“Mechanisms and scenarios of the unprecedent flooding event in South Brazil 2024”

The authors examine the May 2024 flood in the Patos Lagoon basin in southern Brazil by implementing HEC-RAS 2D. The hydrodynamic model is calibrated and evaluated by comparing their simulations with water level records from gauge stations along the tributary rivers, remote-sensing-based data (e.g., water surface elevation from SWOT and NDWI from RapidEye’s images), and field measurements conducted with an Acoustic Doppler Current Profile (ADCP). The results show satisfactory performance of the model compared to the references. Based on the simulations, the authors conclude that the Jacuí and Taquiri rivers are the main contributors to the flooding in May 2024.

The manuscript shows promising results, but I have several comments that need to be addressed (e.g., methodological improvements, restructuring of some sections, etc.) before the manuscript can be considered for publication. My main concern is that the manuscript's goal is not fully addressed, and the experimental design does not allow conclusions to be drawn regarding enhancing our understanding of flooding mechanisms in South Brazil. I hope my comments and feedback help the authors highlight the great work they have done so far.

Main comments:

In the following points, I summarize the observations derived from my review of the paper, all of which aimed to improve the author’s contribution.

- Take-home messages are missing in the abstract. For example, the authors mentioned “major lessons” but without providing further thoughts about them. I recommend including some of the main conclusions of the study.
- In the introduction, an improvement in the literature review is needed to highlight the relevance of this topic and the local context. Here are some points that I am missing: (i) an overview of what is known about flood generation mechanisms in Brazil, (ii) highlight the need to improve flood modeling and keep the consistency in the examples provided (e.g., you mentioned LISFLOOD with a UK flood and the other examples are focus on Brazil- which I considered reasonable and consistent with the study goals), (iii) frame the manuscript as a case study (which is clearer), and some thoughts about to what extent their results are potentially extrapolatable to other regions (this last point should be revisited later in the discussion).
- Continuing with the introduction, I recommend shortening some paragraphs to improve the clarity of the document. The paragraph related to climate change can easily be combined with paragraph one and shortened to 2-3 sentences, as the focus of this manuscript is not climate change, but rather the event of May 2025. In this context, climate change is a motivation (or rather a concern) to improve our understanding of extreme events in a changing climate. To further complement this necessity, you could also include a brief mention of the concepts being discussed today in the community related to compound events (e.g., Heinrich et al., 2023; Hendry et al., 2019, Leonard et al., 2014), hydroclimatic volatility (e.g., Swain et al., 2025), and hydrological volatility (e.g., Hammond et al., 2025).
- After reading the manuscript several times, I think that the objective of the document is misleading, particularly for the use of the word “mechanisms”. Studies analyzing flooding mechanisms focus on, e.g., interactions of hydrometeorological processes, dominant processes, their relationship with flooding magnitude and timing, among others (see, e.g., the study by Jiang et al. (2022) and some of those cited in its introduction). Here, only the contribution of streamflow from different tributaries is being considered, without a deeper understanding of the processes occurring in each of them. In this context, questions arise such as: How sensitive is the response of each catchment to changes in precipitation/temperature,

and how does this propagate downstream (both in magnitude and timing)? When does the regulating effect of the catchment not play a key role anymore? Under what conditions can peak flow synchronization occur? Based on your results, it is clearer to me that the questions you are addressing are those that arose regarding the function of the natural system after the disaster (referring to Hunt et al., 2024; Silva et al., 2024a).

- Following on from the previous point, my recommendation is: (i) rewrite the objective of the study and align it with the questions that arose after the May 2025 flood, or (ii) deepen the analysis to improve understanding of the processes and interactions that shape the characteristics of the flood under study.
- How stable is the riverbed of the rivers studied? I suggest improving the discussion regarding the representativity of the bathymetry (i.e., changes in the riverbed).
- Regarding the databases used, is there information that allows for the uncertainty of each one to be incorporated? This is especially important for products based on remote sensing. However, for altimetry, it would also be important to have information related to the discharge curve associated with each station and its characteristics (i.e., maximum height) to understand the scope of the extrapolated records.
- In the flood modeling part, it is not clear to me how the authors are including the intermediate contribution of water to the flood. In other words, how are contributions to the modeling grid considered? I understand that the streamflow input and output are fixed boundary conditions, but are the rest of the values purely given by the numerical closure of the simulations?
- When calibrating the model, what is calibrated? Only Manning's roughness? How is this calibration performed? Is there an a priori spatial distribution assumption, and then a super-parameter is calibrated to reduce dimensionality (i.e., regularization)? How sensitive are the results to the selected parameters?
- The explanation of the experiments could be improved a little. It isn't easy to follow the experiments and then the scenarios (which contain each other). Additionally, what justifies these scenarios? How feasible are they? How is the downstream influence that the ocean could have on the channels designed to discharge there?
- Results associated with water level simulation are challenging to interpret. I don't know how different the damages can be if we have a bias of 1 or 10 m. I recommend that the authors refer to the level, maybe to, e.g., the riverbanks, to present the results in terms of river flooding potential (or include a line in the plots showing the riverbanks' height). This could help to highlight the results from a hazard perspective.
- In Figure 4, instead of presenting 12 panels, why not show two examples (panels a-b) and then a third panel (c) with a box plot showing the absolute error in time in each of the simulations? Or maybe a plot showing the NSE, RMSE, and BIAS. Note that the use of NSE provides the same information as RMSE. Additionally, how are differences in level translated into total flood volume and maximum flows?
- Considering the availability of a gridded product for water level (SWOT), why was it not considered to present a map of differences with the simulations?
- The flood extension figure (Figure 6) does not allow for analysis of the results. The base map makes it difficult to distinguish the blue lines representing the HEC-RAS simulation. Within each panel, it would be very informative to include the PlanetScope area, HEC-RAS (blue color), and the difference between the two.
- The verification of streamflow (Figure 7) should also be done with the streamflow series recorded by the stations shown in Figure 2. In addition, uncertainty bands should be included in ADCP measurements to make the comparison fairer.
- Figure 8 could be improved by changing the focus of the analysis. Instead of removing one tributary at a time, I think that testing each one independently (by "turning off" the rest) would provide more information. This is because the sum of the tributaries (routed to the control point) should be equivalent to the observed flood event. With that, you can have a stacked area chart where, for each time step, you know the relative contribution of each of the basins.

As a reference to what I meant (applied in a different context, not related to floods), you can see Figure 7 in Ayala et al. (2020).

- How (physically) feasible are the river flood synchronization scenarios? I think this scenario is exciting, but it would be good to explore the likelihood of this happening in more depth (I hypothesize that it would be closely related to the type of storm and its spatial distribution).
- As the results showed that the proposed hydraulic interventions would have a limited benefit, why don't the authors remove the analysis from the main manuscript? It is unclear how these scenarios are formulated or how feasible they are in technical, economic, and other terms (I suppose certain environmental agencies would raise concerns about the construction of a channel connecting the lagoon to the ocean). To better understand the proposed modifications, it would be beneficial to justify them and explore alternative solutions that offer a significant benefit in alleviating flooding in the area.
- The clarity and readability of the manuscript could be improved by splitting the results section from the discussion. Currently, the description of the results is overshadowed by the discussion.
- The findings presented support points (i) and (ii) of the conclusions, but not the second sentence of point (iii) (L428-429). The low contribution of the proposed hydraulic solutions may be linked to the design, the characteristics of the flood, and other factors. There is insufficient information to conclude that location is a determining factor. I recommend rewriting that idea to clarify the point you are trying to make.
- The paragraph between L430-434 should be included in the discussion (limitations) rather than in the conclusions.
- The statements between L440-442 go beyond what is presented in the manuscript. What could be highlighted – instead of mentioning the idea of "serve as a benchmark" - is the incorporation of different sources of information for the evaluation of the modeling. I recommend rewriting this paragraph to highlight the need for verification and constraining parameters in numerical models, based on the incorporation of complementary information to enhance realism and fidelity in simulations.

Minor comments:

- In Figure 2, consider including the points where the ADCP measurements are available.
- For all the figures, check the readability of the labels and (maybe) consider reducing their 'multidimensionality' to guide the readers straight to the point you want to make.
- L42: "In instance" → **For** instance.
- L205: "Finally, a set of hydraulic intervention**s** experiments was **organized**" → Finally, hydraulic intervention experiments were tested.
- L299: "… the peak water would lower **xx** meters to 4.75 meters,…" → typo + verb is missing
- L417: "Our findings **address the following scientific questions**:…"→ Our findings are summarized as follow:

References

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