, possibly within a day. Rather than a negative feedback, couldn't one also argue that (as in Gill's off-equatorial heating), the LH release of the monsoon convection leads to a feedback and strengthening of the flow. What you are seeing here may instead be better explained as part of the migration between states of the BSISO.

Lines 454-455: Are the internal variability and the MJO-related part not somehow related, i.e. they are intraseasonal variability of the monsoon.

Lines 461-462: Monsoon-desert coupling could be mentioned here.

Line 478: The description here of internal variability dominating over interannual variability might be better explained in terms of intraseasonal versus interannual.

Lines 485-486: It would have been useful to have these discussions about the ITCZ migration earlier. This might be better explained in any case in terms of the switch between break and active phases of the BSISO, as the region of maximum rainfall propagates northward from the equatorial position during a break to Indian latitudes during an active phase.

Line 489: Here and elsewhere, the MJO is discussed but it may be better to think in

terms of the BSISO, which is the summer manifestation of the MJO with northward propagation.

Line 499 (and elsewhere): It would be better to expand acronyms such as RG-CPD or explain again what they are in the conclusions, for the benefit of the reader that goes straight to the conclusion section.

Spelling, grammar & other trivia Line 34: Change "influences back" to "feeds back on" Line 63: "in support of suppressing" is rather contradictory English. I suggest changing it to something like, "which tends to suppress convection". Line 76: Insert "The" before "MJO". Line 103: It would be customary for a few sentences here at the end of the introduction listing what sections are to follow in the remainder of the paper. Line 125: missing space before "algorithm". Line 188 and elsewhere: Perhaps put the year of the Pai et al. reference here and elsewhere. Line 208: In describing panel (e) it would be clearer to express this as a composite difference, e.g., "Composite temperature difference between weeks with...". Line 232: timescale –> timescales Line 240: patter –> pattern Line 243: llink –> link Line 262: As in the earlier comment, it might be best to explain as a composite difference. Line 318: "figure" for the new sentence should be capitalized. Line 479: Enables to –> enables us.

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