Main concerns & questions

1. River deltas versus water-terminating fluvial fans. The distinction between river deltas and fluvial fans is key to the premise of this paper, which asserts that these are two fundamentally different landforms with different bifurcation processes and thus different morphologies. Overall this makes sense to me, except in the case that fluvial fans terminate in standing bodies of water (i.e., oceans and lakes). If the distributary fluvial network ends in a standing body of water, isn't that simply a river delta? Lines 74-84 mention this ambiguity, but I don't understand how this is resolved in the paper. At a minimum, more text is needed to explain how some landforms that end in water are classified as fluvial fans, and others are classified as river deltas. Figure 7d shows the different terminations for the fluvial fans in this study (although not the n for each one), and many of them are in the "lake" and "marine" categories. How are those distinct from river deltas? Addressing this question may be a matter of revision to the writing to better explain in advance how water-terminating fans differ from river deltas (i.e., spell this out clearly in line 74-84, using criteria that aren't the ones being tested in this study (bifurcation angle, etc.)). However, if such independent criteria don't exist, then I wonder if it is necessary to remove the lake- and marine-terminating fans from this analysis. Having two independent populations is very important in this comparative analysis, so I consider this to be a critical issue that needs to be addressed before publication.

We understand the confusion regarding fluvial fans that terminate into standing bodies of water. In fact, this confusion is a key motivation for this work, as deltas and fluvial fans are likely to respond differently to climate change and urbanization.

As discussed in the Introduction and Sections 2.1 and 2.2, river deltas and fluvial fans form via different sedimentological processes, which generate distinct channel network morphometrics. We modified the text in the introduction in attempt to further highlight these differences.

Several landforms included in this study (e.g., the Saskatchewan and Niger fluvial fans and deltas) have both a fluvial fan and a downstream river delta, and our analysis shows that these features cluster distinctly into fan and delta categories. Regarding Figure 7d, we now clarify in Section 4.1 that all fan termination styles are represented by 4–6 landforms each, providing clearer context for water-terminating fans within the dataset. Also, we test for differences in fluvial fan channel network angles by termination style, and there are no statistical differences in lake and ocean terminating fans vs other termination styles.

2. River vs wave vs tide delta criteria. The matter of defining the type of delta (river vs wave vs tide) is unclear in this manuscript and needs clarification or to be refined with more quantitative criteria, if no quantitative metrics were applied before sites were chosen. In the intro (lines 187-188), the authors specify that only river-dominated deltas were used, but it is unclear how this is established and also there

are results from wave- and tide-dominated deltas later on (e.g., Figure 9). The current methods sentence (lines 298-299) is definitely not specific enough about distinguishing between types. I do think the cited literature in this sentence is the appropriate body of work to establish specific criteria for defining fluvial fans, but because criteria can vary, this paper needs to specifically define it here. On the same note, how are the wave- and tide-influenced river-dominated deltas actually distinguished (line 366, for example)? Please clarify these criteria as well in the methods.

We acknowledge the need for clearer definitions for delta classification and have revised the text accordingly. In Section 3.4, we now explicitly define our criteria for distinguishing wave- and tide-dominated versus wave- and tide-influenced river deltas. Wave-dominated deltas (e.g., São Francisco, Eel) are characterized by strandplains and a complete absence of bifurcations, whereas wave-influenced deltas retain strandplains but exhibit clear, measurable channel bifurcations. Tide-dominated deltas (e.g., Fly, Yangtze) feature a limited number of channels that widen substantially seaward, while tide-influenced deltas show widening only in their most distal channels. The Supplementary Data which we now reference here includes data on our classification of delta types.

There are no results from wave- or tide-dominated deltas included in this manuscript. We only used wave- and tide-influenced (river-dominated) deltas, including the display in Figure 7 (there is no Figure 9).

3. Bifurcation vs avulsion terminology. This is only a matter of wording and so is less important than my previous two comments, but I think the way the authors have defined “bifurcation” as a process of channel splitting driven by mouth bar formation (i.e., line 139) is too narrow and leads to some confusion throughout the paper. Many geomorphologists/sedimentologists, myself included, think of bifurcation as a channel split which can occur via many mechanisms, including avulsion. I think of the great Slingerland and Smith (2004) paper about avulsions – there is a wonderful section in that paper that thinks about how avulsions occur via a bifurcation stability analysis, as just one example of a key reference where avulsions are treated as bifurcations. In this manuscript, the authors clearly lay out their narrower, mouth-bar focused definition of bifurcation in lines 138-143, so I do understand what they mean. However, is this likely confusion for some readers necessary? Why not call the “bifurcation” group mouth bar bifurcations? That term has process in it, which makes it more equivalent to the “avulsion” category, which is also a process. The word bifurcation is too geometric and isn’t tied to a specific process by broad definition. Changing this terminology would require editing uses of the narrowly defined “bifurcation” throughout the paper & figures, but I think it would be worth it to improve clarity.

We appreciate your comments regarding our terminology for bifurcation versus avulsion and agree that these terms are differently in the community. Consequently, whichever way we proceed, some community members will be confused. We chose to define bifurcation more narrowly as channel splitting driven by mouth-bar deposition, as this usage is common in the deltaic literature, and helps us to be clear about the differences in formative processes in deltas and fluvial fans. We acknowledge that this narrower definition may not capture the full spectrum of processes encompassed by bifurcation in the broader geomorphic sense, but we felt it necessary to distinguish between mouth bar–driven bifurcations and avulsion-driven channel creation to reduce ambiguity in how these processes are expressed in fan-shaped landforms. The lack of standardized terminology in the literature has indeed contributed to confusion regarding the processes that govern channel network evolution on deltas and fluvial fans. By explicitly defining bifurcation in our study, we aim to provide clarity for our classification framework. We modified the text in the introduction to more explicitly state the narrower process-based use of "bifurcation".

Line-specific comments

4. Line 22-24: the abstract should have more of the actual results in it, including the different bifurcation angles found for deltas vs fans.

We have added the mean bifurcation angle values for deltas and fluvial fans to the abstract, as these are a key diagnostic criterion in the study.

5. Line 141: needs older citations defining channel avulsion.

We have added additional older references that define channel avulsion to strengthen the background for our manuscript.

6. Lines 156-165: this paragraph should cite Brooke et al. 2022 and engage with the findings therein about where avulsions occur on deltas vs fluvial fans. In fact I think the ideas from this paper would be useful in other parts of this manuscript as well (such as in section 2.2).

We have further integrated the findings of Brooke et al., (2022) in sections 2.1 and 2.2 to better discuss and differentiate avulsions on deltas and fluvial fans.

7. Line 198-207: Cite & consider local vs regional avulsion ideas in Slingerland and Smith (2004), which also has important discussion about avulsion bifurcations that could be useful throughout this manuscript.

We have added discussion of regional avulsions in Section 2.2, citing Slingerland and Smith (2004). Additionally, we included a note on local avulsions in Section 3.1 (Methods), clarifying that these typically rejoin the downstream channel, and are therefore not included in our channel network measurements.

8. Lines 261-264: How do you distinguish between active and abandoned channels? Is that distinction important for this? What about splay channels versus main

channels? Does it matter if the avulsion bifurcation is partial? This question also came up for me in lines 325-326, that clarity is needed on how splay channels are considered (in both fan and delta environments – since deltas also have splays and they don't form via the mouth bar bifurcation process).

We recognize the ambiguity in distinguishing active versus abandoned channels. Section 3.1 now clarifies how paleochannels are identified and why they are included. For partial avulsions, both older and newer channels may convey flow simultaneously, but this does not affect our methodology as long as channels do not merge downstream. Splay channels are included in some measurements, with their occurrence discussed in Section 5.1. We also emphasize that a sufficiently large sample size is essential to capture representative bifurcation angles and reduce the influence of splays or other local anomalies on mean values for a given fluvial fan or delta.

9. Line 308: typo/wording

We have rewritten the text here to provide more clarity in the terminologies and address the type.

10. Line 326: None of the systems have seasonal change in discharge? That doesn't seem possible.... Probably just a wording issue for this statement.

The statement was intended to convey that, across the datasets we examined, we do not observe significant changes in channel width or discharge across individual fans that could be attributed to differences in the timing of image capture relative to precipitation events. Fluvial fans are highly sensitive to precipitation and short-term variability is possible. Because fluvial fans can extend for hundreds of kilometers, satellite images are often mosaicked from multiple acquisitions taken at different times. As a result, we do not see systematic differences in channel activity across the fan (e.g., lower fan channels do not consistently appear wider in one area of the fan due to recent precipitation).

11. Line 336: typo/wording

We have omitted the typo in this sentence.

12. Figure 7d: labels beneath the violin plots are overlapping and hard to read

We increased the x-axis label sizes on the violin plots in Figure 7 to improve readability, with specific adjustments on Figure 7d for clarity.

13. Line 388-389: why is there a discrepancy with Hartley et al. (2010)? Explain/justify

We have addressed the discrepancies with Hartley et al. (2010) by providing examples in section 4.1 where their definitions do not align with the observed environments, such as referring to playa fans as lacustrine or ocean fans as contributory.

14. Figure 8: because the key goal of the paper is to compare deltas vs fluvial fans, I do not think these data are presented in the optimal way to make that comparison. It would be easier to compare if the y-axes on width plots were the same for deltas and fans, and same for the length plot y-axes. Additionally, move the width plots in adjacent rows so that it is easy to compare between deltas and fans, and then have the bottom two rows be the length plots for easy comparison.

We appreciate the suggestion to standardize the y-axis scales for easier comparison between deltas and fluvial fans. We have applied consistent y-axis scales for the width plots. For normalized channel length, standardizing the scale caused boxes beyond channel order 5 in fans to become unreadable, so we retained the original scales but have clearly highlighted the differences on the figure axes in the legend. We also reorganized the plot to place widths and lengths on the same rows to improve comparison between the plots.

15. Lines 470-476: does this slope-related assumption pan out with the data? This would be a good hypothesis to measure and test, because showing the mechanism seems pretty key & important. If the mean down-fan slopes are known for selected fans, it wouldn't be too cumbersome to plot slope vs bifurcation angle

We do not observe any trends in relation to average angle with respect to fan gradient (Supplementary Figure 2). However, as noted in our results section, Fig. 6b shows that mean angle increases with channel order in fans, which can be considered an indirect measurement of how dowfan a channel segment is. One potential cause of the discrepancy is that gradients over fans can change from steeper in proximal to significantly lower in more distal regions of fans (e.g. Chakraborty et al., 2010), and that a mean angle does not reflect to changing gradient of the fans. We have now included text in discussion 5.2 to recommend future research directions involving

16. Line 503: for this to be used to distinguish fans vs deltas in seismic datasets, what is the minimum number of measurements you would need to be able to make a conclusion? The means are somewhat different, but there is quite a bit of overlapping range in angle measurements. Can you write more about the required dataset size?

In their study of branching angles in the seismic record, Mahon et al., (2024) use as little as one or two observed measurements for their interpretations. As stated in our discussion 5.1, low sample sizes can lead to varied mean angles which can lead to inconclusive result. We recommend a robust sample size. We now highlight that a greater amount of measurements (approximately equal to or greater than 10) is necessary help to more accurately constrain the mean branching angle.

17. Lines 517-520: cite & incorporate findings from Brooke et al. 2022

We have now incorporated and cited the findings in the discussion section 5.4.

18. Lines 522-523: sediment delivery would not be affected? I'm not sure what you mean by that or how that is related to findings from this paper

We have clarified our point to specify that sedimentation would not be affected by sea level rise across most of a fluvial fans surface except in areas near fan toe.