Responses to Review Comments

In the following, review comments are in ***blue italic*** font, while responses are in **black normal** font.

Reviewer #2 (Anonymous Referee)

(1) The primarily reports the results of physical experiments on the effects of channel confinement and sediment input ratio on the characteristics of braided channel patterns formed by submarine turbidity flows. In addition, results are supported by the output from a cellular model of turbidity flows based on caesar-lisflood to model the cross-channel distributions of depth, discharge and sediment flux. The results are compared with fluvial braided systems and shown to behave in similar ways with respect to confinement effects on braiding intensity, active width and volumetric erosion-deposition patterns. This has larger significance in relation to the physics of these flows and the potential to transfer understanding and relationships between these two environments and process-regimes. This also adds to results of Lai et al. 2017 and Limaye et al. 2018 and the paper demonstrates implications for full scale submarine braided turbidity flow channels and related sedimentology.

Reply: We thank the reviewer's positive feedback and constructive comments. Our responses to each question are listed below.

(2) Major suggestions:

(2.1) The role of B (confinement width) and Qin/Qs (sediment concentration or discharge/sediment load ratio) are emphasised throughout in relation to both braiding intensity and bulk volumetric change and active width. The experiments are designed to show especially the effect of confinement width on braiding intensity of turbidity flows. The design of the experiments also means that tests differ in total discharge and, consequently, discharge per unit width (Qin/B).

Reply: Before answering the questions, allowing us to clarify the definition of inflow width (b), confinement width (B) and valley width (W) (see the updated Fig. 2). We also add these widths and the correct inflow unit width discharge (q_{in}) in the updated Table 1.

First, while we understand the logic of dividing the inflow total discharge (Q_{in}) by the confinement width (B) to estimate the inflow unit width discharge (q_{in}), we think that approach would be inconsistent with our observation that in the experiments, the submarine braided channels do not occupy the entire confinement width. Therefore, we instead calibrate the inflow unit width discharge by $q_{in} = Q_{in}/b$, where b is inflow width (not confinement width B) (see the updated Fig. 2 and Table 1).

Second, we control the inflow-to-sediment discharge ratio (Q_{in}/Q_s) and inflow unit width discharge (q_{in}) at the same time. It may not be possible to produce submarine braided channels only by providing correct discharge per unit width without setting the correct inflow-to-sediment ratio. Based on previous reported successful runs of Foreman et al. (2015) and Lai et al. (2017), Q_{in}/Q_s = 60 and 90 are the two reasonable values of this ratio.

Third, we set confinement width (B) as one of the main variables in this study for possible exploration in the future (which would not be limited to an initially straight channel): (1) If B is gradually changed (widened, narrowed or irregular), how B may influence the morphology of submarine braided channels? (2) When the left and right sides of the channel are confined bedrock, i.e., confinement width equals to valley width ($B = W$) or partly bedrock confined, how will this situation affect the development of submarine braided channels? These parts are beyond the scope of this study but we plan to address them in another paper.

(2.2) Based on Table 1, Series A experiments have unit discharge of 40-50, while Series B experiments are in the range 60-85). If the width of the flows is also directly dependent on discharge and discharge per unit width, then there is, in addition to confinement width alone, a potential effect of discharge in the results. An example of this is the comparison of A2 and B1 which have the same discharge but A2 has two-times the confinement width of B1.

Reply: The inflow unit width discharge (q_{in}) of Series A (Runs A1, A2 and A3) is controlled around $q_{in} \cong$ 55 mm^2/s; while the inflow unit width discharge of Series B (Runs B1, B2 and B3) is controlled around $q_{in} \cong$ 84 mm^2/s (about 1.5 times larger) (see Table 1). Although Run A2 and Run B1 have the same inflow total discharge (Q_{in}), the q_{in} value for Run B1 is 1.5 times larger than for Run A2, while b for Run B1 is 1.5 times smaller than for Run A2 (i.e., they do not differ by a factor of 2).

(2.3) The interpretation ascribes the difference in BI to difference in confinement width but is it also potentially the result of the difference in discharge. The experiments assume fixed confinement width (although there is some erosion in some tests, as pointed out in the paper) and presumably differing total discharge and unit discharge are relevant to the outcome in channel network configuration as well as volumetric changes and active width.

Reply: The experimental conditions listed in Table 1 should be read as: In Series A (q_{in} = 55 mm^2/s, Q_{in}/Q_s = 60), we want to compare the effect of confinement width (B) among Runs A1, A2 and A3; In Series B (q_{in} = 84 mm^2/s, Q_{in}/Q_s = 90), we want to compare the effect of confinement width (B) among Runs B1, B2 and B3. When comparing across Series A and Series B, we want to discuss the effect of q_{in} or Q_{in}/Q_s under the same confinement width (B).

(2.4) A larger discharge in the same width, is, in effect, a relative confinement if the channel is not allowed to widen to accommodate the increased flow. If this idea is correct, some further analysis might be useful to look at this discharge effect, or to interpret the existing results in this way. It seems potentially the case that the differences between the A and B series are at least partially related to this effect but the text emphasises B and Qin/Qs as the explanation for differences.

Reply: In our experiment, the channel is allowed to widen laterally (not limited by bedrock). The reason is the same as our reply in question (2.1). For future studies, we plan to explore the effects when the confinement width equals to valley width ($B = W$) or the channel configuration is partly bedrock confined with irregular shapes. In the current experiment, the two sides of confinement width are straight, erodible floodplains or terraces, not bedrock.

In Series A ($q_{in} \cong$ 55 mm^2/s, $Q_{in}/Q_s \cong$ 60), most of the submarine braided channels develop within the given confinement width without much widening to accommodate the inflow. However, in Series B ($q_{in} \cong$ 84 mm^2/s, $Q_{in}/Q_s \cong$ 90), the submarine braided channels would widen the given confinement width to accommodate the larger inflow and cause stronger bank erosion. We add this point to the Result section (see Line 239 to 242).

(2.5) A similar argument might be made for the amplitude of topographic changes between runs (Fig 9) which are also ascribed to B but could also be a Qin effect. [As an aside, 'eyeballing' the DOD maps (Fig 8) seems to show that B2 has largest amplitude of change but this is not apparent in Fig 9].

Reply: The DoD map (Fig. 8) presents the averaged sediment erosion and deposition pattern for t = 4800 s to 6000 s of each experiment. Fig. 9 presents the homogeneous and heterogeneity (i.e., uniform or non-uniform) of DoD in terms of statistical distribution. We found that, under the same unit width discharge, the effect of B on the heterogeneity of DoD is more significant, i.e., a smaller B would result in more homogeneous DoD (e.g., Run A1 and B1); a larger B would result in more heterogeneous DoD (e.g., Run A3 and Run B3). Although eyeballing the amplitude of topographic changes of Run B2 is the most significant, it is possible that a larger main channel appeared just in the last 1200 s, resulting in more drastic topographic changes. However, the statistical distribution shows that the DoD of Run B2 is relatively homogeneous.

(3) The effect of confinement width on BIA is a major message of the paper but this relationship is not plotted in the paper. Figure 6 presents the time series of BIA for each experiment and this might be a good place to add a BIA vs B plot (and perhaps in relation to Q, Qin/B and Qin/Qs also?).

Reply: Except for showing the relationship of BI_Avs. B, Q_{in}, q_{in}, Q_{in}/Q_s, we list more essential dimensionless parameters in Table 2.

(4.1) In places the discussion of the experimental results seems a bit cursory. I think it's a good idea to explain relevance/significance of some of these outcomes within the results. One example is the B-BIA results Lines 195-200, especially in the proposed effect of Qin/Qs in sand bar shapes etc. (which is presumably an important process-based explanation for the results also). Note also, the comment above on the Q-effect on these results.

Reply: In Discussion section, we use dimensionless parameters to interpret the influence of physical parameters on the morphology of submarine braided channels. Although bar shape is a good way for process-based description, it is still controversial to generalize an objective indicator that can be applied to both submarine and fluvial braided channels. We use Table 2 to add more quantitative supports in our discussion section (see Line 321 to Line 324, Line 360 to Line 362 and new Fig. 14 and Fig. 15).

(4.2) Another is around line 220 -225 in relation to erosion, widening and maps of erosion-deposition. These are intriguing results that are passed over quickly and also need a little more

justification, e.g., in the differences in channel erosion (widening) as a Qin/Qs effect rather than discharge alone being a cause of widening in some tests with larger discharge for the same confinement width in series A and B. For example, Qin/Qs cannot explain the clear differences in DOD patterns between B1 and B2 with almost identical Qin/Qs ratios. There is mention (line 225) of the areas of erosion-deposition becoming more "continuous and contiguous" but it would really help to support this claim (and its significance) in the analysis and text because it points to differences in processes and channel dynamics. This carries through to lines 235-240 where discussion and claims of influential variables are also a bit cursory.

Reply: Runs B1 and B2 show the influence of channel primitive width (B) under the same Q_{in}/Q_s and q_{in} conditions. In this comparison, BI_A is proportional to B (see Table 2). The main reason for the bar shapes of Series B become more continuous and contiguous is due to the increased q_{in} of Series B. As a result, many small channels merge into a few larger channels, making the BI_A of Series B slightly decreased. This agrees with the dimensionless sediment-stream power (ω^{**}). Under the same conditions of B, h, and S, the larger Q_{in}/Q_s will make ω^{**} smaller, and the corresponding BI_A will also become smaller. We update this point to the text (see Line 365 to Line 367).

(5) The modeling results are very interesting and reveal patterns of flow and sediment transport that cannot be physically measured. They add insights that help to see the differences between test results and the possible flow-transport explanations for these differences. They also support some of the additional analysis in the discussion (e.g. Fig 14). It would help to connect the model results to the physical test results more explicitly and explain how this modeling supports and extends the physical tests.

Reply: The direct comparisons between modeling results and physical experiments are shown in Fig. S11 to Fig. S16 (in the Supplement). Errors are controlled within an acceptable range (see Fig. S17). We decide to put these results in the Supplement in order to control the length of the manuscript. Therefore, we are confident to use the same calibrated parameters to predict the flow pattern under extreme events (i.e., double inflow total discharge). The results show that the linear relationship between dimensionless stream power (ω^{*}) and BI_A still holds for our model (see the star symbols in Fig. 16).

(6) Fig 14b implies that B/h is a (the?) major control on BIA. Perhaps this suggests that B/h could be used earlier in the analysis of the experimental results? This would also be consistent with fluvial theory and observation.

Reply: We agree that B/h is indeed a simple and direct indicator, which can be used to classify first-order channel morphologies. We superimposed our experimental data in the modified Parker's diagram (see Fig. 14). The results agree with the theoretical prediction that smaller values of h/B and larger values of S/Fr_d result in more braids. Our results are consistent with those of Foreman et al. (2015) and Lai et al. (2017) for laterally unconfined submarine braided channels. These results are also true when comparing to field-scale fluvial rivers and submarine turbidity channels.

(7) A question. Many fluvial braided relationships are from bedload dominated (often gravel) systems. Does the finer grained and suspension regime in turbidity flows make a significant difference to the processes and 'behaviour' of these braided systems?

Reply: In our experiments, the plastic sand provided upstream of the water tank is used to simulate "bed load", so that our experimental submarine braided channels are the result of bed load. We use saturated brine to simulate turbidity currents, implying that the suspended load is completely dissolved in the saline underflow and will bypass the entire basin. Therefore, in our experiments, we cannot observe the contribution of fine sediment settled from the underflow. In terms of sediment transport, we add our experimental data in the Shields's diagram (see new Fig. 15). The result shows that sediment transport behavior of the submarine braided channels is similar to general criteria for fluvial rivers. In future work, it would be a potential topic to compare the difference between submarine braided channels formed by saline underflows and turbidity currents. If there is fine sediment that can settle, there should be a chance to observe the distribution pattern of fine grains.

(8) And a few more minor suggestions:

Presentation of the experimental methods could include information on how sediment input was done and controlled along with experimental design on the basis for the choices of width, discharge and Qin/Qs ratio. The paper mentions D50 of the sediment but not whether it is uniform or not.

Reply: The stable dry sand supply is controlled by a motor-driven conveyor belt. The values of Q_{in} and Q_{in}/Q_s are decided based on the past successful cases reported in Foreman et al. (2015) and Lai et al. (2017). When q_{in} is determined, we can determine the amount of sand to be added by controlling the ratio of Q_{in}/Q_s. The choice of B is based on our basin width (W = 550 mm). In order to distinguish it from previous experiments (Foreman et al., 2015 and Lai et al., 2017) with full basin width, the way we choose B is, B = 0.12/0.55 = 22 % of W, 0.24/0.55 = 44 % of W and 0.48 /0.55 = 87% of W, which represent the proportions of the confinement width occupying the valley width from small to large proportion, respectively. The sediment used in this study has a uniformity coefficient, $Cu = d_{60}/d_{10}$ = 1.64 < 4 (d_{60} = 0.46 mm, d_{10} = 0.28 mm, d_{50} = 0.34 mm), indicating that our plastic sediment is uniformly graded. We update the above descriptions to the Method section (see Line 86 to Line 89 and Line 95 to Line 99).

(9.1) The automated channel mapping using dye intensity is very useful and gives a great visual impression of the channel pattern. From this the BIA is derived but the explanation of how active channel are defined (as opposed to identifying the dye channel threads that are not active) and extracted from these thresholds could be made more explicit. Or are all identified channels assumed to be active?

Reply: We assume all captured channels are "active channels." The automated channels are verified by comparing them to those active channels observed in the experimental videos and time-lapse images. After the imaging thresholds are optimized, we then applied the same criteria to all images. According to our experimental observations, if a submarine channel is nearly semi-transparent (or presents in light green color), it probably has no ability to transport bed loads. Our current automatic imaging method is not designed to distinguish those semi-transparent submarine channels.

(9.2) Also, why is colour separation needed – in what way does that enhance the process and result?

Reply: Using original color images, the quality and precision of the captured channel positions is unsatisfactory, i.e., there is too much noise. Our image processing workflow is to first convert a color image to an “enhanced gray-scale image” and then convert the enhanced gray-scale image into a binary image so that the channel positions and numbers can be captured automatically and precisely.

(9.3) Using Fig 3 as an example, what automated values are used to identify channels and on what basis would that identify, for example, two channels rather than one in the centre of the middle cross-section? The automation is clearly a hugely useful technical step to increase sample size, but does it still require a decision about the threshold value?

Reply: On the binarized image, the positions of channels should be white, i.e., the illumination value should be unity. When the channel has a discernable width, the corresponding brightness values would act like a top-hat function, which will make it difficult to capture the corrected channel positions. However, this kind of top-hat function can easily be diffused by a Gaussian filter and make the peak brightness easy for identification. Then the channel positions can be captured through a find peak function in Matlab. We use the built-in “findpeaks” function in Matlab. The more sensitive the parameters are set; the more channel numbers and locations will be captured. The parameter settings need to be calibrated with the experimental images for optimization. After that, this criterion is ready to be applied to all experimental images.

(10) Qin/Qs is one of the main variables experimentally varied. It would help to explain a bit more in the introduction why this might be an important control and (in relation to 1) to say more about the choice to constrain it to essentially two values – one for series A (approx 60) and one for series B (approx. 90).

Reply: Please see our reply to questions (2) and (8) for the setting of experimental conditions. These descriptions are integrated in the Method section.

(11) “higher confinement width” is a bit distracting as a term – it means wider channel, but “higher confinement” might also imply narrower channel. Would using “higher width” instead be a problem?

Reply: In order to better clarify the definition of various widths and avoid misunderstandings. We add inflow width (*b*), confinement width (*B*), valley width (*W*) on Fig. 2 and Table 1.

(12) Some figure captions and labels could be changed to it easier to follow the results and data. Examples include Fig 3 where the images a-f could be labelled and/or explained in the caption. On Figs 6 and 8 it might help to label each with B, Q and Qin/Qs to avoid having to refer back to Table

1. On Fig 8 are the difference DEMs between time 0 and 6000? – it's not clear what times were used to do the differencing.

Reply: We add the explanation to the figure caption of Fig. 3a to Fig. 3f. We label the experimental conditions and calculated time interval (dt = 4800~6000 s) in Fig. 6 and Fig. 8. We rewrite most of the figure captions to include more information.

(13) A small point on Fig 8; areas of bank erosion are picked out on some panels and mentioned in the text but erosion is also apparent in other cases e.g. B3?

Reply: We mark the position where bank erosion occurs for each run in Fig. 8.

References cited in our reply:

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