Referee report on ‘Improvement of the Rnnmm type climate index approach with a spatio-temporal model based on the Hawkes process’ submitted to EGUsphere with manuscript ID egusphere-2025-2542

My following comments are a mix of major and minor comments.

1. Minor: In the title, “Rnnmm type” should be “Rnnmm-type”.

2. Major: While the article addresses an environmental statistics problem, the main contribution appears to be on the statistical side. However, the introduction cites only a few papers, mostly by the authors, and attempts to convince the reader that the existing literature is inappropriate for the problem discussed in the paper. Near the end of the Section, the authors simply claim to introduce an innovative self-exciting Hawkes process, without citing any papers or providing a proper literature review of their proposal. The introduction suggests the authors are introducing the self-exciting Hawkes process for the first time. The approach was proposed by Hawkes in the Biometrika paper “Spectra of some self-exciting and mutually exciting point processes” in 1971, more than fifty years ago.

   A clearly written paragraph including an appropriate literature survey on the self-exciting Hawkes process must be provided in the introduction section. Not only the Hawkes process, but also its spatio-temporal versions are common in the literature. For example, a review article on this topic is “A Review of Self-Exciting Spatio-Temporal Point Processes and Their Applications”, by Alex Reinhart, published in Statistical Science in 2018.

   The authors should clearly indicate what their novel contribution is from a statistical perspective, or they should simply demonstrate the usefulness of an existing statistical method in the context of climate extremes.

3. Minor: The full form of IMERG is not introduced.

4. Major: Overall, the proposed model is a latent Gaussian model, where separate self-exciting Hawkes processes are used to model individual time series across locations, and then the potentially transformed spatially varying coefficients are modeled using Gaussian processes. In this approach, conditioning on the model coefficients, the data are modeled spatially independently. However, a convolution through a Gaussian process does not introduce extremal dependence. The authors can refer to a large statistical literature on spatial extremes in this regard. Hence, as a spatiotemporal model for inferring spatially varying coefficients, this approach may be better suited, but may not be from a spatial-extreme perspective.

5. Major: The authors choose gamma priors for the variance-related hyperparameters, while an inverse-gamma prior would be conjugate. The justification for choosing a non-conjugate prior should be provided. In the algorithm, the authors mention Step 2 as GI. What does it mean? Inverse-gamma? If so, the usual notation is IG.

6. Major: The manuscript does not include details on the MCMC diagnostics. Besides, no simulation study has been shown. While I agree that EGUsphere is an environmental sciences-focused journal, such details should be provided in the supplement, as the main focus of the manuscript is statistical modeling. Given that there are only 20 locations, I am highly curious about the MCMC chains for the spatial dependence parameters.

7. Minor: I feel that presenting some tables in a horizontal fashion rather than in a vertical fashion (like now) would look better.

8. Minor: Although the authors claim to provide an extensive cross-validation analysis, my concern is that they draw this conclusion based on a very limited dataset. Given that no extensive simulation has been shown, I think the word “extensive” should be toned down unless a larger number of spatial locations or a larger spatial domain is used.