Response to the comments of the editor

July 26, 2018

Stéphane Vannitsem[1] **and Pierre Ekelmans**[2]

[1]Royal Meteorological Institute of Belgium, Avenue Circulaire, 3, 1180 Brussels, Belgium

[2]Theory of Neural Dynamics Group, Max Planck Institute for Brain Research, Max-von-Laue-Strasse 4, 60438 Frankfurt, Germany

Dear Editor

We would like first to thank you for the very constructive comments that you have made during this second round of review. Please find below how we address your three different points that have to be settled. The additional text is colored in green in the pdf version of the new manuscript.

1. Causality: There is a clash of cultures between physical and the kinematic-geometric views. In fundamental Physics and most fields of natural science, causality pertains cause-effect relationships. This is what had been meant as causality in the previous editorial report. In kinematic-geometric approaches to dynamical systems, there is no proven cause-effect among co-members of a dynamical system, but rather a codependence bond as expressed through kinematic-geometric couplings, which may be physically non-causal. The term causation mentioned in the author response is yet another concept that is physically different from that of causality but is not the object of study in the manuscript, therefore the only note I leave in that regard is that these are not equivalent concepts. Closing the matters on point 1 , the conceptual debate on causality can be avoided in the revised manuscript by clearly reiterating that the work addresses causality from a dynamical systems perspec- tive, and acknowledging that this is not to be confused with the physical cause-effect definition. This way, causality statements on the manuscript will be positioned on their specific kinematic-geometric context, deploying the analysis and discussion without conceptual clashes.

We indeed do agree with your point. The notion of causality is multifaceted and it is not easy to grasp this concept in one definition. In order to clarify the specific definition used here, we have introduced the following text at page 3, line 23:

It must be stressed here that the notion of causality is used in the very specific context of dynamical systems theory discussed above, that should not be confused with the traditional cause-effect relationship. From now on, the words causality and dependences will be used equivalently.

We have also stressed the specific notion used here by introducing at several places in the introduction the terms **in a dynamical sense** when speaking of causality.

3. Correlative measures used to validate results, while still widely popular in some fields (e.g. in weather forecasting), are not devoid of caveats as noted in previous referee and editorial reports. However, such measures can be used provided that their limitations are clear and their power is not overstated. All measures have caveats and it is always beneficial to have them at least briefly stated in the text so that the readers will not get overconfident on the actual value of the diagnostics being made. This will settle point 3.

We also agree on that. We have introduced a comment as a footnote in the description of the technique in order to avoid breaking the readability of the description. This is introduced at page 4 as

Note again that the correlation is measuring the linear association that could sometimes be insufficient in the case of nonlinear dependences

4. Euclidean metrics should not be assumed as straightforwardly valid, instead being justified in the light of the problem being studied. Their use can be justified when the smoothness of the dynamics is ensured to enable a homeomorphic mapping between their kinematic geometry and that of the tangent space where the Euclidean metrics are usually deployed. While not all dynamic processes will produce a valid phase space manifold, in many geophysical fluid dynamics applications that working assumption can be justified to some extent in light of the nature of the flow, provided that special care is taken when dealing with discretisation issues naturally emerging from operational finite-step treatments (be they data-based or numerical). By explicitly stating the qualifying attributes (e.g. smoothness of the dynamical system under study), the authors can then provide a geophysically-satisfactory justification for the use of the chosen metric. This will settle point 4.

We introduced a comment on that aspect at page 4, line 3 as

This can be done provided the solutions of the dynamical system display sufficiently smooth properties (e.g. continuity) as it is often assumed for large scale geophysical fluid dynamics.