This research article presents a numerical framework that schematizes a delta channel network as a series of connected bifurcations. The research builds on a large body of work that considers "stability of bifurcations", or what configuration of water and sediment partitioning at a bifurcation enables the bifurcation to persist (i.e., be in equilibrium) rather than abandon one of the branches. This work extends that framework to multiple bifurcations, wherein the upstream leg of a bifurcation is treated as one of two downstream branches of another bifurcation; this is considered to be analogous to a delta network. Through a system of equations derived for any initial delta configuration, system parts and variables are isolated, and their stability is determined. A key finding, per the article title, is that there are multiple flow-partitioning configurations of given planform delta configuration that are stable. The framework is then initialized with data from the real-world Po River delta system, and the stability of this system is explored. Through this analysis, the authors have identified new insights into the possible controls on avulsion and bifurcations stability, as well as potential futures for the Po River delta system.

Overall, the article is fairly well written, interesting, and will be well received by the readers of Earth Surface Dynamics. The introduction and discussion could benefit from clarification to contextualize the research. The model description and presentation of results are excellent. In particular, I enjoyed reading the Po River delta application, and exploration of possible avulsions in the system (Figure 9). I recommend some minor revisions before publication.

Main comments:

1. The motivation in the Introduction could be made more specific to this research. At present, it is very general about anthropogenic modification and "proper management" but does not specifically talk about channels or avulsions. For example, there is discussion of levees reducing flow onto interdistributary basins (line 27) but it is not clear how this relates to channel stability. The authors mention "navigability and downstream infrastructure" (line 388) in the discussion, which may be more relevant motivations for this study.

Thank you for this insightful comment. We agree about the introduction to be more specific to the focus of our study. We will revise this section to better highlight the relevance of channel stability and possible avulsion sites, explicitly mentioning their impact on navigability and infrastructures.

2. I am not incredibly familiar with the Salter 2018 2020, Barile 2023, Ragno 2022, and Durante 2024 papers and the framework described in each (lines 36–54). It would help the reader understand the advance of this study if it could be clarified how each of these models/stability frameworks differs (or not) from the one presented here. This will, overall, help to contextualize this study in the wider literature.

Thank you for your comment. We will rephrase the paragraph to clearly distinguish which models correspond to the single bifurcation and which pertain to the entire delta, thereby better highlighting how the frameworks in the cited studies differ from or align with the one presented in our work.

3. a. The organization of the discussion could be improved. The Discussion could be improved by reducing the number of paragraphs and grouping logically-related ideas into subsections. Subsections could break apart the analysis of (i) internal bifurcation feedback, (ii) system planimetric effects, (iii) and Po River delta application.

Thank you for this constructive suggestion. We agree that the organization of the Discussion section could be improved for clarity and coherence. We will introduce distinct subsections as suggested to enhance readability and better highlight the key aspects of our analysis.

b. I also suggest the authors revisit the logical organization of their paragraphs in the Discussion section. For example, there are several times that the discussion mentions seaward effects (lines 382, 393), but these ideas span a few (sometimes short) paragraphs.

Thank you for your suggestion. We will review the organization of the Discussion section to ensure that related ideas are presented in a more cohesive manner.

4. The idea of adjustment timescales and equilibria introduced in lines 307–312 is not revisited when discussing the channel abandonment (line 344–348), or soft avulsion (line 354–360), or delta lobe progradation (line 382–390). In my opinion, this discussion of timescales is the most important aspect of applying this numerical framework to the real world in any meaningful way, which seems to be of interest to the authors. I realize the framework is not fully morphodyanmic and does not explicitly include a temporal evolution, but the authors could identify terms in the framework that would be compared against real world processes and rates mentioned above (abandonment, soft avulsion, lobe progradation) to determine the scales at which this framework is useful. To me, this is a major limitation to understanding whether this framework has any predictive power.

Thank you for your insightful comment. We acknowledge the importance of discussing timescales in relation to equilibria and the key phenomena characteristic of river deltas. The reviewer is correct in noting that the current model formulation does not incorporate temporal evolution, instead relying on the concept of equilibrium. However, we can provide a rough estimate of the morphological timescale T_M, which gives an order of magnitude for the temporal scale over which the system evolves. This timescale is defined as:

$$T_M = (1-p)\frac{W_{up}^* D_{up}^*}{q_{s_{up}}^*}$$

where W, D and qs represent the width, depth, and solid discharge at the delta apex, respectively. However, we recognize that real-world deltas may respond differently due to varying external factors (e.g., sea-level rise, subsidence, bank erosion, vegetation), and the temporal scales at which these changes occur. Nonetheless, equilibrium-based models remain valuable for identifying dominant processes and feedback mechanisms within the system, as discussed in Zhou et al. (2017). We will revise the manuscript to better highlight these points and clarify the applicability of the model in real-world scenarios.

Zhou, Z., Coco, G., Townend, I., Olabarrieta, M., Van Der Wegen, M., Gong, Z., ... & Zhang, C. (2017). Is “morphodynamic equilibrium” an oxymoron?. *Earth-Science Reviews*, *165*, 257-267.

Minor comments/corrections:

- The meaning of “multiple equilibrium states” on line 4 of the abstract is not clear at this point. I suggest revising the abstract to be more specific about the “unique challenges” facing deltas (see Main Comment 1) and more specific about the numerical approach before stating what exactly the study identifies.

  We agree. We will revise the abstract.

- The actual description of the numerical framework, including relevant terms and their relationships, is excellent. Thank you.

  Thank you.

- The analysis beginning on line 168 assumes a symmetrical planform delta (Lb1=Lc1), correct? This was not clear to me at first: even though it does say symmetrical on line 171/172, the sketch in Figure 3 is not depicting a symmetrical delta, but this sketch is referenced on line 172. Moreover, what does Figure 3 depict that is not already covered in Figure 2? I found this to be a sort of confusing point, because I couldn’t understand how Lb1=Lb2 in Figure 3 when they are clearly different, until I reread a few times and realized the sketch did not match the description. I suggest the authors consider revising Figures 2/3/4c/4d to show the necessary components and only one time, for both a case of symmetric delta asymmetric delta. This will also help clarify how a delta can be planform symmetrical but have asymmetrical discharge partitioning.

  The text introducing Figure 3 has been revised to clarify the original source of confusion.

- Suggest to indicate the meaning of the dashed lines in Figures 6 and 7 in either the figure itself or the figure caption.

  Thank you for the suggestion. We will add a sentence to the figure caption to explicitly clarify the meaning of the dashed lines in Figures 6 and 7.

- “the concept of long-term morphodynamic equilibrium in river deltas may be inherently transient” was a confusing statement to me. I don’t think the authors mean the concept is transient. Suggest revising to be more specific.

  Thank you, we will rewrite the sentence.