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Earth System Dynamics
Discussions

Interactive comment on "A climate network perspective of the intertropical convergence zone" *by* Frederik Wolf et al.

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Response to Anonymous Referee # 1

We gratefully appreciate the generally positive impression of the reviewer regarding the presentation of our study. Furthermore, we thank them for the very interesting and useful suggestions for improving the coherence of the manuscript.

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.

- *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.*

  As also indicated by the reviewer at the end of their comment, our present analysis is meant to be a proof-of-concept that network approaches, which have not yet been applied for this purpose, can provide a tool for better understanding the climate dynamics of tropical rain belts. That is, we show that network approaches can offer a complementary perspective compared to well-established approaches, such as those emphasizing interhemispheric contrasts in SST and energy fluxes, as well as the energy input at the equator. In this sense, the fact that some of the results might appear trivial is at the same time reassuring.

  On the other hand, we also agree that the potential for added value should be more clearly articulated wherever possible, even if this potential is not yet realized in our present work. One example is the fact that the networks based on intratropical connections only fail to capture model differences in the ITCZ position in the control climate (line 295 of the submitted manuscript). In fact, this is different from the well-established approaches mentioned above, which in the aquaplanet context rely on zonal-mean quantities, and indicates that zonal variations, such as those generated by tropical waves or local SST patches can play an important role even in the aquaplanet setup. Another example is the possibility to expand the methodology so that the networks include other fields or represent lead-lag relationships between tropical and extratropical SST (see below).

  We will revise our manuscript to clarify the aim and scope of the present work, and to highlight potentials for future work.

- *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 âĂŤ which at present is not well understood. But this is not discussed in the results.*

  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.

  In the following, we will answer both comments together as they are closely related.

  Our network analysis is solely based on instantaneous correlations between tropical and extratropical SSTs. The fact that extratropical SSTs and the ITCZ position are out of phase is an interesting direction for future work that will be pointed out more clearly in the revised discussion and conclusion section. However, studying this aspect seems not trivial, as one would for example need to decide how to blend the phase shift between tropical and extratropical SST anomalies when constructing the correlation matrix for the network analysis.

  (As a side note, despite an extensive literature search we were unable to find studies that explicitly show that the ITCZ lags extratropical SST changes by a couple of months. While this is plausible intuitively, we would gratefully appreciate if the reviewer could point us to specific studies on this subject. Our own search only resulted in studies that emphasized the time-mean response or responses beyond 1 year after the perturbation. Also, in observations the ITCZ

  leads extratropical SST over the course of the seasonal cycle, as shown, e.g., in Fig. 6 of Chiang and Friedman, Annu. Rev. Earth Planet. Sci. 2012. 40:383–412.)

- *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.*

  We agree that in comprehensive models with realistic present-day boundary conditions, the zonal variations in the tropical rainfall response to warming can make it difficult to extract a meaningful zonal-mean response. However, this issue should be circumvented in the TRACMIP models as their aquaplanet boundary conditions are zonally symmetric. The 'failure' of our network approach to distinguish model differences in the response must thus have a different origin and indicates that the climate change response of the SST networks is *not* tightly linked to the ITCZ climate change response. Although the reasons for the 'failure' remain unclear to us, one possibility might be that unravelling the climate change response would require a different network representation that involves other atmospheric fields in addition to SST, e.g., changes in the vertical profile and gross moist stability of the tropical atmosphere, which have been shown to be able to play an important role. We will more clearly articulate these points in the revised manuscript.

- *Line 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.*

Thanks, and fully agreed! We will adapt the sentence accordingly so as to make clarify this point and to properly characterize the work of Adam et al. (2016, 2018). We will also revise the manuscript to clarify that our network approach links spatial correlation patterns of the global SST field to the time-mean ITCZ position. This should avoid any confusion regarding the fact that the 'traditional' approaches links the time-mean SST field to the time-mean ITCZ position, where the time-mean can be a seasonal mean or a longer time mean.

- *Typo CO2.*

  We will correct this typo in a revised version of our manuscript.