Response to Reviewer #1

Manuscript Title: Hourly-Scale Modeling of Storm Transitions in Southern Brazil with Markov Chains

We sincerely thank the reviewer for their careful evaluation and constructive comments, which have helped improve the quality and clarity of the manuscript. Below, we provide a detailed response to each comment.

Comments of the reviewer #1

The article is well written and well structured. However, there are still some points that deserve attention in the manuscript review.

1. Abstract: The abstract presents an excess of numerical results, which in some sections could be described in a more qualitative way, in order to avoid overloading information in a synthesis that should be concise. In addition, I recommend that the research objective be clearly separated from the methodology employed in the second sentence of the abstract.

R/ Thank you for your comment; the summary has been improved.

2. Introduction: The first and second paragraphs lack more robust justifications for the development of the study. Expand the discussion on the trends observed in precipitation extremes in the Southern Region of Brazil over the last few decades, incorporating more recent references.

Furthermore, it is important to specify which main atmospheric systems are associated with these events, such as Mesoscale Convective Complexes, the South American Low-Level Jet, and frontal systems, among others. Although these aspects are explored in depth in the results, a brief mention in the Introduction would further clarify the study's physical framing.

It is recommended to consider, for example, the following references:

- https://doi.org/10.1007/s00704-007-0329-x
- https://doi.org/10.1002/joc.5031
- https://doi.org/10.1002/joc.7119
- https://doi.org/10.1080/02626667.2020.1863969
- https://doi.org/10.1002/joc.7911

Reorganize the last paragraph of the Introduction to more clearly separate the motivation for the work from the specific objectives of the study.

Although the research is based on hourly-scale data, this scale is not conceptually explored in the Introduction; in this context, the concept of nowcasting could be mentioned.

R/ Thank you for your comment; the introduction has been improved.

3. Study Area: In Table 1, the meaning of the variable "desvest" is unclear and should be explained. In addition, the average precipitation figure needs clarification: is it an hourly average, right? Carefully review this data and detail your findings better in the text.

R/ Thank you for your comment; Table 1 has been revised.

4. The relationship between storm occurrence and the geomorphology of the study area could also be discussed in greater depth.

R/ Thank you for your comment; the discussion has been improved.

5. Methodology: It is necessary to clarify which references supported the choice of thresholds corresponding to the 95th and 99th percentiles. In this context, the climate extremes indices proposed by ETCCDI can be mentioned, for example.

R/ Thank you for your comment; this methodological aspect has been addressed.

6. Is the definition of the 5 mm threshold for characterizing storms widely adopted in the literature? How are storms with high precipitation rates in short intervals, such as 15 minutes, treated? In addition, the consideration of wind gusts could be discussed, since wind can precede precipitation and cause significant damage, operationally characterizing a storm.

R/ We thank the reviewer for this insightful comment. The storm definition adopted in this study, based on a minimum accumulated precipitation threshold of 5 mm and a separation period of 6 hours without rainfall, follows commonly used criteria in event-based precipitation analyses to ensure the identification of independent and hydrologically relevant events (e.g., Lamjiri et al., 2017; Palecki et al., 2005).

Regarding short-duration, high-intensity events (e.g., 15-minute bursts), the hourly temporal resolution of the dataset does not allow their explicit identification as independent events. However, their contribution is inherently captured within the hourly aggregation, particularly through the maximum intensity variable.

With respect to wind gusts, we agree that they are relevant for operational definitions of storms. Nevertheless, the scope of this study is focused on precipitation-driven processes, which are directly linked to hydrological impacts such as runoff generation and flooding. Therefore, wind-related variables were not considered.

7. In line 121, it is unclear how the stations were grouped; was any clustering method used?

R/ We thank the reviewer for this comment. The paragraph has been revised to avoid any implication that a clustering method was applied, and to clearly indicate that the analysis was purely exploratory.

8. In line 174, the stationarity of the process is assumed, but it is unclear whether it was tested or merely postulated.

R/ We thank the reviewer for this comment. The stationarity of the process was not assumed but evaluated through chi-square tests of temporal homogeneity for each station and season. The results ($p > 0.05$) indicate stable transition probabilities over time, supporting the use of time-homogeneous Markov chains. The manuscript has been revised to clarify this point.

9. Results: In Figure 4, the distinction between continent and ocean does not appear.

R/ Thank you for your comment. The map has been updated.

10. In line 207, it is worth noting that extreme events are not necessarily outliers in the physical sense; in fact, it is possible to estimate outliers within the set of extreme events.

R/ We thank the reviewer for this comment. The text has been revised to avoid confusion between outliers and extreme events.

11. In Figure 5, it would be interesting to include the annual boxplot as well. I suggest evaluating the possibility of reorganizing the figures by precipitation intensity classes to make the comparison between seasons more efficient.

R/ Thank you for your comment. We have updated Figure 5 with the annual box plot and revised the analysis.

12. The analysis regarding autumn was poorly explored. The transitional nature of this season could be better discussed, with a focus on the fact that the escalation to severe states is statistically rare during this period.

R/ We thank the reviewer for this suggestion. The discussion of the fall season has been expanded to better highlight its transitional nature, emphasizing that the escalation toward severe states is statistically rare during this period.

13. In line 302, it is emphasized that not only the frontal systems themselves should be considered; Spring sometimes experiences a higher frequency of cold fronts.

Therefore, it is important to highlight the role of more stable air associated with migrating anticyclones.

R/ We thank the reviewer for this comment. The manuscript has been revised to highlight the role of stable air masses associated with migrating anticyclones, in addition to frontal systems.

14. In line 315, also mention the katabatic flow of the Andes and its interaction with the LLJ, which favors the occurrence of Mesoscale Convective Complexes in the region.

R/ We thank the reviewer for this suggestion. The manuscript has been revised to include the role of downslope flow from the Andes and its interaction with the LLJ in favoring Mesoscale Convective Complexes.

15. Carefully review the use of abbreviations throughout the text, ensuring that all are defined on their first occurrence (for example, LLJ in lines 299 and 315).

R/ We thank the reviewer for this comment. The manuscript has been carefully revised to ensure that all abbreviations are defined at their first occurrence.

16. Conclusions: The conclusions are relatively weak and do not clearly demonstrate the study's scientific novelty. I recommend reducing the presentation of numerical values, which have already been extensively discussed in the results section, and emphasizing the work's original contributions and its specific practical implications more objectively.

R/ Thank you for your comment; the conclusions has been improved.