Answer to comment of referee #3

Compound extremes in a changing climate - a Markov Chain approach

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Dear referee,

Thank you for your detailed review of the paper. In the following, you can find our answers to your comments which are written in red text color.

1 General comments

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.

That is an interesting comment and we will refer to this in the introduction of the revised version.

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.

We thank the referee for that comment, because indeed, we have performed tests on the robustness of the Markov descriptors, which are the basis for the decision to use the actual 6 box-regions. The crucial point, why we have used the 6 box-regions is to achieve that each region contains the exact same amount of grid points / data points. This is of utmost importance for the comparison of the regions, otherwise, if the regions have been chosen with differing sizes no consistent comparison would be possible due to the fact that the Markov descriptors depend on the underlying sample size of the used data. To account for the spatial robustness we calculated the Markov descriptors for every grid point in Europe and visualised the results on a maps. From these maps we have seen that the Markov descriptors vary in general not strongly within the prudence regions. Accordingly we have chosen the 6 box-regions within the Prudence regions, which are representative for the respective region, based on the results of the grid point maps. In the revised version we will include the maps showing the grid point results (see Fig. 1)

2 Minor corrections

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 pij=1/m).

Thank you for the comment but our definition corresponds to those of other papers (see eg. Hill et al., 2004). Maybe you have missed the - sign at the beginning or the "'/"' sign in the equation (log(1/m) is the same as -log(m))? By using log(m) we would get negative entropies with our formula.

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.

We agree with this comment and thank you for the notice. We will rephrase the follwing sentence:
The dynamics of complex chaotic systems lie in between these limits, thus the entropy can be used to identify and characterize complex dynamics like deterministic chaos, which is not possible with standard linear methods
by
The dynamics of complex chaotic systems lie in between these limits, thus the entropy can give a hint to underlying complex dynamics like deterministic chaos, which is not possible with standard linear methods. To really test for deterministic choas other methods, based on state space reconstruction (e.g. estimating the correlation dimension, Lyapunov exponents etc.) to find strange attractors, are more suitable.
and rephrase references to the chaotic behavior accordingly throughout the revised version of the paper.

Line 197: Authors say The reason for this is that the CO2 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.

No, because the crucial point is that this sentence (line 197) refers to the **model runs** (cf. line 198). The decadal variability of the model is not intrinsically changing with time. The only difference between the model runs in the past and in the future is the CO_2 forcing. Thus, changes of the decadal variability are of course possible, but the only reason is a changing CO_2 forcing.

Eq. 8 explain the meaning of the bar and subscripts rm.

Yes, we will do so and also include a more detailed explanation of the EDI in the revised version (also see next comment). The bar in equation 8 stands for the climatological mean - $\overline{EP_{d,rm}}$ refers to the climatological mean state of EP corresponding to day d, where the climatological mean is calculated by a running mean of rm days over the 30 years of the respective time period.

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?

The EDI does not have an annual cycle as this is intrinsically removed by the method (e.g. http://atmos.pknu.ac.kr/~intra2/eng.calculation.htm). In the equation:

$$EDI_d = \frac{EP_d - \overline{EP_{d,rm}}}{\sigma\left(EP - \overline{EP}\right)_d} \tag{1}$$

$\left(\overline{EP}\right)_d$ refers to the climatological mean state of EP and is calculated for each day as the 5day running mean over the 30years of the respective time period. Thus, by subtracting $\left(\overline{EP}\right)_d$ from EP, the annual cycle is removed. We are sorry that this did not become clear and will include a better explanation of the EDI in the methods section of the revised version and clearly state that the annual cycle is removed by the method.

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.

Yes, the Ta and Pa refer to daily temperature and precipitation anomalies with respect to the annual cycle. And we have considered 2 parameteres with a partition of 2 categories each which we then combined to a 4 state symbolic sequence. We will add a more detailed description of the anomaly calculation and partitioning in the methods section.

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

thank you for the notice, we will change that.

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.

We will replace *1951-1980 vs 1981-2010.* by *changes between the time periods 1951-1980 and 1981-2010*

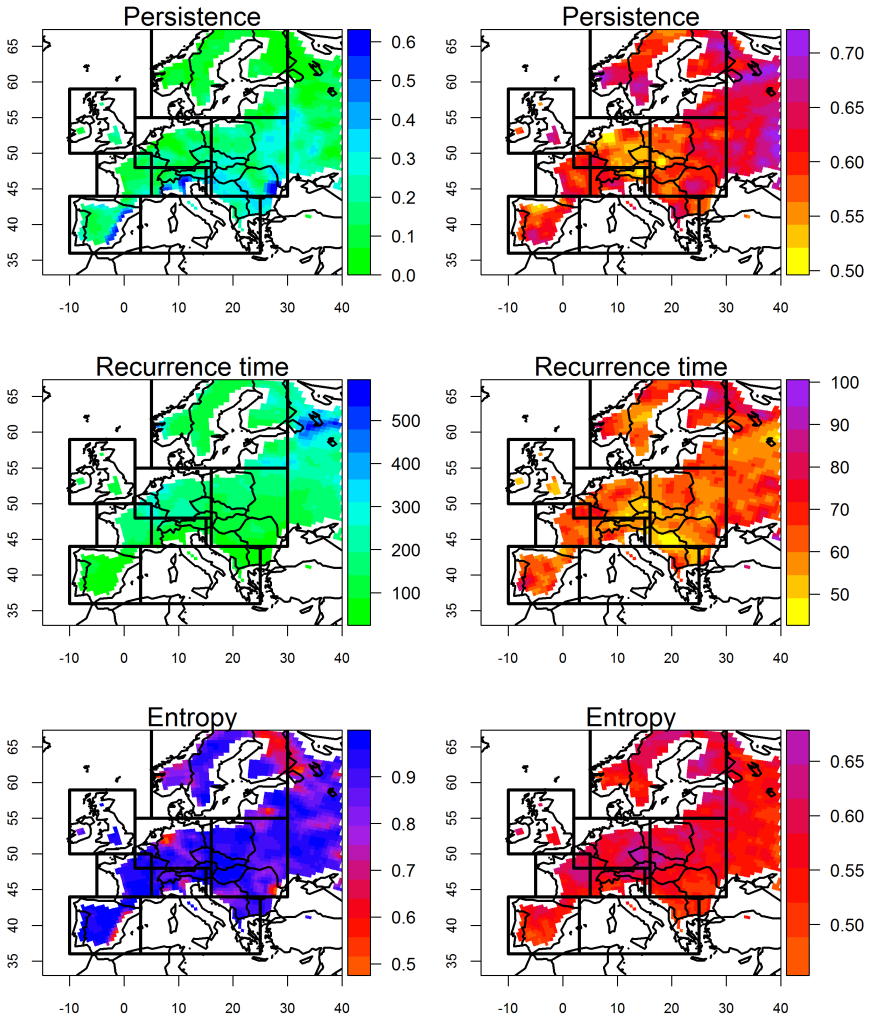

Figure 1: E-Obs descriptors for the reference period (1971-2000). Left side: Descriptors for cold and wet extremes in winter (DJF) (Ta < 10th percentile and Pa > 75th percentile). Right side: Descriptors for hot and dry extremes in Summer (JJA) (Ta > 95th percentile and EDI < 25th percentile). Descriptors were calculated for a moving window over 9 gridpoints and values assigned to the center grid point. Boxes show the Prudence Regions (http://ensemblesrt3.dmi.dk/quicklook/regions.html).

References

Hill, M., J. Witman, and H. Caswell, 2004: Markov chain analysis of succession in a rocky subtidal community. *Am. Nat.*, **164 (2)**, E46–E61, doi:10.1086/422340.