Reviewer#1

I suggest that this article is accepted for publishing. I recommend to the authors to include the responses to my first and second question from the first review (on the possible existence of crossovers for the entire and maybe longer series, and on the need for theoretical error estimate) in the discussion of the manuscript, for I find this will be of interest to the special issue readership.

Reply: Thank you. We have now added the following text at the end of Conclusions section:

“Moreover, longer series should be used in future works to perform GWPE analysis in sliding windows, to address the potential crossovers and long-term trends in predictability.”

Moreover, theoretical error estimate does not appear to be feasible at the current level of theoretical development (see a more detailed reply to the last point raised by Reviewer#2, below).

Reviewer#2

Thanks for addressing the comments I provided in the first round of review. I appreciate the efforts the authors have made in revising the manuscript. However, after carefully reviewing the revised version, I still find several major issues that require further clarification or improvement. I hope the following suggestions will help you further refine the paper and enhance its scientific quality and depth.

1. The authors state that “…, during the sub-period 1986–2010, (which overlap with our second subperiod from 1991 to 2020) an increasing trend in annual precipitation was observed at numerous stations, … Notably, our analysis revealed enhanced predictability of small-scale precipitation fluctuations at these locations, suggesting a possible link between rising precipitation and improved short-term predictability.” However, the trend from 1961-1990 to 1991–2010 is inherently a long-term low-frequency signal, whereas the temporal scale of the so-called “small-scale fluctuations” is never clearly defined in the manuscript. This raises an important question: why would a long-term trend specifically affect the predictability of small-scale fluctuations? Are these fluctuations actually associated with low-frequency variability as well? To substantiate this claim, the authors should provide additional analyses—such as examining the characteristic timescales of the small-scale fluctuations (e.g., via spectral analysis, variance decomposition, or autocorrelation)—and investigate whether their statistical properties (e.g., variance, memory, or scaling behavior) co-evolve with the long-term trend. Currently, the argument rests on a single speculative sentence, which is insufficient to support such a mechanistic interpretation. Clarifying this relationship is essential to establish a physically meaningful interpretation of how long-term trends influence small-scale predictability.

Reply: The increasing trend in annual precipitation in Belgrade, Kraljevo, and Smederevska Palanka was observed by Ruml et al. (2016), while in this work we find that entropy of small-scale (low variance, emphasized for q=-10) for short-term (w=6 days) predictability has decreased between the two periods. This is a phenomenological finding, suggestive of a possible link between rising precipitation and improved short-term predictability. While this is an intriguing observational fact (perhaps to be addressed in more detail in future works), it does not represent one of the main points of our paper, and we feel that additional analyses suggested by the Reviewer would draw attention of the reader from the overall phenomenological findings of this manuscript. We have therefore removed this text from the manuscript.

2. A related concern is the conceptual and terminological imprecision surrounding the so-called "trend." The manuscript defines this "trend" simply as the difference in mean annual precipitation between two predefined periods (e.g., 1991–2000 vs. 2001–2010), rather than identifying an actual temporal trend through statistical trend detection (e.g., Mann-Kendall test, linear regression, or change-point analysis). Using the term "trend" in this context is therefore misleading, as it implies a continuous directional change over time, whereas the analysis only compares two static climatological means.
More importantly, the authors themselves acknowledge that the link between this "trend" and changes in small-scale predictability may be coincidental. Yet they provide no systematic exploration of when or where such a relationship holds—or fails to hold—despite noting "decoupling" at certain stations (lines 225-235). If the connection is indeed inconsistent or non-robust, then merely reporting its existence (or possible absence) without deeper diagnostic analysis adds little scientific value: it neither supports a physical mechanism nor refutes one.
In such cases, the authors should either:
(1) conduct a more rigorous assessment of whether the precipitation shift is statistically significant and temporally coherent;
(2) investigate the conditions under which predictability responds (or does not respond) to mean-state changes—e.g., by stratifying stations by region, magnitude of change, or hydroclimatic regime; or
(3) reconsider whether this comparison belongs in the core narrative at all, unless it can be meaningfully tied to the paper's central question on predictability dynamics.

Reply: In connection with the previous point raised by the Reviewer, we choose option (3), and have now removed this comparison, together with the citation Ruml et al. (2016). Instead, we have commented on local factors that play a role in shaping spatial and temporal precipitation patterns, and two new reference (Putnikovic et al., 2016; Amiri and Gocic, 2025).

3. Line 60: I appreciate the additional methodological details provided in the revised manuscript. However, the structural gap between CECP and GWPE remains unaddressed. Lines 40–55 offer a thorough introduction to CECP and its applications in precipitation studies, but from Line 60 onward, the text abruptly shifts to GWPE without explaining how the two methods are related. For instance: Is CECP particularly well-suited for implementation within the GWPE framework? Does GWPE offer unique advantages for conducting CECP-based analyses (e.g., better handling of nonlinearity or multiscale interactions)? Or does combining GWPE with CECP yield enhanced predictive skill or reveal additional dynamical features?

Reply: We have now improved the text (lines 59-64) to address this issue: PE is a special case of GWPE, and CECP is a special case of GWCECP, for q=0. We have also added at the end of the

Section 2.2 the statement: "Source code in C, Python and R for application of GWCECP (GWPE and GWPEC) is available at https://github.com/stosicresearch/gwpentropy."

4. Figures 2, 3, 8, 9 and Tables 2, 3: The authors interpret the differences between the two periods as a "trend," but no statistical significance testing is provided. This is a notable limitation. For example, Figure 2 shows that some stations (e.g., NE and VG) exhibit decreased precipitation, yet Figure 3 displays only positive anomalies with a colorbar that implies universal increase—potentially misleading readers.

Reply: Thank you for this observation. We have now improved the text (lines 157-158) and the legend of Figure 3.

Similarly, Figures 8–9 and Tables 2–3 report changes in predictability metrics without indicating which differences are statistically robust. I strongly recommend that the authors: Perform appropriate significance tests (e.g., bootstrapping, t-tests, or field significance corrections); Clearly mark statistically significant regions in the figures (e.g., hatching); and/or Discuss in the text which observed changes are likely meaningful versus those that may arise from noise. Adding this layer of statistical rigor is essential for credible interpretation and would substantially strengthen the paper's conclusions.

Reply: The abundant literature with application of CECP rarely deals with significance tests because construction of surrogate series requires a specific choice of the null hypothesis (e.g. for distinguishing noise from chaos). This situation is further complicated for GWCECP as the powers of the variance of the ordinal patterns enter the probability definition (Eq. 6 in the manuscript). The issue of construction of surrogate series for GWCECP has not yet been addressed in the literature.

Rather than performing explicit significance tests, the foundational CECP work (with implications for significance analyses) Rosso et al. 2007 introduces the Complexity–Entropy Causality Plane (CECP) as a discriminative representation to separate chaotic vs stochastic dynamics, which inherently involves testing whether observed (H, C) points lie outside null regions expected under random noise, represented by the right hand vortex in the CECP plane (H=1.0, C=0.0).

While we agree with the Reviewer that adding this layer of statistical rigor would substantially strengthen the paper's conclusions, it does not appear to be feasible at the current level of theoretical development.