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Nonlinear Processes in Geophysics
Discussions

Interactive comment on "Dynamical properties and extremes of Northern Hemisphere climate fields over the past 60 years" *by* Davide Faranda et al.

Anonymous Referee #2

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The authors analyze dynamical properties of daily surface pressure, surface temperature and precipitation rate using two parameters from a universal distribution derived in previous research from extreme value theory of Poincaré recurrences. The two parameters, local dimension of the phase space attractor and the inverse of the average residence time, are physically intuitive, and have the potential to characterize predictability of the analyzed variables.

The manuscript is of interest for both the Dynamical System and Applied Climatology scientific communities, illustrating an approach that could be used in other regions of the world and for other variables of interest. Nonetheless, the analysis raises three

main issues that I think need to be clarified before the paper can be considered for publication in NLPG. In addition, a number of minor changes/clarifications are also suggested to the authors.

Main issues

The manuscript identifies seasonality as a key regulator of the behavior of local dimension and persistence. For example, for surface pressure, two maxima and minima per year are reported, in seasons considerably different (e.g., the typical associated teleconnections are not the same in the two max seasons, with potential implications for the phase-space trajectories and their characteristics). Nonetheless, my understanding is that the composite of anomalies shown in Figure 4 for the maxima and minima are computed from the "extreme" regions in the dimension/persistence diagram (Figure 3, in the example considered here), without discriminating between the two seasons. As a result, I wonder if the present composite analysis is not averaging fields that correspond to different situations. Please clarify this point and the possible differences that could be obtained if analyzing separately the two different maxima and minima.

My second major concern is somehow related to the first one. Although something similar might be happening for precipitation rate (there is only one maximum, but it is so long that different climate agents might be involved, and thus perhaps it makes sense to try to sub-sample that season), I wonder if less noisier results could be obtained if a different precipitation variable is used. As the authors are probably aware, precipitation rate is in general too noisy and far less predictable than accumulated rainfall or rainfall frequency, the latter being the most predictable of all three –at least in most parts of the world. How much Figure 10 changes if frequency of precipitation is used?

I also think that it is adequate that the authors include at least one comment on the fact that considering all grid boxes in the Northern Hemisphere might be "masking" the predictability and/or the dynamical properties of the variables in study, compared to performing the analysis in regions that are known to have a more homogeneous

climatic response to different sources of predictability. In other words: how different are the results if instead of considering the entire Northern Hemisphere, regions of homogenous predictability are considered? Seasonality is different for different regions, so is it possible that mixing the seasonal cycle of so many places degrades the characterization of the dynamical properties of the system?

Minor details (P indicates page number, and L the line number)

P2L12: excise "the" before "attractor"

P2L33: how many are "enough"?

P2L34-P3L1: something is wrong. There is a "." misplaced, and I think the trajectory is around ξ, not around $\theta(\xi)$, which has not been defined yet.

P3L7: I suggest to change "air" by something else. Several of the readers of the paper are going to be fast-readers, and in several other places "air" might be misleading. Perhaps use "t2m", which is one of the standards.

P3L8: what do the authors mean by "peculiar subspace"? Clarify.

P5L2: further explain what is the "length" of a cluster.

P5L15-16: excise the subjective phrasing "a very".

P5L22: maybe the semi-annual cycle is inherited from some semi-annual variability phenomena too? e.g., North Atlantic Sub-tropical High?

P5L28: why is it more restrictive?

P6L16-17: it seems to me that the weakened Aleutian low is actually a high in that figure. Can you comment on that? Also, the magnitudes of the anomalies are really low (amplitude of 10 hPa). This might be associated, again, with the fact that all days are considered together, without seasonal discrimination. The Aleutian low is typical of winter; can the authors comment on why it should be expected to appear during the

maxima of d?

P6L26: please explain why that means intra-seasonal correlation.

P7L11: any hypothesis for why is it absent?

P7L22: explain what do you mean by "high-dimensional situation".

P7L30: it is a bit strange that extremes cannot be matched to large-scale patterns, as they tend to be controlled by synoptic configurations. Maybe what the authors means is that there is no global pattern associated with the extremes? I do not expect that case either. Right now, the sentence is confusing.

P8L12: "Precipitation rate"

P11L9-10: use "twenty" and "fifty", or "20" and "50".

P11L16: along the paper, the authors use 1000 hPa. Did they interpolated? or they just mean "surface". If the latter, please change to "surface".

P11L26-27: I actually expect to see a correlation between pressure gradients and monsoonal precipitation. Maybe I'm not understanding something here.

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