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Interactive comment on "How does the downstream boundary affect avulsion dynamics in a laboratory bifurcation?" *by* Gerard Salter et al.

Anonymous Referee #1

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General comments:
Salter et al., present results from experiments that build on their previous numerical modeling work to examine discharge partitioning through a single bifurcation. Unlike previous models by other researchers, the authors allow net deposition in the system. In the experiments, the authors observe frequent switching of water and sediment discharge between the two branches, with asymmetry increasing for increasing width-to-depth ratios and decreasing dimensionless sediment fluxes. The authors argue that discharge partitioning is strongly affected by progradation vs. bypass, as well as by bar dynamics that generate high frequency oscillations in discharge partitioning. Asymmetry is higher during bypass, as progradation is hypothesized to autocorrect strong asymmetry. In the absence of bars (sand pile experiments), the

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authors find increased asymmetry and a 'frozen' stable asymmetry during bypass, as bars are not present to reroute sediment and reactivate an abandoned channel.

Overall, I find the paper very interesting and of great importance to the scientific community. The authors present a thorough explanation of their experimental methods and analysis of their results. I also find their explanations of avulsion and bar-induced switching compelling, and I appreciate their discussion of the applicability of their experiments. Below I highlight several concerns largely around the presentation of figures, results, and discussion that will strengthen the paper and clarify several key points.

Specific major comments:

Due to a general shortage of referencing, it is often difficult to tell which statements are the authors' own opinions and which are based on previous research/literature. For example, Page 1, Lines 23-24: Which studies? List some examples. Page 2, Lines 8-10: This is clearly based on other studies. Which ones? Include references here. The authors should also more directly relate their results to those of Bertoldi and Tubino (2007) and Bertoldi et al. (2009) (and/or others) throughout the results and discussion. This will put their results better into the broader context.

I find the language regarding the effects of 'deposition' and the 'downstream boundary' on avulsion dynamics confusing. The authors are examining how the partitioning of water and sediment down two branches of a bifurcation is affected by progradation vs. bypass. Yes, of course progradation is deposition, but 'progradation' is also more specific and informative to the reader. It also then makes it clear that the authors are not changing anything about the downstream boundary, but instead the boundary condition is changing as the system progrades. I suggest changing 'deposition' to 'progradation.' This is especially important in cases like Page 3 Line 28: 'The purpose of our experiments was to test the effect of deposition on bifurcation dynamics.' This is misleading on several fronts. First, deposition is a part of bifurcation

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dynamics, so it is hard to understand how you test the effect of it. Second, it sounds like you are examining bifurcation development and evolution, when in fact you are examining discharge partitioning in a bifurcation. This needs to be explicitly stated and clarified. Finally, this distinction will really help in your discussion (e.g., Page 19, lines 17-19). You state that deposition is not necessary for avulsion, as bar migration also allows for channel reactivation. But bar migration necessarily involves 'deposition,' so this statement is not as impactful as it could be. Progradation is not necessary for avulsion, as bar migration can also induce oscillations and channel reactivation.

There is a lot of useful information in the supplemental material, but there is not one reference to it in the manuscript. The authors should reference the supplemental material where appropriate to guide readers to additional data and figures. This is especially helpful for the argument regarding estimations of Shields stresses. I understand that the experimental movies are available through the NCED data repository, but it would also be helpful to have them as supplemental information with the manuscript to make them easier to access.

Figures 4 and 5: I suggest adding some more labeling to these figures to make it easier on the reader. Authors should add text above each plot stating the channel aspect ratio and dimensionless sediment flux. I also strongly recommend calculating and plotting the mean asymmetry for each period (i.e., progradation, transition, bypass) for each experiment and adding that text to the plot. The authors describe differences in asymmetry in each period and between experiments, but quantifying it here would really drive that point home and make interpretation much easier.

Page 8, Line 16: What is the justification for saying exp 2 bypass portion does not have periods of higher asymmetry? From Fig 5, it looks like exp 1 bypass has periods of 0.55 in RL vs. 0.45 in RR. There are comparable numbers in exp 2 bypass, and occasionally an even stronger asymmetry. This needs to be clarified. Quantifying

it on the plot will also make this easier.

Page 12 first paragraph: Call readers attention to the relevant parts of Figure 6. It is hard to follow without knowing which image to look at when. Looking at 6a, it appears the deepened branch is the right one, but the authors refer to a deepening on the river left. So is the left deepening in b? This needs to be clarified.

Figure 8: Also needs labeling. Add estimated timescales from your analysis as vertical lines or asterisks or some symbol. Otherwise the reader has to find your numbers in the text and then go back to the plot and figure out where those go. Your statements regarding controls on avulsion timescales will be easier to digest if the figure is clear.

Page 15, Lines 30-33: Where do alpha and r come from? What are they? Why are they important? You introduce variables that are not defined, are not in an equation in the paper, and appear to never be used again. If this is central to your argument, then you need to elaborate. Since the timescale is not sensitive to them, why are they here? I recommend removing this and, if necessary, adding a supplemental section that elaborates on the model and any relevant sensitivity analysis.

What are the previously unrecognized long-timescale bar dynamics mentioned in the abstract? Your bar timescales described in the paper are all very short. You restate something similar on Page 18, Lines 12-14: Are you arguing bars induced the longer-period switching during bypass? Where is the explanation of this in the data? Your bar-related switching timescales were all < 3 minutes. If you are not arguing this, then you need to reword.

Specific minor comments:

Page 5 Lines 6-8: I don't follow this explanation of your image processing. Why were you only looking in the 0.2-0.3 m range downstream? Are you averaging the cross-stream direction within each branch? So should this be 0.02 – 0.03? This needs to be clarified in the text.

Figure 6: Also recommend labeling. Flow direction, location of bar/scour, quantified asymmetry value.

Page 16, Line 13: Why 3W? Include a reference to justify this choice. Why is this timescale so different from linear bar theory?

Page 18, Lines 3-4: So did you test all of these? More information on these experiments is necessary.

Why was the angle of 16 degrees chosen if the authors acknowledge that this is very low compared to nature?

Technical corrections:
Page 2 last paragraph: 'channel aspect ratio' presumably is width:depth, but please specify for the reader. This can be easily fixed by adding '(i.e., aspect ratio)' after 'width-to-depth ratio' earlier on this page.

Page 3, Line 26: Change 'We did our best to level' to 'We leveled'

Page 5, Line 16: delete comma after 'overhead images'

Page 7, Line 11: replace 'that paper' with actual reference (Salter et al., 2018)

Page 8, Line 8: 'water discharge increases **(i.e., W/h decreases)**, asymmetry decreases.'

Page 8, Line 15: Recommend adding 'where the river left branch is favored, **but the asymmetry is still small (X)**.'

Figure 3: It needs to be clarified if this is water or sediment discharge.

Page 11, Line 1-2: Does this matter? Recommend deleting.

Page 11, Lines 11-12: 'The asymmetry magnitude during these periods. . .' Which periods? There is a 'these periods' phrase also in the preceding sentence. Please clarify.

Page 11, Line 24: Fix parentheses 'experiments of Bertoldi et al., **(**2009).' This error also occurs elsewhere in the paper.

Figure 7: 'Histograms of the **water** discharge asymmetry' Also the asymmetry needs to be defined. It is on the horizontal axis but is never defined in the paper. RL and RR I assume are river left and river right, but this is similarly never defined anywhere.

Page 14, Line 4: This is confusing. Please reword. I assume this is meant to say that right branch-focused strong asymmetry is diminished under bypass relative to depositional phase.

Page 15, Line 25: Which model? SVP 18? Specify.

Page 16, Line 21-22: Reference needed.

Page 18, Line 10: 'deposition acts **as** a stopgap:'

Page 20, Line 15: Add Salter et al., 2018 reference for ‘most relevant existing model.’

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