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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Seasonal predictability of the winter precipitation over Iberian Peninsula and its relationship with finite-time Lyapunov exponents" *by* Daniel Garaboa-Paz et al.

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This manuscript proposes an interesting idea of relating dynamical indicators for atmospheric mixing with regional precipitation. By performing a correlation analysis between seasonally averaged summer-time finite-time Lyapunov exponents, winter precipitation and several teleconnection indices, they establish statistical linkages between these variables, which could be further associated with certain climatic processes and physical mechanisms.

While the manuscript contains some potentially relevant findings, some aspects have left me slightly confused and need to be clarified in a revised paper.

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Discussion paper

First, the title refers to "seasonal predictability" of winter precipitation, as opposed to "seasonal prediction". This might be a subtle difference, but the readership of Nonlinear Processes in Geophysics might wish to distinguish between both aspects. The problem is that I did not really find the "seasonal predictability" (as a nonlinear dynamic characteristic) of the winter precipitation records being quantified (rather, one could argue that the FTLE fields discussed provide a means to quantify the spatio-temporally local predictability of atmospheric flow). I am not convinced that at the considered level of seasonal aggregates, it is even possible to quantify the predictabilits of seasonal precipitation sums, given the available time span of observations. On the other hand, I also did not find the aspect of "prediction" being specifically addressed at all (which would essentially mean building a regression(?) model for seasonal precipitation sums based on covariates identified by the performed correlation analysis.

Second, it is appreciated that the authors use dynamical characteristics of the atmospheric circulation to establish a kind of "climatology" in terms of statistical relationships with teleconnection indices. This is most valuable for obtaining a process-based understanding of the observations made. However, it is not clear to me at all why the authors define their four seasons as "JFM", "AMJ", "JAS" and "OND" instead of using the classical - and climatologically well motivated - definitions "DJF", "MAM", "JJA" and "SON". The problem is that when using the terms "summer" and "winter" in the paper, the corresponding definitions do not match what is usually understood by climatologists when using these terms. This makes it hard to establish clear relationships between the findings of the present paper and those of previous works. I strongly recommend revising the results by sticking to the established definitions of seasons.

Third, I recommend giving precise definitions/explanations of how the different types of anomalies used in the paper are calculated. In some cases, this is not obvious from the text and makes evaluating the obtained results quite hard.

Fourth, atmospheric circulation is highly dynamic and involves a multiplicity (actually, a continuum) of spatial and temporal scales. I think that it can be justified to restrict

the attention within the present work to a single atmospheric layer (850 hPa pressure level) and a constant integration time (5 days; this information should be given in the main text instead of a figure caption), but the motivation of both specific choices should be made transparent. I wonder how much the obtained FTLE fields and established statistical relations may depend on the pressure level at which the tracers are initiated. Moreover, how much can we actually learn from time-averaged FTLEs given that Lagrangian coherent structures, hyperbolic trajectories and related objects embedded in the atmospheric flow are not stationary over the seasonal time scales considered in this work? I am willing to accept that the seasonally averaged FTLE fields still provide useful and interpretable information, but what is beyond the mean? For example, does the variance of FTLEs show similar and possibly relevant spatio-temporal patterns? I think that what the authors present is an interesting starting point, but much more could (and should) be done in this regard.

Finally, the authors just report a relationship between summer mixing and winter precipitation, but I do not find information describing a corresponding physical linkage connecting both seasons. At least some speculations about corresponding mechanisms should be given.

Specific comments:

* p.1, l.3: Teleconnection patterns and severe weather (events) have not just evolved during the last years, but are constantly changing.

* When working with wind data, please specific if you consider just the wind speed or the full vector field.

* p.3, l.10: "the significance of this coefficient was assessed to be greater than 95%" is a quite awkward formulation

* p.4, l.19: What do you mean by "lead-lag correlation"?

* p.4, l.20: What is the "North Atlantic East Ocean"?

* p.5, l.1: What is the "JPNA region"?

* p.5, l.33 and below: Please be specific in whether correlations are positive or negative.

* Tab. 1: use capital letters for indicating calendar months

* p.6, l.9: SCA is not the third leading mode of WINTER SLP variability, but can be computed for all seasons (as every teleconnection index).

In addition, the English could be further polished here and there, especially regarding the proper use of articles and (in just a few cases) the consistency of tenses.

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