ESD-2018-3 Editorial Decision Letter (ESD)

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Vienna, April 17$^{\text{th}}$, 2018

Dear Authors,

Thank you very much for your responses to the referee comments in the interactive discussion, and for having taken my preliminary access review into consideration when uploading the discussion manuscript.

I have high consideration for the diligent efforts undergone by the authors to address a highly relevant and timely problem, and to honestly communicate their work in faithful deference to their background.

Further to the referee reports containing pertinent remarks, and to the authors' responses and diligences, I hereby issue my editorial evaluation with additional aspects for due consideration. I trust that this decision letter will be taken in by the authors as the constructive assessment that it is meant to be.

For ease of readability, this decision letter modularises the evaluation into the following points:

1. Motivation and Scope

The manuscript "Causal dependences between the coupled ocean-atmosphere dynamics over the Tropical Pacific, the North Pacific and the North Atlantic" definitely addresses a pertinent problem of interest to Earth System Dynamics, and it does so with a technical treatment grounded on Applied Mathematics. Therefore, the rationale behind the contribution falls within the scope of the journal.

However, it should be noted that the manuscript actually tackles a form of *dynamic codependence*, which is not necessarily equivalent to *causal dependence*, as shall be explained throughout this document.

2. Methodological Core

The literature statement that observables are causally connected if belonging to the same dynamical system is devoid of physical grounds. A straightforward counter-example stems from fundamental Physics: in Hamiltonian systems there is no causal connection between the intervening variables, notwithstanding their membership to the same dynamical system.

The shared membership is solely a proof that the observables have a dynamic codependence, connected through the kinematic-geometry of the dynamical system. Any statements about causality are simply *working assumptions* guiding the scientific endeavour to formulate informed hypothesis about the system.

This being said, the authors have made efforts to avoid the temptation to assign cause-effect relationships between variables belonging to the same dynamical system, i.e. to the same deterministic prescription of their time variation and state-geometric dependence. Rather, the authors address *codependence*, a legitimate concept when discussing Dynamical Systems, and which entails kinematic-geometric mutual information rather than causality.

In fact, the dependence is aptly described as being bilateral and fundamentally cross-inferential, in that knowledge gathered from one observable provides information about others living in the same system. Naturally, this is essentially the case for fully coupled dynamical systems where all variables are actually connected. However, not all dynamical systems are coupled, for which reason the readers should be further cautioned about the detailed assumptions of the methodological construct. A very good service was already done regarding the distinction between unforced and forced systems, so the authors shall easily complement their exposition with the aforementioned aspects.

3. Dynamic Dependence vs. Causality

This being said, there is a significant risk that the readership will confuse the undeniable inferential power of dynamical system approaches with the concept of causality. For that reason, any statements about observables *influencing* each other should be avoided unless based on the authors' physical interpretation of the specific systems being discussed from their background knowledge.

Providing information about (i.e. *being a predictor of*) something entails inferential power that may or may not be accompanied by a cause-effect relation. The authors do a good service in staying within the inferential view by saying that codependent variables living in the same dynamical system *provide information about* each other.

Traditional Dynamical System frameworks provide invaluable services to describe system dynamics under very specific assumptions stemming from analytical mechanics and kinematic geometry. The readers should be reminded of those assumptions, so that it becomes clear that Dynamical System based diagnostics are fundamentally motion-descriptive. In this sense, a dynamic coupling between observables is fundamentally a local kinematic-geometric correlation among contemporaries. Therefore, no causality can be inferred from such diagnostic.

Overall, a diagnostic of null dynamic dependence does not necessarily preclude the existence of causal dependence. Likewise, the detection of dynamic dependence does not necessarily prove the existence of causality in the system.

Focus should be given on whether this particular method has detected or not dynamic codependence among the study data, for the set of conditions under which the method holds, and for the data features that have been actually captured. The causality considerations then come from the authors' physical interpretation of the mechanisms at play, as the geometry of the method per se is inherently non-causal.

4. CCM vs. Alternative Methods

The CCM method falls into the usual paradigms of inference from shared information, akin to the rationale behind the classical Mutual Information diagnostics in Information Theory (IT). Mutual Information in IT, as CCM, assesses non-directional similarity among observables, yielding positive results when they share information in the multivariate space spun by their dynamics, with the fundamental difference that in IT the treatment is made in the probabilistic space rather than the state space.

Therefore, both Mutual Information and CCM fall onto the same paradigm of cross-inference that quantifies the ability to provide information about one observable from another, but contain no proof of causal relation between them.

It might then be wondered whether popular measures of directional information (e.g. Conditional Mutual Information, Transfer Entropy, Bayesian Approaches) then provide causal information. Transfer Entropy is by the way the non-Gaussian generalisation of "Granger Causality". However, notwithstanding their clear directional inferential power, these are not causal either, as predictors are not necessarily causes and the physics are entirely lost in the statistical constructs undergone in the aforementioned methods.

Overall, while comparison with a vast diversity of other methodologies alleging causal inference would be interesting, in reality none of the many published alternatives provides satisfactory treatment of causality either, so this remains fundamentally an open problem. In that sense, despite being clear that this study it investigating dynamic dependence rather than actual causality, it adds to the crucial debate on these problems and should therefore not be lightly dismissed.

5. Correlation vs. Dynamic Relation in Estimation Quality Assessment

The authors aptly acknowledge the limitations of the classical state space reconstruction, and wisely look for a methodological improvement to implement in their analysis. The adaptations made to CCM are well motivated and enrich the scientific discourse about such procedures.

However, it is not devoid of caveats that need to be taken into consideration. The evaluation of the similarity between actual and estimated value of a variable is done in a statistical correlation-based diagnostic that raises serious questions as to whether the quality of the estimation is being properly evaluated in that manner.

In fact, contrary to what the reader is led to believe from the manuscript (page 4), a high Pearson correlation between actual and estimated values does not necessarily prove that the estimation is good. Likewise, a low (or even null) correlation is not indicative of fundamental dissimilarity between observation and estimation. These caveats stem from that correlation measure being limited in its adequacy to linearly related, normally distributed data, being assessed for statistically aggregate relationships.

In other words, statistical metrics such as Pearson Correlation do not provide the dynamical relation between variables mentioned in the manuscript (page 4). Rather, they provide a first-order linear statistical relation between them, aggregated over the domain where the statistic has been drawn.

6. Prediction vs. Estimation

The methodology takes aim at analogs found around one variable $\mathbf{X}(t)$ to recover the value of another variable $Y(t)$, contemporary to $\mathbf{X}(t)$ (page 4, second sentence). However, in the following sentence the authors mention *prediction*. Physically speaking, a prediction entails the estimation of a future state. Here, however, the reader is informed about an *estimation* of a contemporary variable, i.e. without lead time. Therefore, the action to "predict" should rather be phrased as to "estimate".

7. Metric for Analogs

According to page 3 (bottom), an Euclidean distance is mentioned for analog selection. In smooth flows living in symplectic manifolds that should work well, as there will be local homeomorphisms between the local attractor charts and an Euclidean tangent bundle. However, that is not necessarily the case for real-world applications where such smoothness no longer holds. Euclidean distances should thus be used only under thoroughly justified appropriate conditions.

8. State vs. Phase Space

A *State Space* comprises the *state variables* participating in the system. A *Phase Space* comprises the state variables and the corresponding *conjugated momenta*. A system with N state variables has a N-dimensional state space, and a $2N$-dimensional phase space. For instance, given a state space $\mathbb{R}^N$, the corresponding phase space is $\mathbb{R}^{2N} = \mathbb{R}^N \times \mathbb{R}^N$ (which by the way is a symplectic manifold).

Interchangeable use of state and phase space is thus prone to ambiguity and error: an unfortunate mistake that has propagated across significant sectors of the scientific literature. With all due personal respect and consideration for illustrious past references from which we have learned so much, we must nevertheless exert objective criticism when something that they have written is unfortunately incorrect. Published mistakes should in no way be deemed correct when these concepts are rigorously defined in Physics, where each has their unique meaning as explained in the previous paragraph.

Therefore, for the sake of clarity, physical rigour, and to avoid any ambiguity and further propagation of misinformation, the term *Phase Space* needs to be reserved solely to the space spun by the state variables and their conjugate momenta. The state spun by the state variables of the system will have to read *State Space*.

Caution must also be exerted when treating the state space per se, as not all observables involved in the dynamics are actually state variables. For instance, fluxes are not state variables despite being observable quantities. The bottom line is that the state space is not necessarily the space spun by all the observables, but rather solely by the actual state variables. Again here, fundamental Physics informs on which variables of a dynamical system are state variables and which ones are not.

9. Geophysical Case Study

The geophysical case study discussed in the manuscript is very interesting and ensures the goodness of fit within the scope of Earth System Dynamics. Notwithstanding the existence of prior studies discussing the absence of causal relationships across oceanic basins, the present study has the merit to explore dynamic codependence with a methodology that, despite its caveats, still provides insightful food for thought and discussion.

Whether or not dynamic codependence is detectable with the used method depends on both the specific abilities of the method and on the nature of the relations being diagnosed. For that reason, it is wise to refer to what exactly is being diagnosed and what is being eluded i.e. not assessed by the method. In this sense, the authors have already undergone significant revision efforts in their author responses to the referee comments.

With further reflection and discussion, the case study can be further strengthened, for which reason I encourage the authors to further contextualise their findings in the light of ocean-atmospheric physics, the methodological construct and practical workflow, the details of which should be further discussed as well (see point 10 below).

10. Reproducibility

The reproducibility of the study depends on the reader's ability to read in between the lines. However, that should not be the case: the workflow should be fully explicit, otherwise a significant segment of the readership will be alienated.

While I was personally able to attest the reproducibility in my own terms, I am skeptical about whether the general readership will. In fact, a thoroughly detailed workflow is still largely missing in the manuscript.

A thoroughly detailed workflow with crucial mathematical and algorithmic details will strongly improve the reproducibility and soundness of the manuscript. Only then will be the appealing narrative and results be properly assimilated, and the actual meaning of the diagnostics being undertaken will be properly understood by the readers.

11. Overall Decision

All in all, the manuscript addresses dynamic codependence on the basis of shared attractor membership, rather than causal dependence. For that reason, the title and body should be adjusted accordingly, in order to avoid misinterpretation.

The manuscript should thus be revised taking into consideration the referee concerns complemented by the aspects raised in the aforementioned points in the present editorial report. The revision diligences already undergone by the authors in response to the referee reports are encouraging and should be furthered in accordance with the raised concerns.

I have confidence in the authors' ability and willingness to take the referee and editorial remarks into due consideration when proceeding with their manuscript revision.

For that reason, I return the manuscript to the authors, looking forward to the next revised version.

With very best wishes,

Rui Perdigão
(ESD Editor)

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Reference of this letter:

Perdigão, R.A.P. (2018): ESD-2018-3 Editorial Decision Letter, 17.04.2018, Earth System Dynamics. To be available at URL: https://www.earth-syst-dynam-discuss.net/esd-2018-3/ upon eventual final publication of ESD-2018-3 at Earth System Dynamics (Vannitsem and Ekelmans, in Review).

Reference of the ESD-2018-3 manuscript at Earth System Dynamics - Discussions:

Vannitsem, S. and Ekelmans, P.: Causal dependences between the coupled ocean-atmosphere dynamics over the Tropical Pacific, the North Pacific and the North Atlantic, Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2018-3, in review, 2018.