Response letter

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RC2

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.

We sincerely thank the reviewer for their constructive suggestions and effort, which have helped improve our study. We also appreciate their recognition of our work's scientific contribution.

Based on this valuable feedback, we have substantially revised the manuscript to enhance its clarity and better highlight our research questions and main objectives. Specifically, we restructured the Introduction to more clearly present our study's motivations and goals. We also expanded the Methodology section, adding new subsections to detail our analysis of synchronization and the proposed intervention structures. Furthermore, we have separated the Results and Discussion into two distinct sections, significantly expanding the latter to compare our findings with existing literature and better demonstrate the impact of our work.

We are confident that these revisions have significantly strengthened the manuscript and that they now meet the high standards required for publication.

• 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.

Response: Thank you for your suggestion. We have revised the abstract and included more takeaway messages to better contextualize the manuscript. The abstract now reads as follows:

"In May 2024, an extraordinary precipitation event in southern Brazil triggered record floods in South Brazil, specially over a complex system that includes rivers as Jacuí and Taquari, draining into Guaíba and Patos Lagoon. It resulted in unprecedented impacts on local population and

infrastructure. Considering past observations and projections indicating an increase in flood events in the region due to climate change, understanding the flooding processes in the region is essential for better preparing cities for future events like the May 2024 flood. In this context, hydrodynamic modelling serves as an important tool for reproducing and analyzing this past extreme event. This paper presents the first detailed hydrodynamic assessment of the unprecedented flood that occurred in Southern Brazil in 2024, the worst natural disaster in Brazilian history. We also performed the first validation of a detailed hydrodynamic model using new observations from the SWOT satellite. The main mechanisms governing this flood disaster were investigated, and we accessed scenarios of hydraulic interventions for flood control that are currently under public debate. The focus is on the most populated areas in the Metropolitan region of Porto Alegre (RMPA) capital city. The results demonstrated that the model accurately represents the event, with average NSE, RMSE and BIAS of 0.82, 0.71 meters and -0.47 meters, respectively, across the main rivers in the basin. Furthermore, the flood extent simulation represented 83% of the affected area, as compared to high-resolution satellite images. Our analysis of the mechanisms that influenced the event showed that the Taquari River was the main responsible for the peak in the RMPA, while the Jacuí River contributed the most to the duration of the flood. Additionally, the synchronization of the flood peaks from both rivers could have increased water levels by 0.82 meters. Evaluated hydraulic interventions for flood mitigation demonstrated that the effectiveness of the proposed measures varied by location, with usually low influence in the RMPA water levels (lower than 0.38 m). By accurately assessing the May 2024 flood, this study enhances the understanding of a complex river-estuary-lagoon system and demonstrates adverse scenarios and the limitations of possible hydraulic structure interventions. Finally, modelling this unprecedented event offers valuable insights for future research and global flood management policies.”

• 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).

Response: We thank the reviewer for their relevant thoughts. We agree that the introduction needs to be improved, and the points raised are important and relevant to our study. We have revised the Introduction to improve the literature review and contextualization of our study, and we also structure our research paper as more a case study. Specifically, we now (i) revisit previous studies on flood generation mechanisms in Brazil; (ii) highlight the potential of hydrodynamic models for evaluating flooding scenarios and the efficiency of possible and actual flood protection structures; and (iii) indicating in the introduction that the insights from this study are therefore highly relevant for other regions that are complexly controlled by multiple systems, such as river-estuary-lagoons.

We updated the manuscript with the following paragraphs:

Overview of flood generation mechanisms in Brazil:

"[…] *Floods in southern Brazil, situated at the sub-tropical and temperate portions of South America, have increase significant in recent decades, which has been supported by both historical data and climate projections (Ávila et al., 2016; Bartiko et al., 2019; Brêda et al., 2023; Chagas et al., 2022). Nationally, flood generation in Brazil is driven by a variety of mechanisms. These include intense convective storms causing urban flash floods* (Cavalcante et al., 2020; Lima & Barbosa, 2019; Marengo et al., 2023) *persistent rainfall associated with South Atlantic Convergence Zone (SACZ) leading to large-scale riverine floods, and the influence of major teleconnections like the El Niño-Southern Oscillation (ENSO). In the southern region, specifically, the primary drivers are often intense frontal systems that bring widespread and prolonged precipitation* (Ávila et al., 2016; Damião Mendes & Cavalcanti, 2014) *[…]".*

Regarding potential of hydrodynamic models:

"[…] For instance, these models are particularly useful for studying complex interactions in medium-to-large basins, where precipitation is expected to become more concentrated. In these coupled systems, the synchrony between the peak flows of major tributaries and the estuary–lagoon water level is a primary determinant of flood severity, directly informing the timing and feasibility of structural and operational measures. While previous studies have often focused on individual rivers or local interventions, few have examined synchrony and mitigation within an integrated, river–estuary–lagoon framework at regional scale. Moreover, simulating flood mitigation scenarios is essential for evaluating interventions, defining optimal locations for new structures, assessing the efficiency of existing ones, and identifying areas of high risk […]

Potential to extrapolate for other regions:

[…] The insights from this study are therefore highly relevant for other complex, large-scale hydrodynamic coastal and deltaic regions. […]

• 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).

Response: We appreciate the reviewer's further suggestions for our manuscript. We agree with the points raised and have reshaped the second and third paragraphs of the manuscript by shortening them and highlighting key points. The revised paragraphs are as follows:

"*Floods in southern Brazil, situated at the sub-tropical and temperate portions of South America, have increase significant in recent decades, which has been supported by both historical data and climate projections (Ávila et al., 2016; Bartiko et al., 2019; Brêda et al., 2023; Chagas et al., 2022). Nationally, flood generation in Brazil is driven by a variety of mechanisms. These include intense convective storms causing urban flash floods (Marengo et al., 2023; Cavalcante et al., 2020; Lima et al., 2019), persistent rainfall associated with South Atlantic Convergence Zone (SACZ) leading to large-scale riverine floods, and the influence of major teleconnections like the*

El Niño-Southern Oscillation (ENSO). In the southern region, specifically, the primary drivers are often intense frontal systems that bring widespread and prolonged precipitation (Mendes and Cavalcanti, 2014; Avila et al., 2016). Moreover, climate change is further intensifying this scenario by increasing hydroclimate and hydrological volatility (Swain et al., 2025; Hammond et al., 2025; Stevenson et al., 2022) and altering these flood-generating mechanisms, which raises the frequency and severity of floods, particularly through compound events (Leonard et al., 2014; Hendry et al., 2019; Heinrich et al., 2023)."

• 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).

Response: We sincerely thank the reviewer for the comment, and we agree that the objective of the document is not clear. We have revised the manuscript and inserted in the introduction the main objective of our study for clarification, as detailed in the previous revisions. Regarding the use of the word "mechanisms", we are here referred of the flood processes thar main control the basin and how they could be different from a hydrodynamic perspective, including the flood peaks synchronicity and the relevance of rivers. We acknowledge that the word "mechanisms" was used before to refer to other process. We also understand that this previous usage is not excluded from the processes that we have assessed.

Especially regarding the question "Under what conditions can peak flow synchronization occur?" the answer is relatively simple. The occurrence of precipitations in one region of the watershed and at another could be delayed from a meteorological point of view. We investigated this delay in our study, but looking at the flow routing from rivers.

• 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.

Response: We thank the reviewer for their contributions. We rewrite the study's objectives as follows:

"This study addresses urgent and unresolved questions raised by the May-2024 flood regarding (a) the relative influence of tributary inflows on RMPA water levels and inundation, (b) the consequences of potential peak synchrony between the main rivers, and (c) whether additional

lagoon–ocean outlets or channel operations would have mitigated upstream flooding. Prior studies did not jointly address these system-scale dynamics due to limited integrated datasets and validation across the river–estuary–lagoon continuum. Leveraging detailed bathymetry, ADCP transects, continuous gauges, satellite flood extent, and SWOT altimetry (Biancamaria et al., 2016; Fu et al., 2024), we develop and validate a 2D hydrodynamic model to quantify mechanisms and test counterfactual scenarios. This design yields decision-relevant evidence for stakeholders and government agencies seeking to enhance protection in the most affected areas, and ultimately allowed for the comprehension of how this unique natural system works under extreme conditions. The insights from this study are therefore highly relevant for other complex, large-scale hydrodynamic coastal and deltaic regions."

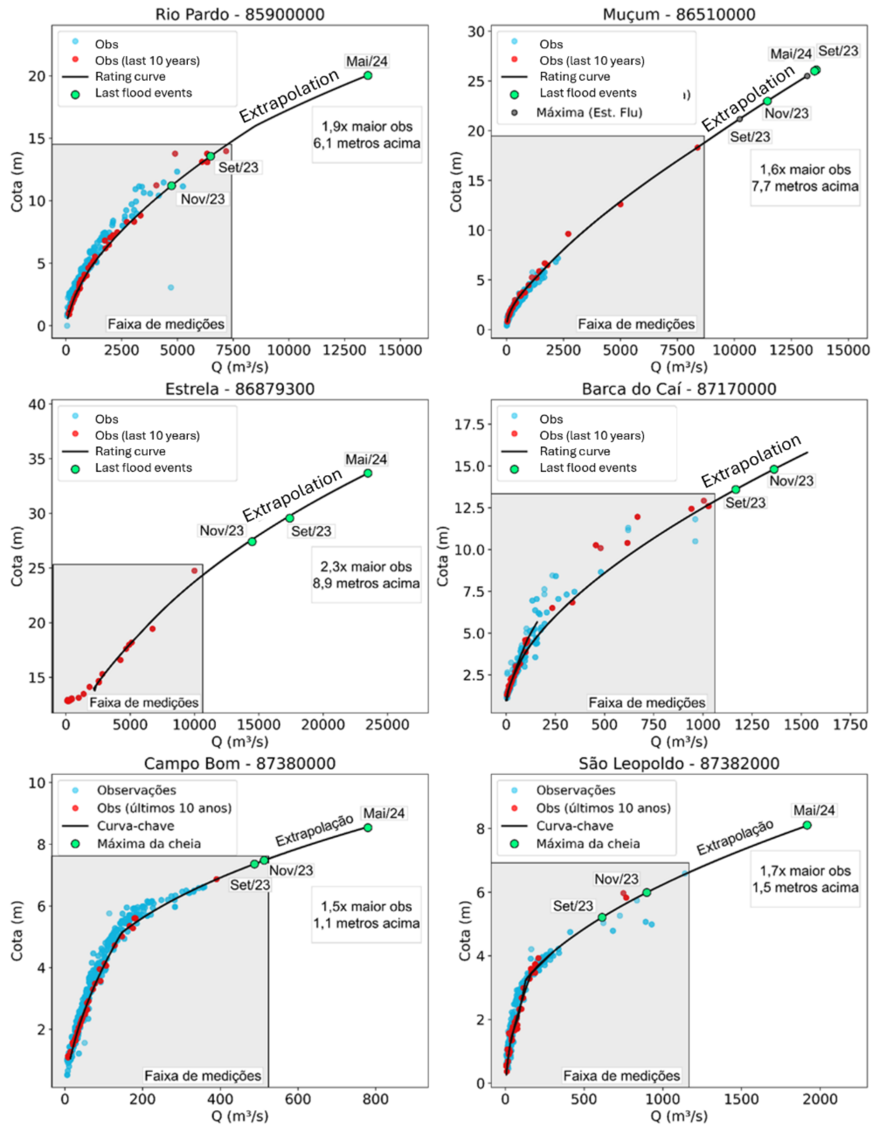
• How stable is the riverbed of the rivers studied? I suggest improving the discussion regarding the representativity of the bathymetry (i.e., changes in the riverbed).

Response: Changes to the riverbeds of the analyzed rivers appear to have a negligible impact on the study's results, as they have, on average, remained largely stable over the years. Specifically, a report (Collischonn et al., 2025) compared bathymetry before and after the flood and found only minor changes (on the order of 10 centimeters). These morphological changes are insignificant when compared to the magnitude of the flood, which involved water level rises exceeding 30 meters.

• 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.

Response: We do not have all the necessary information regarding the uncertainty of each data incorporated in the simulation, whereas the discharge curve for the weather stations does not necessarily have the necessary data/or are updated for newest data. Nevertheless, we will incorporate this relevant topic into the discussion, regarding the uncertainties of each data (SWOT, satellite-derived) and addressing the implication of discharge uncertainty in our model forcing and how this could potentially affect our results.

For instance, we include following the preliminary assessment of the rating curves from main river stations in the watershed. One can see that some of the flows are from the extrapolation stretches of the curve. For the final version of the manuscript, we will further work using this data to discuss the uncertainties, as asked:



- 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?

Response: The model uses fixed boundary conditions. Upstream, the contributions from the main rivers are defined using information from gauge stations. The downstream boundary condition, in contrast, uses tidal data, as the system connects to the ocean. We did not add details on precipitation for the minor tributaries, as their contributions would be negligible compared to the flows from the main rivers. We will, however, add more details about the simulation setup in the Methods section to improve clarity.

- 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 superparameter is calibrated to reduce dimensionality (i.e., regularization)? How sensitive are the results to the selected parameters?

Response: When we refer to the calibration of the model, we are referring to the definition of the manning roughness. We tested different manning values of each river reach, based on what we found in the literature, trying to find the optimum combination of the manning of those main rivers compared to its performance. We believe that including more details related to the Mannings roughness calibration would be benefit for the study, and we would be able to show how sensitive are the results. Overall, the manning values showed medium to high sensitivity to final results.

• 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?

Response: We agree that the presentation of our experimental design can be significantly improved for clarity and that the justification and potential limitations of the scenarios need to be more explicit. We have substantially revised the manuscript to address these points. First, we added an Experiment design overview (new Figure – workflow diagram) in the manuscript, to provide a clear overview of the experiments. Second, we have reorganized the method section as follows:

- Section 3.1 – Model domain, mesh, and parameters: domain extent (river–estuary–lagoon continuum), grid resolution, roughness parameterization, warm-start.
- Section 3.2 – Forcings and boundary conditions: upstream inflow hydrographs, lagoon/ocean levels, data assimilation choices (if any).
- Section 3.3 – Observational datasets and validation metrics: bathymetry sources, ADCP transects, gauge water levels, satellite inundation, SWOT altimetry; RMSE, bias, timing error, inundation overlap.
- Section 3.4 – Experiment design overview (new Figure – workflow diagram): matrix of experiments and outputs.
- 3.4.1 Baseline simulations: configuration and validation period.
- 3.4.2 River-contribution attribution: protocol for selectively scaling/holding inflows to quantify marginal effects on levels/inundation.
- 3.4.3 Peak-synchrony sensitivity (expanded): phase-shift protocol for Taquari/Jacuí hydrographs (regular time increments; volume-conserving shifts), fixed external BCs, pairing of runs, metrics reported.
- 3.4.4 Mitigation scenarios: representation of structural interventions (geometry/roughness or boundary adjustments), performance indicators, and trade-off assessment.

We also added more justification in the Introduction, explicating that "[…] *This study addresses urgent and unresolved questions raised by the May-2024* […] *and yields decision-relevant evidence for stakeholders and government agencies seeking to enhance protection in the most affected areas […]".*

Finally, we incorporated a description of out model's boundary conditions in the Methods section: "*The model's downstream boundary is in the Atlantic Ocean and is forced with dynamic tidal data. This ensures that the representation of the water levels over the basin are realistically subjected to the backwater effects of the tides under variable marine conditions […]*"

• 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.

Response: Thank you for this comment. We agree completely that the practical significance of the water level results is not immediately clear without proper physical reference. Presenting the data showing riverbanks' reference is an excellent idea that will substantially improve the manuscript.

• 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 plotshowing 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?

Response: Thank you for this constructive suggestion. We agree that the original Figure 4 with 12 panels was too dense and that a summary visualization would be much more effective for the reader. We will revise the manuscript Figure based on your recommendations.

• 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?

Response: Thank you for this suggestion. We agree that providing a map comparing the water surface elevation from SWOT and the hydrodynamic model would be beneficial for the manuscript's discussion. We will add this information to the updated manuscript version.

• 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.

Response: Thank you for your feedback. We will update Figure 6 to more clearly distinguish between the simulated flood extent and the extent observed via remote sensing data.

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.

Response: We will add more comparisons between simulated streamflow and observations from gauge stations. We will also incorporate the uncertainty of the gauge data in our comparison with the ADCP measurements.

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).

Response: We thank the reviewer for this suggestion. We appreciate their effort to enhance this figure and will incorporate a revised Figure 8 according to this recommendation.

• 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).

Response: This scenario is highly plausible because the two river basins are geographically distinct enough to experience different rainfall events. A large-scale storm system, or a particular sequence of storms, could cause their flood peaks to synchronize as they propagate downstream. In Southern Brazil, the occurrence of multiple cold fronts within a few days are nor rare. For instance, in the May 2024, two cold fronts of varying spatial extent and intensity passed over the state of Rio Grande do Sul between April 27 and May 2.

Now, we have added the sentence to clarify the reason of the analysis:

“*The analysis of the flood synchronization of the Jacuí and Taquari Rivers in the RMPA was performed considering the context of previous events. In May 2024, two cold fronts of varying spatial extent and intensity passed over the state of Rio Grande do Sul between April 27 and May 2. As a consequence, the synchrony between the peak flows of major tributaries and the estuary-lagoon water level is a primary determinant of flood severity, directly informing the timing and feasibility of structural and operational measures*”

• 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. •

Response: Thank you for raising this point. We included this analysis because it has become an internationally highly relevant topic since the May 2024 flood in southern Brazil. There is an ongoing debate among the public and environmental agencies regarding the efficiency and feasibility of such measures. However, scientific studies on their applicability in our region are lacking. While some reports have pointed to them as a solution (Hunt et al., 2024; Silva et al., 2024) they do not include an in-depth analysis. International consultancy studies suggested, but not tested, these kind of solutions after local assessments (Lamoree et al., 2024), resting unclear for the decision makers and the broad scientific community (specially beyond hydrologists) the relevance of these kinds of measures.

We believe scientific production should be free of confirmation bias and not restricted to reporting only “successful” outcomes. It must also document when widely suggested ideas, whether proposed by specialists or non-specialists, do not achieve the intended effects. Presenting negative or null results is essential to stress-test assumptions, refine hypotheses, and prevent costly missteps in policy and engineering. By transparently evaluating a range of plausible options and clearly communicating both what works and what does not, research better supports decision-makers with evidence-based guidance, a deeper understanding of system behavior, and a more realistic appraisal of uncertainties and trade-offs.

Therefore, our study aims to provide a scientific benchmark for local agencies and governments. We identify the key hydrodynamic processes and evaluate potential solutions for improving the flood protection system.

We added a justification regarding the hydraulic interventions in the Method section:

“*Ongoing debate among public and environmental agencies on how to mitigate flooding in the RMPA has led to various proposed interventions in the estuary-lagoon system. Specifically, these proposals, which have not yet been formally evaluated, suggest constructing new channels to reduce regional water levels. Although these projects are still in the conceptual stage, we used our 2D hydrodynamic model to test their potential effects. This exercise aims to better comprehend the dominant forces controlling the system's dynamics.*”

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.

Response: We totally agree with the reviewer, and we have revised the manuscript to separate the Results (Section 4) and the Discussion (Section 5), allowing for a clearer focus on our research questions.

Also, the discussion section has been significantly expanded with more detailed comparison of our results with the existing literature and investigations regarding the viability and efficiency of structural interventions.

We now proposed the following sections of results:

- Section 4.1 - Model validation
  - Water level
  - Flood extent
  - Streamflow
- Section 4.2 – Hydraulic mechanism of the flood
  - River flood contribution
  - River flood synchronization
- Section 4.3 Hydraulic interventions for flood control
  - Jacuí Guaíba channel
  - Patos Lagoon channel

And for discussion:

- Section 5.1 - Model performance
- Section 5.2 - Uncertainties regarding the two-dimensional model
- Section 5.3 - Recommendations for flooding managements and strategies in the region

• 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.

Response: Thank you for this suggestion. We revised the support points and reshaped (iii) by follows:

"(iii) The proposed hydraulic structures of additional channels alternatives would not have been sufficient to prevent RMPA flooding entirely. Our results also indicated that the degree of flood mitigation structures would not have been uniform across the RMPA. This spatial disparity in performance suggests that the limited overall impact may be linked to a combination of factors, including the specific design of the interventions, local hydrogeomorphic features, and the unprecedented magnitude of the flood event itself'

• The paragraph between L430-434 should be included in the discussion (limitations) rather than in the conclusions. •

Response: Thank you for noting this. We have moved this paragraph to the discussion section and revised the Conclusion to ensure it remains focused on our key findings.

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.

Response: Thank you for your suggestion. The final sentence has been rewritten as follows:

"Finally, this research advances a methodological framework predicated on multi-source data integration for the robust performance assessment of hydrodynamic simulations. By incorporating multiple, independent observational datasets, we significantly enhanced the model's predictive accuracy and its fidelity in reproducing this flood event. We expect that the presented methods will serve as a reference for studies in other locations, as well as for analyses of the efficiency of structural measures for flood control."

Minor comments:

• In Figure 2, consider including the points where the ADCP measurements are available.

Response: Thank you for your suggestion, we incorporated the ADCP measurement's location in Figure 2.

• 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.

Response: We will revise all the figures in the manuscript to improve their readability.

• L42: “In instance” → For instance.

Response: Thanks, we corrected this.

• L205: “Finally, a set of hydraulic interventions experiments was organized” → Finally, hydraulic intervention experiments were tested.

Response: Thank you for noting, we adjusted this in the manuscript.

• L299: “… the peak water would lower xx meters to 4.75 meters,…” → typo + verb is missing

Response: Thank you for noting, we adjusted this.

• L417: “Our findings address the following scientific questions:…”→ Our findings are summarized as follow:

Response: Thank you. We corrected this.

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