Replies to Reviewer #1

November 2020

We would like to thank the Reviewer for the constructive set of comments provided. We provide a detailed reply (in red) to the individual comments (in *italics*) below.

While dynamical systems methods can surely provide additional insight in climate data, the present study does not make a very convincing case. I do not see the advantage over some other used metrics.

We take the Reviewer's concern very seriously, as our main aim in this technical note was not to present a full scientific analysis, but rather to motivate the usefulness of concepts from dynamical systems theory in the analysis of large palaeoclimate simulations. We detail some changes we have implemented in our study to better motivate the use of the dynamical systems approach, in our reply the first part of the Reviewer's Comment #1. Partly in response to one of the comments by Reviewer #2, we have further added to Sect. 4 a discussion of the merits of our approach relative to other commonly used statistical methods. We specifically consider the widely used weather regimes (which are often derived from PCA or clustering approaches) and the Canonical Correlation Analysis, which identifies linear combinations of two variables with maximum correlation. Finally, always in Sect. 4, we have included a discussion of the possible applications of our approach to the identification of tipping points in climate datasets and to other palaeoclimatic problems, such as diagnosing the responses of different numerical models to a given forcing.

1.

- *Looking at Fig. 1 I do not see how to gain additional insight from the two dynamical systems measures compared with inspecting the precipitation (Fig. 2c).*
- *Only for the control simulation uncertainty bounds are given. My guess is that uncertainty bounds for the other two simulations would overlap with the bounds of the control simulation. If this is the case then one cannot say that the measures actually show any significant differences. They are already now almost always in the uncertainty bounds of the control simulation. So, what do we really learn from this?*

Concerning the Reviewer's first point we assume s/he was referring to Figs. 2a, b versus Fig. 2c in the original draft. Indeed, Fig. 1 in the original draft is not related to dynamical systems, and was simply provided to illustrate the climatology of the simulations we are analysing. Focusing the discussion on Fig. 2 in the original draft (Fig. 3 in the revised manuscript), we argue that the dynamical systems metrics provide a very efficient tool to rapidly identify salient differences between the simulations and enable to put forth mechanistic hypotheses on their origins. For example, neither Fig. 1 nor Fig. 2c give any indication as to the mechanisms driving monsoonal precipitation in the three simulations. The increased persistence in the MH_{GS+PD} and MH_{GS+RD} simulations, shown in Fig. 2b, immediately points to the fact that the role of transient atmospheric features is likely weakened compared to the MH_{CNTL} run. This hypothesis would then need to be verified with detailed analyses of atmospheric dynamics, but the computationally inexpensive θ metric is nonetheless valuable in pointing to it as an interesting aspect to investigate further. Similarly, having ascertained that d is sensitive to the monsoon's onset, its interannual variability can be used to quantify the variability of the monsoon's onset within each model simulation. If one were using more conventional analysis techniques, this would likely require defining and computing a monsoon onset index. It is further an aspect which could be easily overlooked, seeing as an inspection of a simple seasonal cycle of precipitation does not evidence the pre-monsoon season as being particularly variable, while for d it is the season displaying the largest variability (see blue shading in old Fig. 2a, c, now Fig. 3a, c). Although we appreciate that this goes beyond the part of the analysis the Reviewer was commenting on, we would like to highlight that the metric which is perhaps the most valuable in providing additional insights compared to more conventional analyses is α. It is not easy to design a computationally efficient, multivariate statistical dependence measure providing a value for each timestep of a dataset. As we show, α can help to probe in a 1-D space the role of large-scale atmospheric circulation anomalies in favouring the northward extension of the monsoonal precipitation. As stated above, we take the Reviewer's concerns on the usefulness of the approach we propose very seriously, since they point to the fact that we have not illustrated the above aspects clearly in our text. We have therefore updated the initial part of Sect. 3.2 and parts of Sect. 4 to reflect more explicitly the value that we believe lies in the application of the dynamical systems metrics we propose.

Concerning the Reviewer's second point, we agree that we should have placed greater care in some statistical aspects of our analysis. We provided an indication of the standard deviation of the control simulation in the figure as reference for the metrics' variability. However, we did not mean to use it as a statistical test of whether the curves are different or not. Indeed, we are not interested in testing the difference on single days (which is what such standard deviation bounds would be showing), but rather on testing whether our approach detects a significant difference in the atmospheric/monsoonal dynamics between the simulations, i.e. a difference over the whole monsoon season or year. To this effect, we have performed a Wilcoxon rank sum test, which enables us to make a statement as to whether the data from two samples is drawn from continuous distributions with equal medians or not. We performed the test at the 1% level to compare the medians of the control simulation to the two perturbed simulations, for the dynamical systems metrics shown in Figs. 2a, b, and 4a, b (Figs. 3 and 5 in the revised manuscript) for both the Pre-Monsoon Season, Monsoon Season and the whole year. We found that in all cases the null hypothesis of the population medians being equal is rejected. Based on this test, we conclude that the dynamical systems metrics highlight a significant difference between the control and perturbed simulations under both the Pre-Monsoon and Monsoon seasons. We have now added a description of these results to Sect. 3.2 in our manuscript, focusing for conciseness on the monsoon season. We further discuss the large variability within each run which, although it does not preclude the statistical significance of our results, is nonetheless a relevant aspect to touch upon.

2. *I do not think that the typical Climate of the Past reader is very familiar with dynamical systems concepts like Poincaré recurrences, Axiom A system, etc. The authors should explain them more carefully and in an intuitive way. Has it actually been shown that the climate system is Axiom A? Perhaps the authors should first provide an intuitive introduction to the concepts and methods and move the more*

technical details to an Appendix.

We understand the importance of making our methodology accessible to a readership which may not be familiar with jargon specific to the dynamical systems community. We have now expanded Sect. 2.1 to improve the qualitative explanation of the methodology. We have specifically added analogies to the flow of raindrops on topography as intuitive "mental anchors" that may be related directly to the concepts of local dimension, persistence and co-recurrence. At the same time, since this is a technical note, we would like to keep the more formal description of the methodology in the main text (Sect. 2.2). We have, however, removed some non-essential jargon (including the term "Axiom A") and added explanations of some uncommon terms to the section, so that a technically-minded reader may be able to follow the derivation without needing to have a background in dynamical systems theory. We have further added a new Fig. 1 to this section to provide a graphical illustration of our approach, and thus facilitate the understanding of the metrics' computation. To clarify this structure, we have added a short introductory paragraph to Sect. 2. Concerning Axiom A systems, (Eckmann and Ruelle, 1985; Ruelle, 2009) we recall that these are a special class of dynamical systems possessing a Sinai-Ruelle-Bowen (SRB) invariant measure (Young, 2002) and featuring uniform hyperbolicity in the attracting set. Such invariant measure is robust against infinitesimal stochastic perturbations, namely it coincides with the Kolmogorov physical measure. Another important property of Axiom A systems is that it is possible to develop a response theory for computing the change in the statistical properties of any observable due to small perturbations in the flow (Ruelle, 1976, 2009). While they may seem a purely theoretical construct, Axiom A systems do have a direct relevance for geophysical fluid dynamics applications, and climate models themselves behave much like Axiom A systems (Ragone et al., 2016). A very nice explanation of the relevance of Axiom A systems for climate science can be found in Lucarini and Bodai (2017): "Axiom A systems are indeed far from being typical dynamical systems, but, according to the chaotic hypothesis of Gallavotti and Cohen (1995), they can be taken as effective models for chaotic physical systems with many degrees of freedom. Specifically, this means that when looking at macroscopic observables in sufficiently chaotic (to be intended in a qualitative sense) high-dimensional systems, it is extremely hard to distinguish their properties from those of an Axiom A system, including some degree of structural stability. One can interpret the chaotic hypothesis as the possibility of constructing robust physical properties for the system under investigation. Therefore, providing results for Axiom A systems can be thought of as being of rather general physical relevance." As mentioned above, we have chosen to remove the term "Axiom A" from the paper, as in earlier works (e.g. Faranda et al. 2017), we have seen that the dynamical systems metrics can be computed for non-uniformly hyperbolic attractors, providing insights in the dynamics such as the existence and the properties of singular points. In the new version of the manuscript, we instead explicitly list the theoretical requirements for a compact attractor and a stationary system, which we found can affect the computation of the dynamical metrics (see, e.g. Faranda et al. 2019b). However, as the Reviewer comments and author replies remain public in the CP discussion, we deemed it important to address the Reviewer's very pertinent comment in some detail here.

As a final note, we would like to highlight two changes we have implemented in the paper beyond those described in the replies to the Reviewer. The first is that we have decided to remove Fig. A3. This was the only figure showing year-round geographical anomalies. It was barely mentioned in the text, since our analysis of the geographical anomalies focuses on the rainy season, and upon reviewing the manuscript we did not think it contributed with meaningful information to the overall discussion. The second change is that, in response to comment #3 by Reviewer #2, we have decided to exclude some datapoints from the analysis based on theoretical considerations on the computation of the dynamical systems metrics. This has led to minor changes in some of the figures, but does not alter any of our qualitative conclusions.

Additional References not Cited in the Study

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