Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2018-3-AC2, 2018

Earth System Dynamics Discussions

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

Stéphane Vannitsem and Pierre Ekelmans

svn@meteo.be

Received and published: 30 March 2018

This document provides the detailed answers to the remarks and questions of the Reviewer. The points of the Reviewer are italicized, and the (proposed) modifications in the text are in bold face. The pages and lines refer to the revised manuscript in which the corrections have been introduced (in red) and provided as a supplement.

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 reprodueced 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.

We thank the reviewer for her/his detailed comments. We are however surprised by the tone of this general comment which contrasts with the content of the specific points below. These specific points can be easily clarified as you will see 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 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.

One of the present authors used a technique developed by Liang (2014) to compute the dependences based on information flow. This technique has been built assuming that the system is linear, and Liang (2014) found experimentaly that this approach could work for nonlinear systems as well. The estimator in this case is a combination of correlations and derivatives of correlations. This approach has been extended by introducing a normalization (Liang, 2015). One problem pointed out by the Liang (2014) is the fact that several choices can be made for the discretization of the derivative. Another aspect is that one cannot have dependences without correlation. The latter situation seems surprising since some systems (like the one pointed out by the reviewer in his/her remark 6) can display dependences without correlation. In view of these difficulties, we decided to postpone the investigation based on these approaches to a future work.

We understand that our comment in the mansucript is not based on sufficiently firm grounds, so we modified it in the following way (page 2, line 26):

Alternative approaches based on the transfer of information are very appealing and a lot of progresses have been made in that direction (Liang and Kleeman, 2005, Runge t al, 2012, Liang, 2014, Liang, 2015). We however do not pursue in that direction and let the use of these techniques for a follow-up study.

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.

Well the phase space is a terminology which is not only valid for position-velocity space. It is used in general to describe the abstract space spanned by the coordinates which are the variables of the dynamical system themselves, and it is used interchangeably with the other terminology of "state space". I refer to classical books like Nicolis (1995) or Broer and Takens (2011). We do not see why we have to change the terminology.

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.

We have modified the sentence by expliciting the distance used. Other distances can be used. The text added at page 3, line 32:

$d_i = \sqrt{\sum_j (X_j(t) - X_{j,i}(t))^2}$ **where** $X_j(t)$ **and** $X_{j,i}(t)$ **are the (delay) coordinates of the reference point and the ith analog, respectively. Other distances could be used.**

4. The weights in eq 2 seem to be completely arbitrary. First I assume that the authors use something like $exp(-d_j/d_min)$, 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!

You are right. Thank you very much for pointing out that. Now the weight given in Eq 2 is modified.

The development of this type of nonlinear forecasting traces back to several seminal papers, see for instance (Casdagli, 1991, Elsner and Tsonis, 1992) for a detailed discussion. In its original version, more general weights w_i were proposed that should be fitted through a least square approach (Casdagli, 1991). (Sugihara and May, 1990) proposed to simplify the approach by using a simpler variant based on exponential functions depending on the distance between the analogs and the reference point. This weighting penalizes analogs that are far from the reference point, and the normalization by the minimum distance allows for having weights based only on the relative distance. This technique works well as discussed in (Sugihara and May, 1990). Moreover it does not need any additional parameter, implying that the approach is parcimonious.

We have added in the manuscript at page 4, line 11:

The development of this type of nonlinear forecasting traces back to several seminal papers, see for instance (Casdagli, 1991, Elsner and Tsonis, 1992) for a detailed discussion. In their original versions, more general weights w_i **were proposed that should be fitted through a least square approach (Casdagli, 1991). (Sugihara and May, 1990) proposed to simplify the approach by using a simpler variant based on exponential functions depending on the distance between the**

analogs and the reference point. This weighting penalizes analogs that are far from the reference point, and the normalization by the minimum distance allows for having weights based only on the relative distance. This technique works well as discussed in (Sugihara and May, 1990). Moreover it does not need any additional parameter, implying that the approach is parcimonious.

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?

This is true, and in order to evaluate the uncertainty associated with the choice of the sample L, we have performed a large number of other random sampling and provide the mean value and a standard deviation as a measure of uncertainty. We added a paragraph on that at the end of Section 2 (page 6, line 1):

Note that in order to evaluate the impact of the random sampling of L events in the datasets, we can repeat the sampling a certain number of times and infer a mean (or a median when strong assymetries are present) and a standard deviation. In the experiments that will be described below, this approach is adopted and each correlation value is estimated over a large number of samples.

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.

It seems that the reviewer is confusing the correlation between X and Y, and the correlation between $\hat{Y}$ and Y, where $\hat{Y}$ is the estimate based on the X attractor. This correlation is a measure of the quality of inference which has been used for a long time

now, and which is still used to evaluate weather forecasts. This measure is related to the error between the inferred situation and the observed one.

Coming back to the example given by the reviewer, what is matter is the inference $\hat{Y}$ based on X and what we get with this circular solution is a correlation between $\hat{Y}$ and Y very close to 1 (and not equal to 0). So a kind of perfect inference. Now there is no increase as a function of L due to the fact the same source of information is present in both, the signal X.

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.

Well it seems that the reviewer is confusing the approach based on delay embedding and the approach adopted here in which contemporary data are used. We add the term 'contemporary' in the description of the approach at page 5, line 24.

Note that even in the delay embedding method, the correlation coefficient between Y and $\hat{Y}$ should not decrease. The added elements in X are points of the form X(t), X(t-τ), X(t-2τ)...where the delay τ is chosen specifically to optimise the estimate $\hat{Y}$. In the case where Y does not force X, the added information in the dynamics of X do not improve the estimate of$Y$. In the case where Y forces X, this added information will improve the estimate. There is however no reason the estimate should worsen as L increases.

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.*

Thank you very much for the comment. indeed the projection used here is meant to investigate the large scale dynamics and the link between the different ocean basins. There is by no means any aim at providing or using information at smaller scales. We add a paragraph on that (page 7, line 4),

This approach of reducing the dynamics of the ocean and the atmosphere to a few spectral large-scale components may at first sight look arbitrary. However for the two midlatitudes basins these modes possess the largest amplitudes (Vannitsem and Ghil, 2017), and for the Tropical Pacific, it is known that these large scale flows are strongly affected by the interaction between the ocean and the atmosphere. Moreover we are interested in the basin scale interaction between midlatitudes and the tropics. If such an interaction exists, we expect that these should be visible through the analysis of these large-scale fields. Now it is clear that these specific variables do not represent the whole dynamics, and additional analyses with more modes is worthwile, in particular to see what is the role of the main currents present in the ocean like the Gulf Stream or the Kuroshio current.

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

Yes it is monthly averages. We have clarified that at line 1, page 8, and in the caption of Figure 1.

10. What was dmin for each time series?

The value of d_{min} changes for each reference point (on the attractor projection) in the sample of size L, since it is the minimum distance among the analogs around the reference point. And of course it also changes with the sample chosen. There is no specific value of d_{min} for a series. To clarify that, we have modified the description at line 10 of page 4 as

The quantity $\min d_j$ denotes the minimum of d_j of the $j = 1, .., E+1$ analogs around the reference point $\vec{X}(t)$.

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 other problems. Can the authors instead provide error estimates on their results?

This is what we have done by randomly selecting new samples of L events a certain number of time and applying the CCM algorithm again. This provides an error estimate of the possible values of $\rho(L)$. We translated that in a CI using the mean and the standard deviation. This is now explained at the end of section 2 (page 6, line 1):

Note that in order to evaluate the impact of the random sampling of L events in the datasets, we can repeat the sampling a certain number of times and infer a mean (or a median when strong assymetries are present) and a standard deviation. In the experiments that will be described below, this approach is adopted and each correlation value is estimated over a large number of samples.

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

We tested different period of exclusions when selecting the analogs. We chose 48, 24 and 12 months. Since there were no substantial differences in the results, we decided to keep a minimum of 12 months between analogs. A sentence has been added at page 10, line 15:

Note also that for this analysis, the analogs have been selected to be at least separated by a period of 12 months. Other exclusion periods have been used without substantial differences.

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

We have increased the size of the legends.

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

No but the text should be corrected. We have done that at the beginning of page 12.

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

We have now represented the full curves.

16. Please describe the distribution of the random anomalies in page 13 line 15.

These random anomalies were simulated assuming a gaussian distribution around each monthly mean. The variance of the distribution is estimated from the anomalies of the corresponding month for all years in the datasets. the information has been incorporated at page 14, line 14:

These random anomalies were simulated assuming a gaussian distribution around each monthly mean. The variance of the distribution is estimated using the anomalies of the corresponding month for all years in the datasets.

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.

Thank you very much for this interesting point. We have analyzed using the CCM the monthly anomalies discussed above. The results are displayed in Figs. 1 and 2 at the end of the present response. This analysis confirms what was said when analyzing the surrogates.

One very interesting result is the fact that there is no influence between the anomalies over the Tropical Pacific and the North Atlantic for both reanalysis datasets. See panels (a) and (c) of each figure. These results are now incorporated in the text, together with the figures in an Appendix B in the revised manuscript provided as a supplement, as follows,

In section 4.1, at page 14, line 32:

Finally to further test the robustness of these results a complementary way to clarify whether montlhy anomalies between different basins are indeed related to each other is to apply direcly CCM on these anomalies. The results are displayed in the Appendix B, Fig. B1. The same conclusions are reached with the absence of dependences between the variables over the North Atlantic basin and the Tropical Pacific, and a strong mutual dependence between the ocean dynamics over the North Atlantic and the North Pacific.

In section 4.2, at page 16, line 33:

Finally for the sake of completeness, the application of the CCM to the monthly anomalies is displayed in Fig. B2. Here as for the ERA-20C/ORA-20C dataset, no link between the anomalies of the Tropical Pacific and the North Atlantic is found (panels (a) and (c)). An important difference is however visible on the influence of the North Pacific ocean temperature on the North Atlantic and on the Tropical Pacific (panels (b) and (d)). Again this contrasts with the results obtained with the other reanalysis dataset.

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?

This paragraph has been modified (page 16, line 24) as,

We may conjecture that the ORA-20C reanalysis data set is more reliable since a more consistent atmospheric forcing has been applied and the ocean model has gotten more time to equilibrate around its climate. But care should be taken here is drawing definitive conclusions on that. A better approach to disentangle which of these reanalyses provide the correct answer is to investigate a full coupled ocean-atmosphere reanalysis obtained for the whole 20th Century.

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.

The conclusions have been amended. First a new item is introduced mentioning the different results for the coupling between the North Pacific and the other basins obtained with the two different ocean reanalysis (page 19, line 21),

The results presented here for the two ocean reanalysis datasets disagree on the nature of the dependence between the North Pacific basin and the other ones. The ORA-20C indicates a dynamical influence (transport), while the ORAS4 suggests more an influence dominated by the ocean temperature. This difference is probably related to the specific data assimilation setup used in each case. To clarify what is really at work here new reanalysis datasets are needed. We can conjecture that the most reliable one should be built by using a coupled ocean-atmosphere data assimilation system running continuously on the longest period possible.

and also the last item has been modified as follows,

The inter-dependences between the North Atlantic and the North Pacific on longer time scales than a year seems to be important, and is probably related to the coupled ocean-atmosphere dynamics on long time scales. One could conjecture that the thermohaline circulation should play a role on this link. Additional analyses with longer data sets, with climate model runs, but also with the analysis of additional basins like the Tropical Atlantic or the Indian Monsoon region are necessary to clarify this role.

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.

Well if it does not work in such a case then there is no reason to think that it will work on real data. This is why we did it, to get some confidence in the method. Now the application to more sophisticated systems is planned and we add a comment on that at page 5, line 31:

It provides some confidence in the CCM algorithm, but we should keep in mind that the system explored in Appendix A is relatively simple and the application of CCM on more sophisticated climate models is worth performing. This is left for a future study whose results will be compared with the ones of the present analysis.

References

Broer, H., and F. Takens, *Dynamical systems and chaos*, Springer, New York, 2011.

Casdagli, M., Chaos and deterministic versus stochastic nonlinear modeling, Santa Fee Institute Working Paper, 1991-07-029, 35pp, 1991.

Elsner, J.B. and A.A. Tsonis, Nonlinear Prediction, Chaos, and Noise, Bull. Am. Meteorol. Soc., 73, 49–60.

Liang X.S., Unraveling the cause-effect relation between time series, Phys. Rev. E, 90, 052150, 2014.

Liang X.S., Normalizing the causality between time series, Phys. Rev. E, 92, 022126, 2015.

Liang X.S. and R. Kleeman, Information transfer between dynamical system components, Phys. Rev. Lett., 95, 244101, 2005.

Nicolis, G., *Introduction to Nonlinear Science*, Cambridge University Press.

Runge, J., J. Heitzig, N. Marwan, J. Kurths, Quantifying causal coupling strength: A lag-specific measure for multivariate time series related to tranfer entropy, Phys. Rev. E, 86, 061121, 2012.

Sugihara, G. and R. May, Nonlinear forecasting as a way of distinguishing chaos from measurement error in time series, Nature, 344, 734-741.

Please also note the supplement to this comment:
https://www.earth-syst-dynam-discuss.net/esd-2018-3/esd-2018-3-AC2-supplement.pdf

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2018-3, 2018.

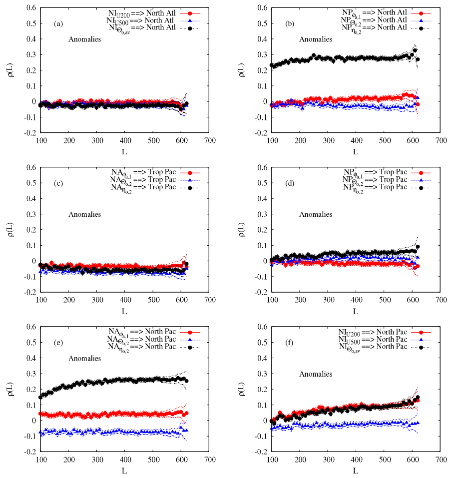

Figure 1: CCM as a function of the length L of the samples, as obtained from the anomaly of the monthly time series displayed in Fig. 1 for the reanalyses ERA-20C and ORA-20C. Each line with symbols corresponds to the influence of one variable on a specific coupled ocean-atmosphere basin. The specific variables are denoted in the labeling of each line in each Panel.

Fig. 1.

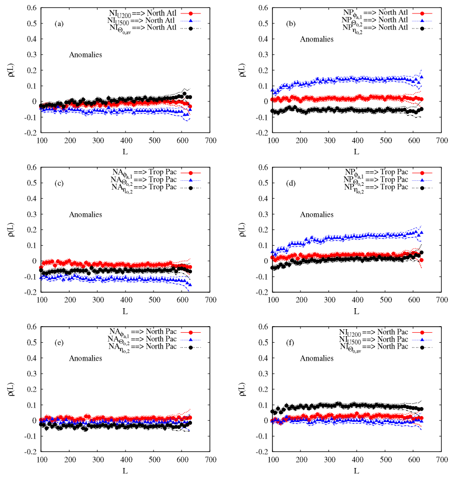

Figure 2: CCM as a function of the length L of the samples, as obtained from the anomaly of the monthly time series displayed in Fig. 2 for the reanalyses ERA-20C and ORAS4. Each line with symbols corresponds to the influence of one variable on a specific coupled ocean-atmosphere basin. The specific variables are denoted in the labeling of each line in each Panel.

Fig. 2.