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Discussions

Interactive comment on "An interaction network perspective on the relation between patterns of sea surface temperature variability and global mean surface temperature" *by* A. Tantet and H. A. Dijkstra

Anonymous Referee #2

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Review of esd-2013-25, An interaction network perspective on the relation between patterns of sea surface temperature variability and global mean surface temperature, by Tantet and Dijkstra.

This paper is overall well written, addresses relevant scientific questions within the scope of ESD, uses novel concepts, and reaches an important conclusion: the impact of ENSO on global SST is over-represented by the MPI Earth System Model (ESM) whereas the impact of the AMO (presumably a proxy for variations in the strength of the Atlantic Meridional Overturning Circulation, AMOC) are under-represented. I

suspect this weakness in the representation of the main aspects of SST variability will be found in most other ESMs.

The paper, as written, is highly mathematical and unfortunately is hard to wade through. Much of the language is technical and much of the paper is borderline "clear only if known". The abstract, as written is very weak: it fails to convey most of the main findings of the paper but rather serves more like an initial introductory paragraph.

Nonetheless: I rate the paper "Good" in all of the categories and suggest it could be accepted for publication provided the points below are addressed.

Even though I checked the "minor revisions" box, I think properly addressing this Major Point below is likely to involve a level of work between a major and minor revision. I urge the authors to revise the paper in light of this comment, rather than "rebut" the comment in a length reply that does not lead to much (or any) change in the paper.

Major point:

1. The two weakness of the paper are:

a) all of the analyses are conducted for zero lag (line 20, page 747) b) the analysis is restricted to 50S to 80N

There is a statement on the top of page 748 that reads:

Even though the links in the network are based on SST correlations at zero lag, a connection is not only representative of instantaneous relationship, since the time series are 13 months low-pass filtered.

I am nonetheless quite concerned that, despite this off-hand statement, the paper may be lacking physical meaning due to the assumption, throughout, of zero lag.

Most analyses that related ENSO to either global land surface temperature, global ocean surface temperature, or global mean surface temperature assume some type of lag. See Table 1 of Foster and Rahmstorf, Global temperature evolution, 1979–2010, Envir. Res. Lett., 6, 044022, doi:10.1088/1748-9326/6/4/044022, 2011 for instance. Use of a 13 month low pass filter is unlikely to properly represent what known time lapses in the global extent of ENSO.

Physically, it takes months for the ENSO (or AMOC) to exert an influence on other parts of the world. Also, the AMOC is expected.

Also, the Hadley SST record extends further south than 50S. The restriction to 50S to 80N is therefore hard to understand. As stated in Srokosz et al., Past, present, and future changes in the Atlantic meridional overturning circulation, BAMS, 93, 1663–1676, 10.1175/BAMSD-11-00151.1, 2012:

A weakened AMOC is typically accompanied by a slight warming of the Southern Hemisphere, though details differ between models.

It is incumbent that the authors address the physical limitations of their otherwise exhaustive analysis due to the choices to show results only for zero lag and to restrict SST to 50S to 80N.

Minor points:

1. Please re-write the abstract to reflect the findings of the paper in the language that most earth system modelers, who know nothing about network theory and page ranks, can understand. For example, the thoughts on lines 16 to 19, page 759, should be reflected in the abstract.

2. Two recent important papers on the AMO and the AMOC should be cited.

Srokosz et al., Past, present, and future changes in the Atlantic meridional overturning circulation, BAMS, 93, 1663–1676, 10.1175/BAMSD-11-00151.1, 2012 provide a nice overview of how North Atlantic temperatures may serve as a proxy for variations in the strength of the AMOC. The submitted paper lacks any physical discussion of the AMOC: this should be added upon revision, with a reference to Srokosz et al. (2012) or perhaps to Srokosz et al. (2012) and references therein.

Canty et al., An empirical model of global climate – Part 1: A critical evaluation of volcanic cooling, ACP, 13, 3997–4031, doi:10.5194/acp-13-3997-2013, 2013 was published (18 April 2013) before Muller et al. (2013) was even submitted (22 April 2013). Canty et al. (2013) (their Figure 9 and related discussion) showed “that the variability of global land surface temperatures (GLST) is strongly connected to the AMO”. This paper must be cited on lines 10 to 12 of page 745, and perhaps elsewhere. Indeed, even though the manner of presentation of the submitted paper is starkly different than the presentation in Canty et al., it seems these two papers reach rather similar conclusions for the analyses that overlap.

3. The phrase “qualitatively identical” on page 747 is an oxymoron. Please consider different wording.

4. Figure 4 is first mentioned on line 13, page 753. At this stage, however, Figure 3 has not yet been referenced. I assume ESDD expects figure numbers to reflect the order of reference in the paper.

5. For the record, I doubt the statement on page 758 “Also, the phase synchronization of the time series of both communities has contributed, to a large extend, to the increase in GMST since the 1970s” would survive serious scientific scrutiny because the analysis appears to be conducted entirely using linear detrending of the various time series. Yikes! The radiative forcing of climate due to rising GHGs and anthropogenic aerosols has not been linear except for very short time periods. Ideally, this entire analysis should be repeated using a proxy for anthropogenic RF of climate to detrend the various data sets, rather than a linear term, as has been done in several studies including the above mentioned Canty et al. paper.

I am not however actually suggesting the detrending be handled differently at this stage, as this would be a timely prohibitive suggestion. I am however stating, for the ESDD record, skepticism about this sentence.

6. Line 3 of page 759 ends “to the GLST”. Is this correct ?!? I think perhaps GOST is

what was meant.

7. The phrase “are orphan” appears on page 759. I do not know that this phrase means in a scientific context: please consider different wording.

8. Lines 6 to 10 of page 762 state “First, the maximum cross-correlation . . .” Would be helpful if a figure supporting this sentence could be added, either to main or a brief supplement.

9. Line 3, page 767, “Muller, R. a.” should be “Muller, R. A.”

10. The tables would be easier to understand if the names of the first few communities could be given in the words that appear above each table.

11. Unless “orphans” is common terminology in the networking literature, I suggest using another word such as “unassigned” for the caption of Figure 2.

Interactive comment on Earth Syst. Dynam. Discuss., 4, 743, 2013.