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Nonlinear Processes in Geophysics
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Interactive comment on “Detecting Changes in Forced Climate Attractors with Wasserstein Distance” *by* Yoann Robin et al.

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I have found the paper quite interesting and innovative and I support its publication in NPG once certain issues are analysed in greater detail. I would like to make some remarks that I hope the authors will take into consideration.

(1 - Page 1, Line 21): The authors should consider giving a look Lucarini V. (2017) where an extensive statistical mechanical analysis of climate response to forcing is given.

Response: The reference has been added.

(2 - Page 2, Line 24): "Intuitive" is not really a good world. Our visual impression and

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the way we interpret it is far from being in any sense objective. I understand what the authors say, but I kindly ask to re-formulate.

Response: We agree with you.

Modification (Page 2, line 16): “Although it is intuitive from Figure 1 that...” has been replaced by “Panels a and b in Figure 1 are visually very similar, whereas Panel c cannot be deduced from a trivial transformation of the first panels. Therefore, it is expected that μ is "closer" to ν than ξ.”.

(3 - Page 4, line 18): The construction of the pullback attractor requires the integrations started at a $t = t_0$, with t_0 going to minus ∞. Otherwise no well-posed definition is possible. This should be clearly explained. Is one year of integration enough, in this case?

Response: In Section 3, the integration is performed between 0 and τ. Because the dynamic of Lorenz 84 for winter and summer does not depend on time, it is equivalent to an integration between $-\tau$ and 0. The parameter τ is chosen to be 5 “years”, but we checked than 1 year is enough. Following Drotos et al (2015), we keep τ at 5 years. In Section 4, a first integration is performed between 0 and τ, and then we integrate between 0 and 200×73 from first integration. Due to cyclicity of seasonal forcing, the first integration is equivalent to an integration between $-\tau$ and 0.

Modification (Page 4, line 16): The parameter τ has been set at one year ($\tau = 73$), and the first integration is performed during 5τ (i.e. 5 cycles / years). We have added the sentence “Snapshot attractors are special cases of *pullback attractors* Chekroun et al (2011). The latter class requires an integration between $-\infty$ and a desired final time. Eq. (2) does not depend of time, so the integration into Sec. 3.1 can be performed on any length intervals”.

(4 - Page 5, line 11): In this part there is no mention of the way A is chosen. This seems quite important for the rest of the paper.

Response: See Question 6.

(5 - Page 4, line 13): The authors might want to note explicitly that each of the realised estimate of the measure supported by the pullback attractor come from initial conditions at t_0 (see point 3) distributed uniformly according to Lebesgue of the union of the little cubes.

Response: We agree with you

Modification (Page 4, line 11): We have replaced "we draw N random initial conditions in a cube that includes the attractors, and iterate the dynamics of the systems for a time τ", by "we draw N random initial conditions following a uniform distribution. All margins are independent. This approximates a Lebesgue measure in a cube that includes the attractors. We iterate the dynamics of the systems between $t_0 = 0$ and a long time multiple of τ."

(6 - Page 6, Section 3.3): Discussion on the value of A is missing

Response: A was a misleading parameter. It represents only the number of boxes with non zero mass, but it can be different for different realization of same attractor. The value of 0.1 gives 40 to 60 bins on each axis, assuming that the attractor lives in a box of $[-1;3] \times [-3;3] \times [-3;3]$. This means that the volume is divided into $40 \times 60 \times 60$ boxes. This number is the same order of magnitude as the number of gridcells in the NCEP reanalysis around the North Atlantic region, should one be interested in the climate attractor of that region (e.g. Faranda D. et al (2017)). We also tried values of 0.05, 0.2 and 1.0 for the size of the boxes (so, a factor 2 for the two first, and one scale up for the last) for the protocol of Section 3. For all values, the maximal variation of standard deviation is 0.01, and the detection is not affected. For a size of 0.05 and 0.2 the maximal variation of median is 0.03. For the size 1.0, the maximal increases of median of box plot of winter (resp. summer) against itself is 0.22 (resp. 0.18), but the difference with median of winter against summer is at least equal to 0.3.

Modification 1 (Page 5, line 5): We have been added the sentence: “We chose a bin length of 0.1 for the Lorenz attractor, which remains in a $[-1;3] \times [-3;3] \times [-3;3]$ box. Therefore $40 \times 60 \times 60$ bins cover the attractor. This number of bins is comparable to the number of gridcells that cover the North Atlantic region in the NCEP reanalysis (or most CMIP5 model simulations). This example refers to a few papers dealing with climate attractor properties (e.g. Corti S. et al (1999) and Faranda D. et al (2017)).

Modification 2 (Page 7, line 15): The sentence “This protocol was also applied for bin sizes of 0.05, 0.2 and 1.0. For 0.05 and 0.2, the maximal variation of median (resp. standard deviation) of Wasserstein distances is 0.03 (resp. 0.01), so the distributions are indistinguishable in practice. For a bin size of 1.0, the maximal increase of the median is 0.22, but the difference with the median of winter against summer is at least equal to 0.3.” has been added at the end of Sec. 3.3.

(7 - Page 7, line 3): I disagree with the use of "visual impression".

Response: We agree with you.

Modification (page 7, line 11): “This visual impression is confirmed...” has been replaced by “This discrimination is confirmed...”

(8 - Page 8, lines 13-14): The statement is indeed overblown if given in all generality as here.

Response: We agree with you.

Modification (page 8, line 25): “We conclude that the Wasserstein distance has a high

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capacity of discriminating between different dynamical systems" has been replaced by "We conclude that the Wasserstein distance has a high capacity of discriminating different attractors coming from this dynamical system"

(9 - Page 9 - End of Section 4): There is a fundamental misunderstanding here, I believe. It is true that a much lower number of integrations is needed to say that two attractors are different. This is a very interesting result. But you are not able to quantify well what is (quantitatively) the difference between the expectation value of any given (possibly interesting) observable of relevance. So, you are left with a statement that is in fact qualitative rather than quantitative (the two attractors are different!). How can you relate the Wasserstein measure to any useful information? This does NOT diminish the relevance of the performed analysis, to be clear.

Response: It is true we do not use the link between Wasserstein distance and other dynamical information to measure qualitative changes in attractors (e.g. dimensions). The chapter 9 of Villani (2003) gives some link between the Wasserstein distance and entropy.

(10 - Page 11, line 7): not clear the relationship between ρ and μ.

Response: ρ is the density of the measure μ.

Modification (page 11, line 25): We state that: $\rho(\geq 0)$ is the density of the measure μ. Hence, for all Borel set A in phase space, they are related by:

$$\mu_t(A) = \int_A \rho_t(\mathbf{x}).\mathrm{d}\mathbf{x}.$$

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