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Earth System Dynamics Discussions
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Interactive comment on "Causal dependences between the coupled ocean-atmosphere dynamics over the Tropical Pacific, the North Pacific and the North Atlantic" *by* Stéphane Vannitsem and Pierre Ekelmans

Anonymous Referee #2

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The paper discusses the use of a relatively new method to infer causal dependencies in climate dynamics. While the subject is of profound interest I am not convinced about the contributions of this paper. In short, the method is introduced as an ad hoc method, its description is confusing, and the results cannot be reproduceed as crucial information is missing. Also, the conclusions are not fully backed up by the results. I suggest either a major revision or rejection, based on arguments detailed below.

1. There is a quickly growing literature on causal inference methods based in information theory. The authors provide one sentence on page 2 lines 21-23 on it, saying

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something like 'finding a good estimator for analyzing real data is difficult'. I have no idea what this is about, and see no reason why the information theoretic methods could not be used here, even with the time series used in this paper. Please provide a proper argument on why these methods cannot be used, and why the proposed CCm method is better/more suitable. My fear is there is no such argument.

2. section 2: The authors talk about phase space while they mean state sspace. A phase space is a position-velocity space, which is of interest when describing the motion of particles, but not for fluid dynamics, which is governed by first-order time derivatives. Please correct.

3. The distance measure is a crucial ingredient in CCM as far as I understand it, yet no discussion is provided. Saying that typically the Euclidian distance is used is not useful, more discussion is needed. This is related to the next point.

4. The weights in eq 2 seem to be completely arbitrary. First I assume that the authors use something like exp(-d_j/dmin), otherwise the whole procedure doesn't seem to make sense. But that might be just a typo. More importantly is why this is a good weighting, also relation to Euclidian distances. As far as I can see this is completely ad hoc. At least a justification for this choice of weights needs to be given. Furthermore, the question immediately arises how sensitive the results are to form of the weights. This is not discussed. Please do!

5. The prediction defined by eq 1 depends on which analogs have been used, so on the prior sample. How large is the sensitivity of the results to that?

6. The CCM methods starts out strong with the embedding, but then falls back to correlations, with all weaknesses that the paper wants to avoid in the first place. For instance, if X(t) = sin(omega t) and y(t) = X(t - pi/(2 omega)) the relation between X and Y is circular, so the correlation in eq 3 is zero. This, however, would negate the existence of a very strong causal relation.

7. As far as I can see, if the causal relation between X and Y peaks strongly at say 2 tau adding more elements in X will not increase the correlation, it might even make the correlation smaller. But the authors say differently in e.g. p4 lines 19-25. Please clarify.

8. section 3. The authors assume that the main ocean dynamics is governed by Sverdrup balance in mid latitudes. This might be true for the large-scale dynamics, but is not so for scales smaller than a few 100 km. Are the main conclusions of the paper justified when all dynamics of the ocean beyond Sverdrup are ignored? For instance, isn't is essential for the North Atlantic circulation and the interaction with the atmosphere, e.g. via the storm tracks, that the GulfStream extension is not zonal? I think the authors should at least justify much more strongly why they think they can use this simple mode structure to project the reanalyses data sets on.

9. The authors use the monthly fields for the analysis. Are these snapshots or monthly averages?

10. What was dmin for each time series?

11. The authors calculate a CI, and there is a growing tendency in the science community to eradicate its use because of its arbitrariness, among bother problems. Can the authors instead provide error estimates on their results?

12. It is important that the analogs are independent from each other. How did the authors ensure that? Please at least discuss this.

13. Please make the legends of all figures larger, and the error estimate lines thicker.

14. Fig 3 © and (e) should be interchanged(?)

15. What happens for low time lags in fig 4f?

16. Please describe the distribution of the random anomalies in page 13 line 9.

17. page 13 lines 18-24. I don't necessarily agree with the authors' conclusion on the fact that the annual surrogates give the same correlations as the full time series. This

does not mean that there is no underlying dynamical causal structure, only that the annual signal is dominant. One has to be careful, as the annual signal is very, very strong. While the authors argue that they don't want to try to remove the annual signal from the time series because of the issues with doing that, perhaps they have to do it this time, e.g. by using different methods and compare results.

18. section 4.2. I'm not sure one can say that ORA-20C is more reliable than ORAS4. I understand from their description that both do not assimilate ocean variables, they only differ in their atmospheric forces. But the ocean circulation is not only determined by the surface forcing, but also, and perhaps to a much larger extend, on the initial condition and on model biases. How do the initial conditions differ between the two ocean products, and assuming they are both initialised from a smoothed observational data set of T and S observations, could it be the case that the ORA-20C run has a less realistic ocean vertical structure because ocean model biases have degraded that longer compared to ORAS4?

19. The conclusions are too strong given the CCM method used, the high simplification of the Earth system dynamics to a few in my view quite unrealistic modes, apart from the other potential problems mentioned above. The first conclusion especially, see point 17 above, but also the 4th conclusion. The authors seem to miss the possibility that the atmospheric forcing needs some time to give visible changes in the ocean, e.g. the gyre circulation needs a few years to adapt to a change in the wind stress. It is not necessarily ocean only dynamics that sets time scales of over a year.

20. The fact that the CCM method is useful for the simplified model in the appendix does not provide a trong argument that it will work for the real ocean as the simplified model does indeed only have a few modes, while the real Earth system has many, with interactions that are much more complicated and more nonlinear. Please provide a stronger justification on why this experiment backs up the analysis on the far more complicated real system.

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