Nonlinear Processes
in Geophysics

Discussions

Interactive comment on "Compound extremes in a changing climate - a Markov Chain approach" *by* K. Sedlmeier et al.

Anonymous Referee #3

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The authors apply an idea which was previously introduced by Mieruch et al. (2010), studying the sensitivity of Markov chain quantitative descriptors (persistence, recurrence time and Shannon entropy) for compound events (extremes of temperature and precipitation and/or EDI) with respect to: a) different European regions, b) different recent climate-normal periods of 30 years length and c) different periods comparing the present climate and a future climatic scenario. The ensemble variability of descriptors is obtained from ensemble model downscaled simulations.

The proposed method is an interesting approach, among many other possible diagnostics for studying the dynamics and trends of joint (compound) extremes of different physical properties.

C1

Some points must be addressed:

1 – The authors should refer other approaches like the geostatistical analysis of spatially distributed extremes (Neves 2015). That is important because extremes have themselves some spatial organization.

2 – There is no clear justification for the choice of the 6 box-regions and their size (6x6 grid points). Why they are representative of the PRUDENCE regions? Some minimal study about the spatial robustness of the Markov diagnostics should be presented. For example, does the results keep similar or change substantially when contiguous boxes are considered? The ideal should be to present maps of the diagnostics throughout Europe.

Minor corrections

3 - In the entropy definition H (eq. 7), $\log(1/m)$ must be replaced by $\log(m)$ so that H equals 1 for a random system without memory (all probabilities $p_{ij}=1/m$).

4 - Line 189: Authors claim that H between 0 and 1 is an identification of deterministic chaotic behavior. However that condition is necessary but not a sufficient condition for chaos. Authors shall carefully rephrase the paper by taking that into account.

5 - Line 197: Authors say 'The reason for this is that the CO_2 forcing is the only difference...'. In fact, decadal variability is also likely. That sentence must be weakened by replacing 'the only' by 'the main difference beyond the natural decadal variability'.

6 - Eq. 8 – explain the meaning of the bar and subscripts rm.

7 - Line 234: Droughts may have different time scales from months to years. That is the reason for defining the SPI (Standard Precipitation index) (McKee et al. 1993). The presented EDI is appropriate for annual scaled droughts. Add this comment to the text. Moreover the EDI has its own annual cycle since the precipitation weights contributing to EDI are larger near the Julian day d. Does the annual cycle of EDI was removed?

C2

8 - L235-238 Does temperature anomalies (Ta) and precipitation anomalies (Pa) refer to daily Ta and daily Pa with respect to the respective annual cycle. Please clarify. Add a sentence about the number of categories of the Markov chain and what categories of the compound attractor were considered? I suppose that authors have considered 2 parameters with a partition of 2 categories each. Confirm that at this stage for the sake of the paper understanding.

9 - Fig. 3 In the recurrence plot I cannot see the black triangle for region 1.

10 - Fig. 4 In the caption, descriptors' changes refer to changes in the period 1981-2010 with respect to 1951-1980? Rewrite it in a clearer way.

References:

McKee, T.B., Doeskin, N.J., Kleist, J., 1993. The relationship of drought frequency and duration to time scales. Eighth Conf. on Applied Climatology, American Meteorological Society, 179–184.

Neves, M.M. 2015. Geostatistical Analysis in Extremes: An Overview. Mathematics of Energy and Climate Change. Volume 2 of the series CIM Series in Mathematical Sciences pp 229-245. DOI 10.1007/978-3-319-16121-1_10