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Interactive comment on "Global warming makes weather in boreal summer more persistent" *by* Dim Coumou and Paolo De Luca

Anonymous Referee #2

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The topic of the persistence of atmospheric motions and trends therein is certainly interesting, and one that has received ample attention in the literature. The authors here propose a new perspective on atmospheric persistence, by utilising a metric which does not depend on the definition of weather regimes. I am sympathetic with this attempt, but I overall find the study to display serious methodological and interpretational shortcomings. I further struggle to understand what new conclusions the metric adopted by the authors provides, as it seems to largely confirm the results identified by more conventional (and intuitive) approaches. I believe that these issues are structural in nature, and may not be satisfactorily addressed within a major revision. As such, I can only recommend rejecting the manuscript.

Major Comments

Form and Contextualisation

1. The paper feels as though it has been written in a short letter format and then sent to a journal accepting full-length articles in a second moment. There is very little analysis and discussion in the main paper, and plenty of largely uncommented results in the Supplementary Materials. In order to support the many unsubstantiated statements and conclusions in the text, there would be the need for a full-length study including substantial additional analyses – from verification of the methodology to analyses of extreme events etc. Seeing the extremely compact form of the current study, there is certainly the space (and, as mentioned above, the need) for this. See also my other comments below, and in particular comment #3 in this section.

2. Most of the abstract and the first paragraph of the introduction, as well as parts of the discussion, focus exclusively on climate extremes. However, there is not a single part of the analysis that systematically considers extreme events. The closest there is, is a one-off mention of a heatwave in Fig. 1b and an analysis of trends in highly persistent states, but even then no analysis is performed to make an explicit link to climate extremes.

3. The whole discussion/conclusion section, much like the introduction, appears to summarise results from the literature with a very tenuous link to the analysis within the paper. The authors discuss a number of points, from frequency of extremes to meridional temperature gradients, to land-ocean temperature contrasts, yet none of these are touched by their analysis. Some of the numerous additional analyses that would need to be performed to justify this discussion include: verifying whether the results from the outlying CMIP models may be explained by biases in those models relative to meridional gradients and/or other mechanisms evoked by the authors or systematically linking regional high-persistence events as diagnosed by the theta metric the authors propose to specific classes of high-impact climate extremes.

4. The authors are very perfunctory when they review the literature on the persistence

of atmospheric flows, including some odd omissions. I believe that rectifying this will require something beyond simply adding a few references to the text. The authors need to perform a careful review of the approaches and techniques developed to study atmospheric persistence and reframe their study in that context.

a. ll. 12-13 As a number of studies cited by the authors (and some that they do not cite) show, this is perhaps a light-hearted evaluation of the rich body of literature on the topic. Some potentially useful missing references are: Kucerova et al. (2017) and Francis et al. (2018). Also, during a brief online search for the literature dealing with atmospheric persistence, I found that one of the authors recently published on the topic (De Luca et al., 2019). This work is not cited, even though the authors are far from shy with self-citations elsewhere in the manuscript. Is there a reason for this?

b. The authors cite Huguenin et al. (2020) in the introduction, but do not discuss anywhere that the conclusions of the latter study contrast with those presented here.

c. ll. 61 and following. As for point (a) above, this is again a rather sweeping statement and one that does not reflect the rich literature in the field of persistence metrics, including those that have focussed on addressing the "discontinuity" issue highlighted by the authors in the weather regime approach. Some examples – but there are others – include: Franzke et al., (2011); Fereday (2017) and Miller (2020).

d. There is a rich literature on persistence in dynamical systems that the authors ignore entirely. While I would not normally expect to see this referenced in a climate science paper, the specific approach the authors adopt here demands some acknowledgement of and contextualisation relative to this literature.

Methodology

1. The authors focus their analysis on Z500, and then state that similar results are obtained when looking at SLP.

a. I would tend to disagree that this is the case. Indeed, a comparison of Figs. 3 and

S4 shows numerous differences, both in terms of significance of the trends and of the absolute magnitude of the trends. This is somewhat puzzling and makes me wonder how robust the results shown by the authors may be. On large scales I would expect a close link between the Z500 and SLP footprint of atmospheric features.

b. It is understood that long-term temperature trends are associated with long-term Z500 trends. The fact that Z500 shows different (larger) persistence trends relative to SLP, and that Z500 presumably shows systematic long-term trends not seen (or seen to a lesser degree) in SLP, makes me wonder whether the results the authors are showing are an artefact of their choice of variable. I am well aware that there isn't any accepted universal theory for renormalisation of dynamical systems, which complicates verifying this, but the results should not be published unless there is absolute clarity on this point.

2. There are important methodological steps and theoretical groundings that are entirely missing from the paper. For example, on l. 131 the authors describe quite an important methodological step for the paper, yet there is no explanation of what sigma is, nor of why its inverse should provide useful information on the evolution of a dynamical system whose Poincaré recurrences satisfy eq. 2. One should not need to hunt for key details in the references provided. Similarly, for what systems were the theoretical results the authors discuss derived? What were the underlying assumptions?

3. The description of the methodology is severely flawed. If the method is indeed novel, as the authors state, tests as to its suitability for the current study should be performed. The fact that it has been previously implemented in the literature does not mean it may be applied blindly without any statistical checks.

a. As I understand, the Freitas et al. (2010) results referenced by the authors apply to stationary, Axiom A systems. Is this indeed the case? If so, what are the implications for the current analysis? The question of applying results developed for Axiom A systems to non-Axiom A data has been discussed at length in the dynamical systems literature, and the results show that this may not always be advisable and particular care is needed.

b. How well do the recurrences the authors find converge to the limiting distribution in Eq. 2? Are the shape parameters indeed close to zero? If not, do the authors have a way of evaluating whether the procedure still provides reliable estimates of the local dimension and persistence?

c. A quick scan through Süveges (2007), indicates that the estimator for the extremal index relies on a number of properties of the system, for example concerning the limiting distribution of inter-cluster times. Do these assumptions hold for the current data? As I mentioned before, previous application in the literature to different climate datasets and observables does not guarantee that valid results are automatically obtained for any set of climate data.

d. Related to the point above, as Süveges proposes an MLE estimator, can the authors provide some uncertainty estimate, or an evaluation of the stability of the estimator?

4. There are a number of imprecisions and mistakes in the description of the dynamical systems methodology, some of which seem to stem from the authors misunderstanding the results they cite and equations they provide.

a. ll. 73-75 After reading through the results section, I am puzzled by this statement. While such a claim may be relevant for the local dimension, and presumably refers to the classic embedding approaches for estimating attractor dimension, why would estimating theta ever require any assumption as to the system's dimensionality?

b. l.81 Based on the authors' explanation of the methodology, I do not believe that theta corresponds to speed in phase-space. Indeed, being derived from the extremal index, theta appears to have no information on the distance between successive samples taken along the trajectory, other than that they need to reside in a given neighbourhood. The size of the latter, as per Eq. 1 and the following paragraph, has a variable size

which is determined by the geometry of the trajectory in the relevant region of the system's phase-space.

c. ll. 125 and following. The authors mix cursive \theta and normal \theta. From looking at Moloney et al. (2019), it is pretty clear that the extremal index and the theta the authors call "persistence" are not equivalent.

d. l. 131 This doesn't make much sense. By definition, xˆ-1 = 1/x, so \thetaˆ-1 can only be equal to 1/\theta.

e. ll.163-165 Why would some missing values make it impossible to compute the metrics? Have the authors verified whether removing a small number of random dates from the other datasets causes any large difference in the results?

5. The authors state that they analyse the data on the native horizontal resolution of each model. Since the methods for computing local dimension and persistence appear to be based on recurrences and Euclidean norms, this makes any comparison, such as that performed on l. 177, an apples-and-pears comparison. Computing distances over the same region in different datasets with a different number of gridpoints within said region will introduce very large biases in the comparison of the results for absolute values of the local dimension and persistence. Working on the native grids may be acceptable if the main scope of the analysis were to evaluate the impact of spatial resolution, or if each dataset was only compared to itself, but this is not to the case here. A convincing test of the impact of resolution on the dynamical indicators used here should be conducted to justify the analysis being presented.

6. I am very puzzled by the fact that persistence seems to be larger for the whole mid-latitude region than for the sub-regions (Fig. 3). An intuitive understanding of atmospheric persistence would suggest that smaller regions should have the potential for longer persistence, as there are less things that can change. By comparison, a region comprising two storm tracks, with multiple active high or low pressure systems at any one time, even in summer, should show very low persistence. How can the results

from theta be reconciled with a conventional understanding of atmospheric dynamics? If they cannot, what is the indicator used by the authors really showing here and how may it be linked to the conventional notion of atmospheric variability?

Interpretation of the Results

1. Throughout the text, there are a very large number of unsubstantiated comments and vaguely-posed arguments that have no place in a scientific manuscript.

a. l. 178 This hypothesis is not supported by the analysis, due to the different resolutions of the datasets – see also my point on the methodology.

b. ll. 199-201 This is an unsubstantiated statement. While when taking a weather regime/clustering approach extremes are often related to persistent states, it is not clear to me that: (a) the inverse is true, i.e. persistent states are systematically related to extremes; and (b) the same applies for theta, seeing that the authors report very low persistence values (typically lower than 3 days). As the authors adopt an unusual metric, this link would need to be shown explicitly for the regions/datasets considered here.

c. ll. 205-206 I do not understand why an increase in persistence is "consistent" with a decrease in "d". While the two may be correlated for specific systems/observables, from the explanation of the methodology I see no a priori reason to deem the increase in persistence to be consistent with a decrease in local dimension.

d. ll. 229-231 I would be careful in drawing such strong conclusions from regressions (with low R2 values) in this context, however low the p-values are. There may be a large number of covariates here and this simple analysis does not show that the weakened westerlies "increase the persistence".

e. ll. 261-263 This statement conflates local dimension and persistence, and is completely unsubstantiated. The analysis shows no evidence that a low "d" is associated with stalling weather events, nor that a low "d" is linked to events that "matter to so-

ciety". Indeed, the authors do not provide any systematic proof that low "d" and high persistence are related for the data being analysed. The fact that the trends in the two indicators are opposite (cf. Fig. 3 with Fig. S5) may bring one to further question the robustness of this proposed link.

Novelty

1. The authors make a rather sweeping statement on the current state of the literature in the introduction, and argue for the need for a new metric to evaluate the persistence of atmospheric flows and trends therein. They propose to adopt a rather complex metric, issuing from very theoretical results, which they term theta. Throughout the text, they then make frequent references to the link between theta and the more conventional persistence of weather regimes. Finally, in the concluding section they discuss how the results they obtain from theta largely match the results in the literature and our physical understanding of atmospheric motions. At this point, the reader is brought to question the relevance of the results obtained here. A general slow-down of the summertime atmospheric circulation/increase in persistence has already been repeatedly hypothesised in the literature, as the authors themselves discuss in the introduction. To be justified, a new and far from intuitive metric needs to provide some clear additional insights compared to the existing approaches, which I do not see as emerging from the analysis conducted by the authors.

a. A good example of the type of issue I have with novelty here is on ll. 240-245. Here the authors seem to suggest that analyses based on weather regimes may overlook positive trends, yet with few exceptions the studies using weather regimes find increases in persistence, much like the authors here.

Minor Comments

l. 14 and elsewhere in the text: "More-persistent", please remove the hyphen.

ll. 15 and 73 "full state of the atmosphere". This is definitely not the case. As I understand, the authors base their analysis on two observables, which are most definitely not a “full state”. At most, they could call these Poincaré Sections of the system.

l.65 “thus hampers proper statistics” This is a very strong statement. However, some may argue that there are studies based on weather regimes that conduct much more rigorous statistical analyses than those presented here.

l.68 Cortesi et al. (2019) in their introduction seem to suggest that spring and autumn are problematic seasons, rather than summer. Please remove this reference or justify more clearly how it supports the statement you are making.

Fig. 1 Caption: “many different type of new states”, please correct.

l. 129 “the parameters are sensible”, please correct.

ll. 201-202 This is hard to evaluate, as Fig. S2 points to Table S3 for the numerical values of the trend, yet the caption of Table S3 states that the table refers to Fig. 3.

ll. 246 and following The authors should clarify that this is not something that they show here (beyond a plausibility hypothesis for zonal wind), but that they rely on previous work for this interpretation.

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