Review of

# A climate network perspective of the intertropical convergence zone

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**General**

The authors use 'functional climate network representations' to study the relation between the ITCZ and the SST field. The uniqueness of the approach is that, unlike previous studies that considered the climatological relation between these fields, the analysis based on climate network uses the internal variability of the SST field (based on monthly means). The analysis is performed on the Tropical Rain Belts with an Annual cycle and a Continent Model Intercomparison Project (TRACMIP), which includes 14 GCMs with idealized ocean components. Being based on idealized models, TRACMIP suffers from several limitations which are noted in the text. The analysis, therefore, serves as a 'proof of concept' which can later be applied to more realistic datasets. My understanding in complex network/cluster analysis is quite basic, which limits my ability to scrutinize the technical aspects of the study. My comments are therefore directed at the interpretation and scientific merit of the results.

The paper is well written and well organized. The clustering method and results seems to make sense. However, most of the results appear quite trivial (e.g., clusters with ITCZ close to the equator being also more hemispherically symmetric). The added value of the rather complicated novel analysis over more 'traditional' simple methods is either not present or not well communicated. Nevertheless, the authors do refer to the analysis as a first step, establishing the merit of the methodology before examining more broad applications in climate dynamics. In that sense, the consistency of the analysis with known results could be regarded as satisfactory. Some specific comments are provided below.

**Minor comments**

1.  It is known that the response of the tropical belt to extratropical SST perturbations lags by 2-4 months. It is not clear to me whether the effect of lagged response is included in the analysis. Since the analysis is based on monthly SST anomalies, it stands to reason that the analysis would be able to convey something about the nature of the lagged response — which at present is not well understood. But this is not discussed in the results.

2.  Again, I wonder whether introducing lagged correlations would affect the analysis of tropical vs. extratropical variations. It seems to me that the effects of tropical and extratropical SST anomalies on the tropical rain belt can be thought of as competing paradigms. Tropical SSTs affect the position of the ITCZ via local constraints, whereby the ITCZ resides over the warmest waters. Extratropical SST variations affect the global energy budget, causing the ITCZ to move toward the warming hemisphere. I don't see that the analysis captures this distinction.

3.  The 'failure' to diagnose distinctions between the models in response to global warming is somewhat consistent with the minimal zonal-mean ITCZ shifts seen in projections based on comprehensive climate models. The response of the tropical rain belt to global warming is mostly zonally asymmetric, an aspect that was not examined in this work.

**Comments by line number**

36    The energetic framework, as well as SST based arguments have been examined and found to be relevant for time-dependent variations, e.g., during the seasonal cycle (Adam et al. 2016) and in diagnosing potential sources of the double ITCZ bias (Adam et al. 2018). Perhaps this sentence can be clarified or replaced with simply stating that these frameworks are relevant for seasonal or longer climatologies.

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