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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

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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.

In this manuscript the authors investigate causal relationships, in monthly to interan-

nual time scales, between the climate dynamics of three ocean-atmosphere basins (The North Atlantic, the North Pacific and the Tropical Pacific region) using three reanalyses datasets (ORA-20C, ORAS4 and ERA-20C). The applied methodology, Convergent Cross Mapping (CCM) has been applied to other systems, but has not yet been used (to the best of my knowledge) to investigate climate causal relationships. I found this study very interesting. As the authors acknowledge in the introduction, unveiling causal relations is a very challenging task, and different methods (also depending on the choice of variables), are likely to yield different results. Here the CCM method is well motivated and described, and also the datasets used for reconstructing three- dimensional attractors are well justified. The results obtained are sound. Therefore, I am happy to recommend the acceptance of this manuscript, after the authors have taken the following points.

We would like first to thank the reviewer for her/his positive support to this work. Below we answer her/his specific points.

1 By using three time series, the authors reconstruct three-dimensional attractors (instead of using one time series and Takens' delayed coordinates). Could the authors discuss how important is the method used to reconstruct the attractor and the chosen attractor dimension? What could be expected if (i) two-dimensional attractors are reconstructed from two time series (instead of three, using, e.g., only the zonal velocity at either 200 or 500 hPa and the ocean temperature)? and (ii) three-dimensional attractors are reconstructed from one time series using Takens delayed coordinates?

The motivation of this approach is to avoid the difficulties associated with the embedding and the delay. Now the choice of three variables for the Atlantic and Pacific is motivated by the results we have obtained in previous works on the low-dimensional modelling of the coupled ocean atmosphere system, in which we have found that these three variables dominates the dynamics (Vannitsem et al, 2015; Vannitsem, 2015).

This 3D space constitutes a kind of projection of the full phase space. For the Tropical Pacific, the choice is motivated by the importance of the surface temperature and the zonal wind in the development of the coupled ocean-atmosphere dynamics. For the latter region we could indeed imagine to use only 2D maps.

(i) Using 2D maps, useful results can also be obtained. An example is given here for the influence of the Tropical time series on the North Atlantic in Figure 1. In the different results presented in this figure, the only influence which emerges is from the atmospheric Tropical observables to the 2D map defined by the Geopotential at 500 hPa and the Ocean Temperature. But the growth as a function of L is weaker than the one obtained when using the 3D map (Fig 3a of the manuscript). As mentioned by Sugihara et al (2012), the variation of ρ with L is faster before saturation when the strenght of coupling is higher. Here the coupling between the variables when a 2D map is used seems to be less important than when using the three variables, suggesting that the Tropical Pacific atmosphere indeed influence the coupled ocean-atmosphere system as defined by the three observables. The latter result suggests that the dependences between these regions is better elucidated based on the three dimensional space. Care should however be taken here drawing definitive conclusions on this comparison since the setup of the analysis has considerably changed with a change of phase space dimensionality and a reduction of the number of analogs used (only three in this case).

In the text, we add at page 11, line 5:

Note that the reduction of the phase space coordinates associated with $\vec{X}$ from three to two also provides interesting results with a dominating influence from the atmospheric Tropical Pacific observables on the two-dimensional projection $(NA_{\Psi_{a,1}}, NA_{\theta_{o,2}})$. However the increase before saturation of $\rho(L)$ is much more limited than when using the three observables to build the North Atlantic pro-

jection of the attractor (not shown). The latter result suggests that the dependences between these regions is better elucidated based on the three dimensional space.

(ii) When building attractors using the Takens' theorem, one needs to define an embedding dimension and a delay τ. Estimating the embedding dimension based for instance on the estimates of the correlation dimension of the attractor is very challenging when the expected embedding dimension is high since the approach needs to select close analogs to work properly (e.g. Kantz and Schreiber, 1995). It therefore needs very long time series that are usually not affordable (Van den Dool, 1994, Nicolis, 1998). So a way to overcome this problem is to increase progressively the embedding dimension and see whether the results are robust or not. For the delay τ, one usually uses a time period for which successive situations become sufficiently decorrelated, but not too much. Different methods are usually proposed to evaluate this delay, for instance based on decorrelation times, or simply by trial and error (e.g. Casdagli, 1991; Parker and Chua, 1989). In the present cases these delays should be relatively short for the atmosphere, but much longer for the ocean as it can be guessed by inspecting the time series of the right panels of Figures 1 and 2 of the manuscript. For the latter we are therefore facing an important problem since the decorrelation time (or delay) is not substantially smaller than the length of the time series. In the present context, we therefore opt for another approach based on selecting a subset of variables, a projection on a low-dimensional space.

We have modified the text after line 14 at page 5 to clarify point (ii):

Estimating the embedding dimension based for instance on the estimates of the correlation dimension is very challenging when the expected dimension is high since the approach needs to select close analogs to work properly (e.g. Kantz and Schreiber, 1995). It therefore needs very long time series that are usually

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We also added a sentence explaining what information can be extracted from the growth of $\rho(L)$ at page 5, line 4.

Another important behavior of $\rho(L)$ as a function of L is that the rate of increase is related with the strength of coupling.

2) The authors state that "If there is a causality relation of Y on X, "ro" (Eq. 3) will increase with L". However, what this study uses (and I am not sure is always true) is the fact that "an increase of "ro" with L reveals/uncovers a causal relation of Y on X". Could the authors discuss the limits of validity of these two statements? I assume they hold if "L" is appropriated (not too small, and not too close to the length of the dataset). How about if X and Y are both driven by Z?

This point is one key element of the method. The main hypothesis behind these statements is that when extra information (larger L) is present, then one should expect to get better analogs around X and therefore to get better knowledge on the correct value of Y. This has been amply demonstrated on different simple systems by Sugihara et al

(2012), and also by others. Note also that in an ideal context where the attractor can be reconstructed with precision and for L going to infinity, the correlation should converge to 1. In practical situations, this precision and the asymptotic limit are never reached. The convergence is then limited to a certain level by the presence of observational error, the approximation of the dynamics (like when a low-dimensional approximation is made of the full system) and the length of the series, L.

When a common driver Z is at play on X and Y, and that X and Y are independent, then the correlation between Y and $\hat{Y}$ is positive but it does not depend on the data set length L. So a constant value as a function of L is expected. This has also been shown in Sugihara et al (2012). So we added at line 6, page 5:

For instance, if a confounding factor Z affects both $\vec{X}$ and Y (that are otherwise independent of each other), they will contain a similar information, and the inference of $\hat{Y}$ based on $\vec{X}$ will display a correlation with Y, but which is independent of L (Sugihara et al, 2012).

To explain the behavior as a function of L, we also add at page 5, line 9:

Note also that in an ideal context where the attractor can be reconstructed with precision and for L going to infinity, the correlation should converge to 1. In practical situations, this precision and the asymptotic limit are never reached. The convergence is then limited to a certain level by the presence of observational error, the approximation of the dynamics (like when a low-dimensional approximation is made of the full system) and the length of the series, L.

Minor comments are

3) In the Introduction, the authors say "an important question nowadays is to know whether the Tropical Pacific system forces the dynamics of the climate system in the *extratropics". In my view there is plenty of evidence (it is well known) that the Tropical Pacific system forces the dynamics of the climate system in the extratropics, and therefore, I suggest the authors to re-word this sentence.*

Right. We reformulate the sentence as follows (page 2, line 5):

In particular, an important question that has attracted a lot of attention in the past decades is to know whether the Tropical Pacific system *forces* the dynamics of the climate system in the extratropics.

4) Regarding the link between galactic cosmic rays and global temperature variations, there is a discussion (Questionable dynamical evidence for causality between galactic cosmic rays and interannual variation in global temperature, doi: 10.1073/pnas.1510571112 and the reply, DOI: 10.1073/pnas.1511080112) that the authors, in my view, should also cite.

Thank you very much for these interesting references. These are incorporated in the new version of the manuscript.

5) Table 1 would be easier to read if there is a space between the numbers (i.e., instead of x|y, x | y), also the letters in the figures are too small.

Thank you very much for the suggestion. We did it.

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Please also note the supplement to this comment:
https://www.earth-syst-dynam-discuss.net/esd-2018-3/esd-2018-3-AC1-supplement.pdf

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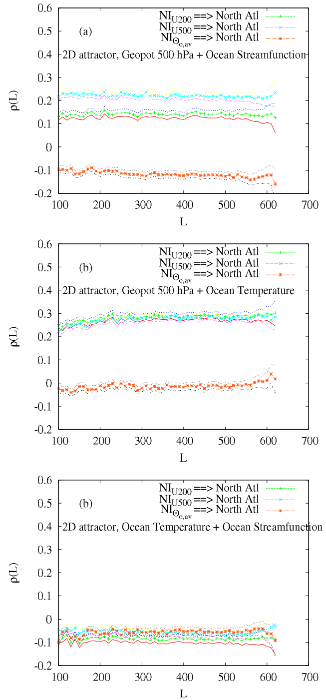

Figure 1: Example of CCM as a function of the length L of the samples, as obtained from the 2D maps for the reanalyses ERA-20C and ORA-20C. The influence investigated here is the one from the Tropical Pacific on the North Atlantic. Each line with symbols corresponds to the influence of one variable on a specific coupled ocean-atmosphere basin. The specific variables are denoted in the caption corresponding to each line in each Panel. The 2D maps used are (a) Geopotential at 500 hPa + Ocean Streamfunction, (b) the Geopotential at 500 hPa + Ocean Temperature and (c) the Ocean Temperature + Ocean Streamfunction.

Fig. 1.