Assessment: The manuscript is well-presented, relatively easy to read and follow, and is free of grammatical errors. Overall, I found the paper interesting and the results showing the variable effects of downstream length, in the channels immediately downstream or downstream of the second bifurcations, follows well-understood phenomena based on models from these authors and others. The comparison to the Po River delta is a welcomed piece, and the model does provide some interesting insights into the dynamics of that system. I have some criticisms of the contextualization of the results, especially with the assumption that flow is contained completely within channels. I am not suggesting that the authors redo anything, but it is my opinion that this assumption warrants some consideration and discussion. The abstract also needs to be edited to provide more substance and detail. At the moment it is very short and general to the point it is difficult to tell what novel insights this paper provides, and there are plenty of interesting results the authors could include to improve it.

Major points:

Abstract: The abstract lacks substantive information on the results of the paper. It I clear that equilibrium configurations are analyzed, but it's not clear what novel information or insights this manuscript provides. These authors have published several papers on equilibrium configurations in bifurcations, deltas, etc. and it would be beneficial to add specificity to the abstract.

Thank you for your valuable feedback. We agree that the abstract could be more effective in highlighting the key results of our study. We will revise it to provide greater specificity and clarity regarding the novel insights and contributions of our work.

It appears that there is no overbank flow allowed in such a model. If my understanding of the way these models work is correct, there cannot be an internal outlet. Is perfect flow conservation within the channel deltaic network to the seaward boundary realistic? For eg., see Allison et al (2023; 2012), Feizabadi et al., 2024; Gao et al., 2023; Hiatt & Passalacqua (2015;2017); Hiatt et al., (2018); Shaw et al., 2016. Even if it is not realistic, how does this assumption affect the results? Several studies suggest that connectivity with the floodplain is a primary driver of morphology in river deltas (e.g., Coffey and Shaw, 2017; Olliver and Edmonds 2021), and it stands to reason that this should affect bed morphology, etc. I recognize many of these examples are from Wax Lake Delta, but nevertheless this is often considered a prototype river dominated delta. I think this should be explained in the context of the assumption/limitations of the model design and in the discussion section to understand how containing flows to the channel network influences predictions of morphology/stability. This point may be important when considering the results shown in Figure 5. Water level asymmetry can drive lateral flow (Gao et al., (2023)) and may have implications for the results showing disagreement with the Po system, especially in the more downstream reaches (Tolle), where I assume the assumption of conserved channelized flow fails (I am not sure of this, of course, but there certainly looks to be connections in Figure 1b).

We sincerely appreciate the reviewer's insightful comments. The reviewer is absolutely correct in noting that the current model formulation does not include overbank flow. This was adopted as a first simplifying assumption to isolate the fundamental mechanisms governing the equilibrium of river deltas. Beyond the significant ecological implications, lateral flow dispersal primarily reduces flow velocity within channels, potentially enhancing local sediment deposition. However, as the reviewer rightly points out, this assumption may be overly restrictive when comparing the model to real-world deltas.

Nevertheless, the principle of flow conservation remains largely valid for many river deltas worldwide, such as the Po River Delta, where extensive human interventions, including widespread levee construction, have been implemented to confine the flow within designated channels. As noted by the reviewer, the degree of confinement decreases in the seaward reaches of the Tolle branch and the main channel, which likely contributes to the observed discrepancies in flow partitioning and bed elevation estimations in these areas.

Additionally, the model does not account for two secondary branches at the downstream end of the main channel. Field studies (Zasso & Settin, 2012) indicate that these branches (Busa di Tramontana and Busa di Scirocco) divert approximately 25% of the local flow discharge. This, combined with flow dispersal towards the lagoons at the delta fringes, introduces a degree of error in the local free-surface slopes and may influence the computed flow partitioning at the lowermost bifurcation considered.

Future studies could address these limitations by incorporating both localized and distributed flow losses into the model. However, implementing such modifications requires careful consideration when evaluating the long-term equilibrium of the system. A preliminary estimate of these contributions could be guided by site-specific field studies, though direct incorporation of such measurements assumes long-term constancy, which is highly uncertain, particularly in young and actively prograding deltas.

We will incorporate these considerations and the relevant contributions on perfect flow conservation within the channel and connectivity with the floodplain mentioned by the Reviewer into the discussion section of the manuscript, as we believe they provide an important context for interpreting the model's results. Additionally, we welcome further feedback on this matter and hope it fosters future collaborations.

Minor points and edits:

Line 10-11: Is the quantity and quality of sediment relative to the accommodation space really the key driver? In other words, even if the quantity of sediment is very small, if the space that needs to be filled is very small then a delta will be formed. The opposite is also true for large sediment loads with lots of space to fill. I suppose the point is moot because the authors mention this is in the next sentence, essentially.

Thank you for your comment. We will revise this introductory sentence accordingly to clarify the relationship between sediment supply and accommodation space.

Figure 1b: If the bathymetry of the channels is shown, we likely need a colorbar to distinguish elevations, otherwise it should be removed to match the birdsfoot. Also, I know the Po is the focus of the manuscript, but there should probably also be an inset map for the Birdsfoot. The inset map in Figure 1b is also not supremely helpful to those that are not familiar with northern Italy, so I'd recommend showing the full country and political boundaries.

Thank you for pointing this out. We will revise the figures accordingly, including improvements to the inset maps for both deltas. The colors in panel 1b were solely intended to highlight the course of the branches, as they would otherwise be difficult to distinguish. In the revised version of the paper, we will only highlight the river axis of each branches with a single colour line in order to highlight the delta network that otherwise would be difficult to be captured.

Lines 21-22: "…drains a significant amount of water and carries a substantial quantity of sediment…" I'd recommend just reporting those annual figures here instead of using qualifying adjectives. Just give the quantities.

Thank you for the suggestion. We will add the appropriate values in the manuscript. Specifically, at the Po Delta Apex the mean annual discharge is approximately 1500 m^3/s and the corresponding total sediment load is in the range $9\text{–}12 \cdot 10^6$ tons/year (Lanzoni et al., 2015; Milliman & Farnsworth, 2013; Nienhuis et al., 2020).

Lines 30-32: This statement should be modified or removed: There are many studies focused on deltas that consider things other than flow routes and bio-ecology (not even sure what bio-ecology is). I would recommend a rewrite of this whole paragraph, giving proper consideration to the literature. Deltaic science is multidisciplinary are there are myriad studies across disciplines, so saying that most studies related to deltas "…focus merely on hydrodynamics…" is incorrect. The second argument also may not be correct – there are quite a number of detailed morphological modeling studies in the Mississippi River Delta, for example (e.g., Meselhe et al., (2021) cited below).

We acknowledge that our statement was overly broad and did not fully capture the extensive multidisciplinary research on deltaic systems. Our intention was to highlight the challenges in assessing the long-term equilibrium of river deltas. We will revise this paragraph to more accurately reflect the existing literature and provide a more comprehensive discussion of relevant studies.

Lines 36-42: While I am familiar with these models and agree that they are useful , I recommend being more objective and removing words such as "easier" and "powerful insight."

Thank you for your feedback. We will revise these sentences to ensure a more objective tone.

Line 57-58: There is some work on the bed morphology in Wax Lake Delta from Ehab Meslhe's group using a Delt3D Morpho model (Meselhe et al., 2021).

In this context, we are specifically referring to morphodynamic models applied to the Po River Delta. However, we acknowledge the relevance of the work by Meselhe et al. (2021) and will consider referencing it where appropriate.

Line 182: What is the critical threshold for R_Up? Can the authors please present an example for on of the L'tot values so the reader can more easily contextualize the results in Figure 4?

Here the R_cr is considered as the point where the single solution at DQ=0 splits in the 3 separate solutions. In other words when each single line in Figure 4 bifurcates moving leftward. To improve clarity, we will add a filled dot, matching the color of the corresponding lines, in Figures 4 and 5 to indicate this threshold.
Regarding L'tot, it is defined in eq. (14) of the manuscript. It basically represents the distance of the bifurcation from the sea, aggregating the length of the downstream branches.

To get a first estimate of the value of R_cr, one can use the formulation for the single bifurcation as defined in Durante et al. (2024). Manipulating equation 28 of such paper we retrieve:

$$R_{cr} = \frac{3}{4} \frac{\left({}^{7}\!/_{3}\, L \; - \; {}^{3}\!/_{2}\, Fr^{2} \right)}{(6L + 3Fr^{2} + 4\, L\, Fr^{2})}$$

Where, in order to compare with L'tot, L need to be formulated as:

$$L = \frac{L'_{tot}\, W^{*}_{up}\, s_{up}}{D^{*}_{up}}$$

with W, D and s are the width, depth and slope of the upstream channel.

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