Peer review of "Discriminating fluvial fans and deltas: Channel network morphometrics reflect distinct formative processes"

This manuscript submitted to *Earth Surface Dynamics* is an interesting paper that uses satellite imagery to quantify the difference in bifurcation angle and channel dimensions between river deltas and fluvial fans. The authors find that bifurcation angle is lower in fluvial fans (n=40 fans) than river deltas (n = 40 deltas), which suggests that the differences in bifurcation process between those two landforms has a morphometric impact. Overall, this paper is clearly written with clear methodology and well-supported conclusions. However, I have some questions about study site selection that need to be clarified in the text (or perhaps adjusted with some supplemental study sites) before publication. Below I outline overall concerns/questions and then line-specific comments.

<u>Main concerns & questions</u>

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.

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.

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.

<u>Line-specific comments</u>

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

Line 141: needs older citations defining channel avulsion

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).

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

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)

Line 308: typo/wording

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

Line 336: typo/wording

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

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

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.

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

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?

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

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