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Comment on esurf-2021-92

Jeongyeon Han and Wonsuck Kim

Author comment on "Linking levee-building processes with channel avulsion: geomorphic analysis for assessing avulsion frequency and channel reoccupation" by Jeongyeon Han and Wonsuck Kim, Earth Surf. Dynam. Discuss.,
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We sincerely thank the reviewers for providing constructive comments which tremendously helped to improve and clarify our manuscript. We did our best to cover all the points you have raised in detail and our response to each comment is below in italic font.

In a revised version of the manuscript, we changed the title to clarify the main concepts of our study as:

"Linking levee-building processes with channel avulsion: Geomorphic analysis for assessing avulsion frequency and channel reoccupation"

Response to Douglas Edmond

I have now read the paper submitted to Earth Surface Dynamics by Han and Kim. The paper presents a 1D advection-settling model of levee formation, and then uses that model to make insights into how the avulsion process is linked to levee building. I found the ideas presented to be thought provoking—it would be remarkable if we could link levee building and river avulsion style. I think that the paper is overall interesting and I applaud the authors on a job well done, and like many ideas they can benefit from revision. I provide some suggestions below and in a marked-up pdf that I hope will be helpful to the authors.

Thank you for taking the time to review our manuscript and giving such positive and constructive comments which improve the manuscript significantly. Based on the reviewer's comments, we could further refine the manuscript and better support our findings.

Suggestions:

I supplied a marked-up pdf with many questions and suggestions for writing clarity.

We appreciate all the detailed line-by-line comments on our manuscript. We mostly edited the manuscript based on the comments given in the mark-up pdf and selected some major comments and questions in the pdf and listed them at the end of the major comments below with our responses for them.

I think that the authors have inappropriately assumed that local and regional avulsions correspond to annexational and progradational, respectively. The work we have done (Edmonds et al., 2016; Valenza et al., 2020) show that avulsions overwhelmingly reoccupy existing pathways (regardless of whether they are local or regional). Even progradational avulsions usually have some portion of their new channel that reoccupies a pre-existing channel (making it partially annexational). In my understanding of the Heller and Paola (1996) definition, local and regional do not refer to style (i.e., progradational or annexational).

Thank you for pointing out the right use of the terms. We deleted the terms 'local' and 'regional' to avoid potential confusion but emphasized the effects of levee geometry on the avulsion styles (i.e., progradational and annexational). We still believe that the levee geomorphic condition can lead to more (or less) reoccupations of abandoned channels and cause annexational (or progradational) avulsions. In the revised manuscript, we enhanced our description of levee geometry-avulsion style linkages and further explained potential stratigraphic channel stacking patterns associated with channel reoccupation.

The authors could help the reader by refining their conceptual model. They present an interesting idea in section 4.5.3 but it is a bit hard to follow. I think a cartoon schematic might be really helpful here to follow the logic. In particular, I am not sure how channels get 'removed' from the floodplain in the conceptual model. Do you assume they are filling over time from flood sediments? It is also not clear in this conceptual model and the one in 4.5.4 where exactly 'upstream' or 'downstream' are relative to the mountain front. Does upstream mean right at the transition from confined to unconfined as flow leaves the mountain?

First of all, we added a schematic in Sect. 4.5.3 to explain our conceptual model in response to the reviewer's suggestion. We changed 'removed' to 'covered' and emphasized the process of infilling of the abandoned channels by overbank deposition which can reduce their topographic lows. In the schematic and enhanced description in the text, we tried to show the topographic lows in abandoned channels can be more effectively covered by deposition during floods for the gentler levee case compared to the steeper levee case. The gentler levees in our findings 1) indicate a wider spread of floodplain deposition, 2) develop relatively shorter adjacent local floodplain relief, and 3) take a longer time to reach the next avulsion, all of which provide better conditions to the influx of the overbank deposits to the abandoned channel.

Lastly, we didn't imply 'upstream' and 'downstream' as exact locations along a river or specific depositional environments, but rather we tried to indicate general changes in grain size and flood regime along a river upstream to downstream.

Section 4.5.4 contains some interesting thoughts, but I find it to be quite speculative, and it might be more impactful to combine that section with the previous one into a clearer conceptual model that can guide the thinking of others. As it stands, I am not sure how useful it is to speculate on these stratigraphic implications when the model presented is

quite far detached from stratigraphy and heavily simplified.

Great suggestion. We provided a figure of our conceptual model and avulsion styles to describe what are progradational and annexational avulsion styles in our view, which can connect well between the current and previous sections and thus guide readers better to follow our reasonings. For the stratigraphic implications, we rephrased sentences to reflect the reviewer's comment, and add more references to support our discussion. The current discussion is admittedly based on simplified assumptions. Still, we expect that this would bring a healthier discussion in the community about linking levee-avulsion-channel stacking patterns so we would like to keep the current chapter.

The authors make the unstated assumption that the alluvial ridge (and hence superelevation that drives avulsion) is governed only by suspended sediment deposition during overbank flow. I think that is probably true for rivers that don't meander. But for rivers that meander they are constantly cannibalizing their outer bank levee and leaving behind their inner bank one. Over time and many cutoffs, this could create quite a complex amalgamation of levees, scroll bars, abandoned channels, etc. This 'amalgamation' is the meander belt which probably contributes to the construction of the alluvial ridge. In that way, alluvial ridges could be more than just the levees that are attached to the main channel, and the time history of overbank deposition could be important. I think this work would be stronger if the authors could clarify the limitations of their conceptual model and/or clearly state these kinds of assumptions.

The alluvial ridge is developed by both suspended and bedload sediment transport and deposition and would be varied by inner or outer bank locations and speed of lateral migration in the field. This would be a great future research topic but beyond the scope of the current research. The current study focuses on the first-order understanding of the linkage between the levee geometry and avulsion process, which will provide the basis for the understanding of more complex conditions e.g., meandering river and bedload-dominated levee deposition. In the last discussion section, we provided our initial idea of how to use the current insight into more complex conditions e.g., lateral channel migration, and suggest expected stratigraphic patterns. We also have updated the limitations of the current model more clearly in the new model limitation section (Sect. 4.6).

Minor comments:

L50 When you say accounting for avulsion, what do you mean? Do you mean that there are no levee building models that are connected the channel such that the model can create levees as well as aggradation of the channel bed and superelevation?

Yes, the reviewer is correct. We have specified the sentence as:

"...accounting for the river avulsion processes by measuring depositional patterns of levee deposits..."

L60 To be fair, Mohrig et al found that many rivers avulse at a superelevation < 1 . This rule is more of a convenient short hand rather than a conclusion from that study.

We have corrected the sentences as:

"It is traditionally thought that the river would be ..."

Fig.1 What is the boundary condition at the downstream end of the model? Do flow and sediment leave through here or is it treated like a wall? L148 Is this choice because the Vistula River floodplain is that wide? What happens to flow and sediment at this boundary, does it leave the domain?

We have added the downstream boundary condition and the reference (Wyżga, 1999) in which we collected the floodplain width as:

"We also assign the extent of inundation as a floodplain width (L) of 200 m (Wyżga, 1999) and suspended sediment reaching over the floodplain width ($L > 200$ m, here) would leave through the downstream boundary of the model domain."

L137 I am confused, S_f does not appear in your model equations and in Fig 1 the floodplain has no slope. Do you mean down-valley floodplain slope?

We meant S_f as an initial cross-valley floodplain slope, and we set S_f equal to the downstream channel slope. We have clarified this both in the sentence and in Figure 1.

L140 Is this the right equation number? I am not sure because it refers to the rouse number.

Yes, this is correct. However, we agree that the explanation might be confusing so we rephrased it.

Fig.3 (b) I really don't understand how the constant depth vs constant water level case can create the exact same result. Something seems off to me. The constant flood level should faster velocities near the levee that decelerate toward the floodplain. This should create SOME kind of change (even if small).

As we indicated in Figure 4 (b2), there are small but differences in elevation change over time. We explained our interpretation of why they look similar under the test conditions in L294-296. At the channel-floodplain boundary, all conditions are the same as the flood level and levee-crest elevation aggrade at the same rate and thus depositions near the levee crest for two tests are similar. As the distance increases from the channel margin, sediment concentration becomes small resulting in only minor changes. Yet, we rephrased some sentences to enhance our explanation.

L202 Test 2? For Test 3 the rates look identical to Test 1.

Please see our response above explaining Test 1 and Test 3. They look similar but slightly different. We changed the sentence to indicate this.

L199 This seems obvious to me given that you have a steady flow and supply of sediment and no feedback in the model to slow down deposition.

Good point. We incorporated the reviewer's comment into the sentence.

L253 much lower? It looks to me like they are quite similar. Furthermore, on line 203 you say "Tests 2 and 3 are not significantly deviating from Test 1" but here you say test 2 is much lower.

We rephrased the sentence.

L256 True, but you could also show this in a figure by plotting the cumulative value of E over the run. Otherwise it sounds like speculation.

We added a new plot for showing E with time in the supplementary material.

L266 I don't follow how these different water level cases refer to wide and dry vs narrow and wet. Seems

Considering the cross-valley gradient in the water level during a flood, the 'wide and dry' case can drive faster flood flow with a water surface gradient over the floodplain while the 'narrow and wet' case leads a filling of water in a river valley without a significant water surface gradient. We enhanced our explanation in the text to reflect the reviewer's comment.

Fig.7 later The avulsion frequencies you report in this figure are VERY fast, and I understand that arises in your model from some simplifying decisions (e.g., not including floods, though curiously you do account for intermittency later on, so I am not sure why these are so fast), but it would be good to discuss/mention this in the text.

As stated in the manuscript, our justification for the intermittency is in L352-354. We reset this value of 0.002 based on the depositional conditions accounting for the estimated sediment flux in the floodplain. Thus, the avulsion frequency shows an order of magnitude reduction which can be more reasonable compared to the natural systems (cf. Fig. 7a in the revised manuscript). However, we would like to emphasize that the main conclusion of this study does not change depending on the range of avulsion frequency.

L317 Important to note that this is an avulsion criterion only when the channel bed ALSO aggrades. If only the levee aggrades then it won't necessarily create an avulsion.

True, we also think that the aggradation rates of channel and levee will be different rather than the same in natural system. This simplified assumption of equal aggradation is still a reasonable first-order approximation to understand the channel-levee complex as it has been used in previous studies (e.g., Jobe et al., 2020). We have added a new paragraph, section 4.6 in the discussion to describe this limitation in the model clearly.

L324 The use of H_c here is confusing. H_c is the depth over the levee, not necessarily the channel depth measured from the bed to the top of the levee, which I think is what you mean when you use it here. H_c (as it is drawn in Fig 1) could be much larger than the initial channel depth.

We changed H_c to H_t which represents the total water depth including the bankfull channel depth (H_c) and the overflow depth above the bankfull (H_f).

L351 It is worth mentioning here or elsewhere that this assumed the alluvial ridge is built entirely by overbank flow advected from the channel. In reality it is equally plausible that alluvial ridges are constructed (in part) by the meandering and cutoff dynamics of the river. In other words, the alluvial ridge can plausibly be a complex feature that is more than just the levee.

Thank you for the suggestion. We agree that lateral migration of channel and cutoff dynamics would affect the levee deposits and can be more critical to the alluvial ridge formation. Since the meandering and cutoff dynamics are beyond our scope of this study, we have added limitations that the current model only considers levee deposits on the alluvial ridges in section 4.6.

L359 Did Mohrig et al. 2000 present values of levee relative to downstream slope ratio? I am not sure I follow you here.

Yes, Mohrig et al. (2000) discussed the avulsion setup comparing the floodplain slope ratio (a ratio of levee relative to down-valley floodplain slopes) vs. superelevation. In Table 1 of Mohrig et al. (2000), they presented both the normalized ratio of levee slope to down-valley floodplain slope and the ratio of levee height to channel depth (superelevation) in modern avulsive rivers and showed the former has larger variabilities than the latter suggesting the superelevation threshold can be a better criterion for the avulsion.

L365 If this is true then it implies that levee slope does not matter since levees near the channel grow faster than the distal parts. right?

Of course, in the current simple model, the levee grows faster in the proximal compared to the distal locations, but does not include any phase changes (i.e., two phases of proximal rapid growth and backloading). We think such a two-stage levee formation is not applicable to all the natural levees and even with the two-stage formation, the proximal levee would grow with different slopes based on the boundary conditions. Here, we rather tried to explain why the floodplain slope ratios are much scattered to highlight the importance of both the superelevation and characteristic levee slope for evaluating the channel avulsion processes.

L365 What is meant by an "increment in the floodplain slope ratios"?

Rephrased.

L382 This sentence is confusing, do you mean something like "Because abandoned channels are topographically lower than the floodplain and have more easily erodible materials than nearly floodplain substrate, they readily capture overbank flow and route avulsions."

We rephrased this sentence.

L385 The gradient is not what 'protects' the abandoned channel, but rather than height of the levees relative to the undisturbed floodplain.

We see the reviewer's point. We now have clarified how the levee geometry can protect the abandoned channel as:

"In the case of relatively steep levee slopes, the steep levees would extend to only limited distances to the floodplain, and consequently less modify their initial local relief between the levee crests and adjacent floodplain (Fig. 8a). Hence, the abandoned channels can be protected by the high gradient levees which would maintain their hostile surroundings for the influx of flood deposits."

In that sense, the slope of the levee would affect the local relief between the levee crest of the abandoned channel and the adjacent floodplain, and it can make the overbank sediments flooded from the current channel hard to fill inside of the preexisting abandoned channel as protected by relatively high relief.

L385 The relationships that levee slope is directly proportional to avulsion frequency arises (I think) because of grain size. Steeper levees are narrower because coarse grains settle faster and occupy less area. This assumes that the total overbank sediment flux is independent of grain size, and is always the same. It may be unlikely, for instance, that one ever encounters a river that supplies the same the overbank flux for a river with $d_{50} = 0.05$ mm and $d_{50} = 0.25$ mm.

We understand the reviewer's point. We agree that the total overbank sediment flux would not be independent of grain size in the natural system. If the grain size of a river increases, overbank sediment flux to the floodplain will decrease even with the constant river sediment discharge during floods because suspended sediment concentration becomes smaller based on the Rouse profile. However, the current study isolates the effects of grain size and flood discharge on the levee geometry to gain the first-order understanding of Linking levee-building processes with channel avulsion. Nevertheless, we are pleased the comment could formulate an interesting future study.

L386 I don't follow what you mean here, if avulsions happen faster why would that leave the abandoned channel less time to fill? What abandoned channel are you referring? The one left by the avulsion? Or the one the avulsion reoccupies next on the floodplain? I don't understand how avulsion frequency can be linked to the time it takes to fill an abandoned channel. Can you elaborate on what you mean? Do you mean faster avulsions would leave behind MORE abandoned channels? L399 So you mean that because the avulsion time period is longer than other abandoned channels in the floodplain have time to heal?

In our manuscript, the abandoned channel in the floodplain here represents remnant channels previously abandoned that may have any chance to be reoccupied. Assuming floods under the same conditions (e.g., flood regime and occurrence), if the avulsion happens faster, there would be fewer opportunities for infilling preexisting channels by overbank deposition and healing topographic lows.

L387 But you just said on line 384 that for steep levee slopes the abandoned channel is protected and unlikely to be reoccupied. You seem to contradict that statement here.

No, we suggested that steeper levee slopes can protect the abandoned channels from channel infilling, and reoccupation would occur as the abandoned channels can take advantage of their topographic lows. However, we rephrased and deleted some misconceptions in this paragraph for clarification.

L438 I don't understand why high lateral mobility would allow a short time for gently sloped levees to form. Do you mean that there would NOT be enough time to form gently sloped levees? I thought your argument states that steep levees form fast and gentle levees form slower, so if the time for gentle levees to form is 'short' wouldn't the time for steep levees be even 'shorter'?

We meant a short time for vertical accretion. We have updated the sentence.

L439 This proposed linkage needs more description.

Based on the above reviewer's comments including a new figure, clear definitions for different avulsion styles, and the relationship between inter-avulsion duration and potential levee vertical aggradation, we believe the current section can be understood better now. We also rephrased the sentence to clarify our point.

L462 To me this is not a conclusion so much as an implication/discussion point. Keep in mind that your model does not predict upstream to downstream changes in levee geometry, and there are no data that clearly show levees change geometrically in the way you propose from up to downstream.

We agree with the reviewer that grain-size downstream fining is common but does not guarantee that grain-size fining organizes a systematic downstream change in the levee geometry.

In the previous papers, Hudson and Heitmuller (2003) reported that "Kolb (1962, 1963) provided an interesting caveat. Data from 24 borings along a 265-km reach of the Lower Mississippi River in the deltaic plain revealed a progressive downstream reduction in levee width, thickness, and slope as the suspended sediment became increasingly wash-load dominated, resulting in a reduction in the amount of sediment suitable for levee construction.". Also, Cazanagli and Smith (1998) suggested that "Finer suspended sediment is more easily transferred over the banks, ... and deposition in distal portions of the levee reduces the slope". Their descriptions of the levee geometry are with our suggestion.

However, more field measurements should be collected to thoroughly confirm our proposed general upstream to downstream trend. We thus revised the last conclusion point that this is a potential view to understand the downstream change in avulsion style and should be supported by more field confirmation.

Response to Anonymous Referee

Han and Kim quantify geomorphic (elevation and grain size) change for levees in time and space and suggest a link of these to avulsion style. First, a 1D advection-settling model is presented.

Given the parameters investigated, input grain size and discharges change the elevation and grain size trends the most. Next, a scaling analysis is used to find the correlations between levee topography and avulsion styles. Results suggest that the levee slope correlates with avulsion frequency. However, avulsion style is assumed to be tied to aggradation rates at and beyond the levee. Recent literature on connections between avulsion and levees has focused on the crevasse splay in already existing levees. This work highlights the connection between levee morphology and morphodynamic avulsions, where both the channel bed and levees aggrade. The discussion of levee building on morphodynamic avulsions, presented here, is important in developing tools to help predict avulsion potentials. I look forward to seeing this work published after revisions, including:

Strengthening the connection between the 1D levee building model and scaling analysis/conceptual framework. Currently, these two components of the manuscript are not well connected. Two suggestions are 1) including a motivation for parameters investigated in the 1D levee building model. These motivations are revealed, in part during the discussion, and would be beneficial to learn about earlier i.e. Introduction and Test parameters; and 2) including a more complete introduction of the avulsion styles and expected floodplain aggradation. Additionally, the authors could base the 1D levee building model on one of the rivers discussed in the avulsions style section or highlight how the current setup is relevant to slopes and timescales of those rivers.

Including additional figures to help the reader understand the 1D levee building model better. I would suggest including the spatial variations in velocities, sediment concentration, and slope on the levee. The velocity and sediment concentration plots can especially highlight the links between levee shapes and velocities and/or sediment availability.

Timescales are addressed in both the 1D and scaling analysis. There is an opportunity to more clearly compare the findings of modeling durations in the 1D levee building model to the avulsion timescales.

We gratefully thank the reviewer for the insightful and encouraging comments and suggestions. All the comments from the reviewer are a great help in strengthening our manuscript. We reply to those major suggestions in the major comments below to provide more detailed answers and thoughts individually.

Major comments:

Line 48-49: The motivation right now is to “analysis the effects of geometry and deposition rate of fluvial levees on the stratal association of channel complexes”. However, the stratigraphy is not discussed again. Consider revising the text be more specific. This is

again brought up in line 64 “comprehension of river avulsion associated with floodplain architecture”, where floodplain architecture is not further described.

We have briefly added the possible applications of our modeling results to the fluvial rock record (Sects. 4.3 and 4.4) and how the channel stacking patterns appear in the stratigraphy depending on the floodplain architectures (Sect. 4.5.3) in the discussion of the manuscript. We note that in large scale, levee evolves on the floodplain and modifies the floodplain topography.

In Section 4.3 Overflow discharge,

“When applying this trend to modern and/or ancient examples of levee deposits, the similarity in d_{90} between the proximal and distal levee deposits (Fig. 5c) can be interpreted to arise from the high flood-flow velocities.”

In Section 4.4 Input grain size,

“Given the responses in the levee geometry and spatial grain-size distribution, it would therefore present the possibility of an increase in the overall grain size supplied into the floodplain when the thickness and d_{90} of the levee deposits markedly decline toward the distal locations from the main channel.”

In Section 4.5.3 Channel reoccupation vs. levee slope,

“The model of Jerolmack and Paola (2007) demonstrated that channel reoccupation repeatedly occurs within a limited number of active channels called “active channel set”. This active channel set thus may occur in concert with steeper levee slopes along the floodplain channels taking advantage of remaining local conduits and producing multistory sandbodies (or vertically stacked patterns) in the ancient avulsion deposits (Jerolmack and Paola, 2007; Sahoo et al., 2020; Slingerland and Smith, 2004).”

Line 138: The observation of channel and floodplain were based on (Wyżga, 1999) as stated in Line 135. A look at the cross-section of the Vistula River, reported there, has a maximum channel depth of 5m. Please specify or model how do varying channel depths affect the results? Furthermore, the floodplain water depth of 4m, reported there, is based on observation of the 1997 flood. It would be worth exploring what reoccurrence timescale this flood represents and how variations in water depth would affect the test results.

We see the reviewer’s point, but the exact channel depth for the field is not critical to our study. Our main conclusion would not change whether the channel depth is assumed as 5 or 4 meters. We agree that changing the flood level influences the flood flow velocity over the floodplain assuming the constant flood discharge. This also can cause different grain-size distributions and levee geometry. However, the overall trends that we observed associated with the input grain size and flood discharge would be the same.

We also approximated the flood intermittency (flood reoccurrence timescale) based on the given data which may not be precise, and this intermittency could change the avulsion frequency in the model. Though, the main conclusion we would like to suggest here remains the same. We appreciate the reviewer’s comments which formulate a great future research topic, but the tests using the current parameters still provide interesting outcomes that support our main conclusion.

Line 138-139: Velocities for both in channel and floodplain were given in Wyżga, 1999. Do the values presented here reflect the values measured by Wyżga, 1999 on the floodplain and in the channel?

Yes, we approximated the mean velocity values of the channel and floodplain presented in Fig. 3 from Wyżga (1999). As we mentioned above, our intention is not to reproduce the field area exactly, we tried to choose reasonable values within a range of parameters in nature, so the exact values are not critical for drawing our main conclusion.

Line 141: Please state the assumption and limitation for only looking at a case when channel bed and levee crest aggrade simultaneously. For example, Ganti et al. 2016 show that levees can independently grow compared to channel bed elevation.

This suggestion is also raised by the other reviewer. Please see our response above (the review comment for L317).

Line 142-144: Please explain how the total suspended flux in the channel and grain size were chosen.

Thanks for the comments and we have added justification for the parameters we took as:

"We approximate the total suspended sediment flux in the channel (q_{ts}) of 0.001 m^2 using the equation from Guy (1970) assuming a total SSC in the channel of 0.034 kg/m^3 which was roughly estimated from the Vistula River (Pruszek et al., 2005). We also allocate it to SSCs for seven different grain sizes at the channel near bed with the median grain size (d_{50}) of 0.125 mm estimated from Wyżga (1999)."

Section 2.3: Since the 1D modeling tests are introduced here, it would be beneficial to motivate the test cases either in this section, the previous section, or the introduction.

In response to the reviewer, we incorporated the motivation of selected parameters in Introduction part.

Line 158-160: I am not sure how velocities and flood water levels (hydraulic gradients) can be disentangled. What are the implications of changing velocities while keeping a constant water depth? For me, this means discharges in the river must increase. This increase in river discharge would have implications for the other parameters held constant in the river.

As you described here, varying velocity means a change in the flood discharge. In nature systems, this causes differences in other parameters. However, we tried to isolate the effect of each parameter on the levee evolution using the model. In the case of Test 3, the model keeps the overflow discharge constant and calculate flood velocity based on the relationship, $Q_w = Uf(t)Hf(t)$, and the flood level condition (i.e., the constant flood water vs. constant water depth cases). Therefore, the constant flood water level leads that the overflow velocity and depth are changed over time and space while the constant water

depth case provides constant water velocity across the levee length over time. Please also see the response above (the comment for L385), which is in line with the current comment.

Line 189-191: Slope variation is a major component of the results and discussion. It would be useful to plot the slope in addition to the elevation.

We appreciate the reviewer's suggestion to make a clearer comparison between our test results. Now we updated average slopes in Figure 3 for each test.

Figure 3: It seems like the plots are cut off in the y-axis for levee width < 20m. It would be useful to show elevations for the levee closest to the river. Since the authors are not defining where a levee ends and the floodplain begins in this model, I would encourage relabeling the x-axis to distance from the river bank. It would also be useful to describe the tests in the caption since this is the first time the reader will see the results.

We implemented the 2nd and 3rd suggestions in Figure 3. However, for the 1st point, we would like to keep the current plots. Since the model has 20 nodes over the 200-m modeling domain, the first node is at 10 m from the river bank. We keep track of this first node as the levee crest in the modeling tests. Even if we increase the number of nodes and produce more elevation data closer to the river bank, the overall results will be the same.

Section 3.2: I agree with the description in this section that results are strongly tied to the time of building deposit. Given the importance of tying levee slope and deposition time, it would be worth exploring and describing the relationship with slope and time to deposit, highlighted in lines 190-191.

We discussed this point in L305-306 in the revised manuscript.

Line 231: The upward coarsening in grain size for Test 2 is very interesting. I wonder if it hints at architectural style and therefore is still important to emphasize and incorporate even when elevation and mean grain size is not too different from Test 1. A comparison to the literature on grain size changes on levees (i.e. Bridge 2009) can strengthen this section.

We thank the reviewer for this interesting comment which can enhance the text. As you indicated, in the modeling result, for an environment with finer sediment that can be more selectively entrained, upward coarsening in the deposit would be stronger. We have overall added a more detailed application of our insight gained from the modeling results to sedimentary records. Please see the response above for the first major comment.

Section 4: A summary of the expected results based on motivating studies and the difference between model results and previous work can strengthen the beginning of the discussion.

These suggestions have been very helpful in strengthening the manuscript. We have incorporated those description in the beginning of the discussion.

Section 4: Since the 1D model is based on dimensions of the Vistula River (Line 135), how do the grain sizes on the levee compare to those of Wyżga, 1999?

It is a very good idea to compare the grain sizes of the levee deposits shown in our modeling result with the observations from the Vistula River. We appreciate the suggestion but in our model, median grain sizes of suspended sediment in the channel (0.125, 0.25 mm) are roughly estimated from a grain size range of levee deposits from Wyżga (1999) ($d_{50} = 0.27-0.33$ mm). Thus, this would be useful to verify our model if we can obtain the actual value of suspended sediment grain size distribution of channel or floodplain in the Upper Vistula River.

Line 272-273: Are the results presented here sensitive to the water depth on the floodplain? I would suggest looking at variations before drawing this conclusion here. Line 274-277: This seems counterintuitive to me. Could the authors plot the velocities away from the river bank for both cases to support their argument? Do changes in concentration affect levee shape and grain size trends, especially given the constant sediment discharge?

We think that the reviewer misunderstood the main point here. Within the conditions given to Tests 1 and 3, the different flood level cases didn't provide large differences in the results. The maximum water depth difference, i.e., 2 m occurs at the end of Tests 1 and 3 at the distal end of the modeling domain. This means over the evolution of the levees the water depth differences are less than 2 m. For example, in the early stage when the levee is small, water depths are very similar, resulting in very small differences in the results. For the far distal end, the sediment concentration is in fact extremely small even over the later stage of the run, which is reflected in the small differences in the modeling results from Tests 1 and 3.

We provide more plots (i.e., water depth, velocity and concentration) to support our explanation on this issue as the reviewer suggested.

Line 288-289: It is important to acknowledge here that other tests (T5 and to a lesser extent T2) produce the same results.

What we stress here is relative grain-size changes in d_{90} and d_{50} between the distal and proximal locations. Only for Test 4 when we increase overflow velocity, the difference in d_{90} between the prototype and Test 4 substantially increases at the distal location compared to the proximal location while the other tests show a decrease (or slightly increase) in the difference of d_{90} from the prototype towards downstream. However, we rephrased the sentence to avoid such misreading.

"In Fig. 6c, the difference in d_{90} between the prototype and Test 4 at the distal location substantially increase from the proximal location compared to that in d_{50} ."

Line 324: Could you clarify how the levee slope calculation is derived here? I am especially

curious how the channel water depth is included in the calculation.

Since we build the levee crest until it reaches the same height as the channel depth (superelevation), we used HC in the equation. We revised the equation as $(\eta_0)^2 / (2 \times \sum \eta_i \cdot dx)$. The equation is basically derived using the total levee area by integrating elevations at nodes over the levee:

Levee area = $(\eta_0)^2 / (2 \times \text{characteristic levee slope}) = \sum \eta_i \cdot dx$. Please see the full equations in the revised text.

Line 335: A major assumption of this model is that the in-channel aggradation rate equals the aggradation rate of levees. I think v_a is traditionally expressed as the channel aggradation rate. Please elaborate on the implications and limitations of this assumption.

Thank you for pointing this out. We realized that the parameter, v_a can be confusing to readers if we use it without explaining our assumption here so we changed v_a to the channel aggradation rate and revised the sentence as below.

" v_a is the vertical aggradation rate of in-channel bed which is the same as the aggradation rate of levee crest in our study."

Equations 12 and 13: Please describe the implications of deriving avulsion frequency in two different forms and how they compare.

Basically, equation (12) shows a time duration for building a levee with a given volume using a given sediment supply, which scales the timescale for an avulsion. Here the sediment flux q_s is one during a flood. It is generally accepted that the morphodynamic evolution is significant mainly during a flood. Under this assumption, equation (12) indicates a total duration for active floods that takes to build the levee to trigger an avulsion. Equation (13) is one over this duration, a frequency. However, to get a frequency based on the real time (including inter-flood durations), we need to consider the intermittency.

Line 348-349: To better assess this statement, it would be useful to know how avulsion frequency and levee slope were found in Figure 7.

We appreciate the reviewer's comment but the descriptions of how we calculated the avulsion frequency and characteristic levee slope were already presented in Sect. 4.5.1. Alternatively, we updated the sentence implying that values are from our levee model.

Line 363: The 1D levee building model results suggest that steeper sloped levees took longer to superelevate compared to the initial case. Please clarify how the 1D levee building model results are supporting your point here.

The reviewer is correct, for our Test 5, an increase in grain sizes of the channel reduces the overbank sediment flux and it takes a longer time to reach the critical superelevation. However, when we calculate the avulsion frequency via our levee-building model, we assume the total overbank sediment flux is independent of the grain size distribution so

that all model runs in Fig. 7 have the same overbank sediment flux. A steep levee can reach the threshold height at the crest with a smaller amount of sediment and result in a higher avulsion frequency. A similar suggestion was made by the other reviewer (see the response above for L385).

Line 364-366: This conceptual framework is not clear to me. Can you link this to the results in the 1D levee building model to clarify?

In response to the reviewer, we have clarified the sentence. Unfortunately, our 1D levee-building model does not include the bi-directional influences between avulsion frequency and levee topography, (i.e., backloading). We instead tried to suggest a possible reason for wider variations in the floodplain slope to highlight the importance of both the superelevation and characteristic levee slope for evaluating the channel avulsion processes.

Section 4.5.3. To my understanding, a major assumption for this section is that floodplain building time scales are the main driver in aggradation across a floodplain. However, concentrations are equally as important. The implication for varying sediment concentrations needs to be discussed in greater detail. This might also clarify the linkages between levee building and floodplain aggradation throughout, especially beyond the initial 200 m presented in the 1D levee build model.

We agree with the reviewer that depending on the levee geometry, varying sediment concentrations in the overflow can be an important matter in the floodplain aggradation as well as abandoned channel infilling processes. Although changes in sediment concentration were not directly taken into account in the current conceptual model as they are beyond our scope of this study, it can be a great future research topic. We now elaborated our descriptions of the linkages between levee geometry and floodplain deposition in the text.

Line 385: Could you please clarify how high levee gradients prevent the influx of floodplain deposits? Levee gradients mainly affect water depths associated with lower or higher elevations further away, assuming the same levee crest elevation is reached. I could see how higher levee crest elevations could change the sediment concentration and grain size distribution entering a levee based on decreasing concentrations associated with the in-channel Rouse profiles. Line 418-420: See comments to line 385. Please clarify the links between steep topography and floodplain deposition.

The reviewer makes a great point on the linkage between levee gradients and an influx of the floodplain deposits. We tried to explain that the conditions to build higher levees can develop higher floodplain reliefs between the abandoned channel levees and floodplain surface, which aid in protecting the abandoned channels from filling. As mentioned above, we have added a schematic and further updated the manuscript to clarify their linkages.

Section 4.5.4. This section could be significantly strengthened, including linking the 1D levee building model and results. One suggestion is to introduce modeling results specific to the upstream and downstream scenarios presented in Valenza et al. 2020. For example, the channel geometry and thus suspended sediment concentration might also be inherently different for these sections described which the model comparison can include.

We appreciate the reviewer's suggestion. The current section is for expanding our insight through the modeling about avulsion styles to field cases. Even though the suggestion is a good idea to verify our levee building model in the upstream and downstream scenarios presented in Valenza et al. (2020), there are data not enough to conduct meaningful modeling, we would like to keep the current conceptual way to discuss the field application. Yet, we believe that the revised manuscript can be enough to guide readers to better understand this section since we newly have added a figure for the conceptual model and a schematic to describe what are progradational and annexational avulsion styles in our view.

Line 438-439: This is counterintuitive to me. For example, Adams et al. 2004 mention that levee height is established relatively quickly, and width increases with time. How do these results compare with the levee modeling results presented earlier?

Our model builds a levee continuously assuming continuous floods. In nature, however, there are intermittent floods. Imagine that channel overflows and builds a levee layer by layer over multiple floods. Each layer has a triangular wedge shape, so the levee slope would increase over floods. Now, if there are two channels: one has no lateral migration but the other migrates laterally with bank/levee erosion, the rapid migration channel would not have time to build a steeper levee because it erodes into the outer bank and levee deposits over floods while the stationary (slowly migrating) channel would have enough time to build a steeper levee. Note that the same conditions are assumed in these scenarios except the channel lateral migration to isolate the effect of lateral mobility. Here, we intended to explain a short time for vertical accretion. We revised the sentence to avoid further misunderstanding.

Minor comments:

Line 98-99: Please state the implications for the channel total sediment flux and near-bed concentration being kept equal. Are there any studies that can support this in-channel trend?

It is common to set sediment flux and near bed concentration and/or other parameters constant in modeling studies to gain a first order understanding of the system. We have added a supporting reference using constant sediment flux and/or near-bed concentration under a simplified steady flood condition to reflect the reviewer's comment. We note that both parameters can be relaxed to be time variables in the model.

Line 106: Please describe the part of the model and subsequent results that rely on the assumption that deposition takes longer than flood inundation.

We meant deposition that can make a significant morphologic change to influence the flood flow takes longer than the flood flow inundates over the floodplain.

Line 131-132: Please describe the implications for assuming porosity is zero for stratal architecture and mass calculations made later.

We assumed no porosity to simplify our model, but easily include a certain value for the

porosity. The porosity may decrease the avulsion timescale and increase the avulsion frequency, but the overall conclusion will be the same.

Line 160-161: What are the motivations for choosing 2m as a crest height to reach. Are there links to the deposition measured on the Vistula River?

We simply limited the levee height until it reaches half of the levee crest because even in the mid-stage before achieving the critical superelevation, levee geometry and grain size distribution evolve to show significant trends in response to various initial boundary conditions.

Line 163-164: I am confused if equations 7 and 8 are used in all test cases and incorporated in Eq 9. Please specify here when these equations are used.

As stated in the manuscript, we used Eqs. (7) and (8) only for Test 2 whereas we assumed no entrainment for other tests ($E = 0$ in Eq. (9)). We revised the sentence to reflect the reviewer's comment.

Figure 4: Comparing results between tests is not straightforward in this layout. Is it possible to plot all test cases on one plot for proximal and another for distal?

A plot with all test results is rather complex to indicate an individual effect of the change in the boundary condition. Instead, we have added a slope for each result line in fig. 4 to strengthen the comparison across all the tests.

Figure 4: I would encourage plotting the entire run time for all tests cases. This can nicely highlight the run time variations for different cases.

We implemented the suggestion to the figure.

Figure 4: The comparison between normalized ratio of elevation difference shows an interesting nonlinear change in normalized elevation difference over time that also seems to be different between proximal and distal. How do these results compare to other test cases? It would be interesting if the authors described the implication of these in the discussion.

We thank the reviewer for this interesting suggestion. We used the subplot for Test 3 to emphasize the impact of flow dynamic changes towards the distal levees. We plotted the normalized ratios of elevation difference for other tests in response to the suggestion. However, the results do not change our main discussion and we still believe that the original plot already shows a clear trend in aggradation rate changes from proximal to distal locations without normalization. Therefore, we would like to keep the original figure and discussion.

Line 199: "suggests that the local surface elevations increase linearly". This also means that no equilibrium is achieved. Is the conclusion from this model that there is no equilibrium form that levees achieve or is it a result of the model set up?

Good question. The model levee has no equilibrium topography. The advection settling distributes more sediment based on the concentration and flood velocity and aggrades the levee surface continuously. The current model limits the growth until the levee crest reaches the superelevation, otherwise, the levee would be consistently taller. We think that a future model should include bedload sediment transport, so a steeper slope drives more sediment transport to limit the continuous growth of a levee. Unfortunately, this is beyond the scope of the current research.

Figure 5: Please specify the time of run these results represent.

We think this comment is for Fig. 6. We thus updated the total run times in Fig. 6.

Line 324-325: Since the levee slope is defined by total sediment volume, does assuming a porosity of 0 affect these results?

Yes, it does. Please see the response above (the comment for L131-132). The porosity parameter would increase the avulsion frequency as it can increase total sediment volume. However, we ignore this effect in our study to focus on first-order controls of levee morphodynamics.

Line 325-326: What is the motivation behind using a different sediment flux from initial levee modeling?

Since we assumed total suspended sediment flux supplied into the floodplain is the same in all levee model for calculating avulsion frequency and characteristic levee slope, we designated the approximated overbank sediment flux based on the value of our prototype model, Test 1. Thus, except for Test 5, the values of overbank sediment flux in the initial levee building models are equal to this value of $0.0003 \text{ m}^2/\text{s}$.

Line 326-327: Is this the same flood recurrence interval related to the flooding depth that was used in the initial levee modeling? Please elaborate on changes if there are any. Would it be possible to calculate the flood duration from station data or how much does flood intermittency affect the results given the uncertainties/variations?

We appreciated the reviewer for these very interesting questions. We did not include the flood recurrence interval in the initial five tests since we only care about the total run time for each test. Unfortunately, the flood duration of the Smolice station in the Vistula River was not described in previous documents so we reset the flood duration of about 3 days when the recurrence interval is 3 years accounting for the estimated overbank sediment flux in the revised version. The intermittency value changes avulsion frequency as described above (please see the responses for Equations 12 and 13, L138 by the current reviewer and Fig. 7 by the other reviewer).

Line 376-378: The statements here are currently hypotheses. Please describe how are they supported by the results of this paper.

Thank you for the suggestion. In fact, we propose a new conceptual model of channel reoccupation related to levee geometry based on the insight gained through the modeling study. We now elaborated our description and added a schematic to strengthen our explanation.

Line 401-402: In addition to deposition duration, does sediment availability and concentrations at the distal part also affect more distal floodplain deposition patterns. Maybe this is where re- entrainment becomes important since the bypass of sediment, especially for d50 sediment, is occurring at the distal ends (Line 260-261). This could be a reason to keep re-entrainment in the modeling for this section.

We agree with the reviewer that sediment availability and concentration at the distal part affect distal floodplain deposition patterns, and entrainment might be important in the long-term floodplain deposition. However, as we demonstrated using the modeling results, within the parameter ranges we selected here, the effect of entrainment is minor, but the levee geometry constructed by ranges of possible grain sizes and flood velocities provide a significant impact on the avulsion frequency and thus channel reoccupation. What we focus on is rather the first-order control of the levee deposition on the channel reoccupation. However, we also believe that a more complex depositional environment in the floodplain should be incorporated in the future study.

Line 435-436: Please specify which processes investigated here are affected by channel mobility.

We compared the vertical accretion rate vs. lateral migration rate in terms of associated timescales (please see the response above for L438-439). To reflect the reviewer's comment, we have specified a definition of channel mobility in the sentence.

Line 469: Based on the findings presented here, is there anything in particular related to levees that could help better predict avulsions?

Very interesting suggestion. Since avulsion can be catastrophic in human life, it is crucial for us to predict when and where an avulsion would occur. Based on the study, we expect that a channel with steeper levees potentially can avulse more frequently showing dominant channel reoccupation. Despite levee deposits have not being thoroughly incorporated in avulsion process studies, we hope that the current findings can enhance our ability to predict avulsion.

Line 463-470: The statements here seem more like an outlook for future work than conclusions of the modeling and scaling work presented here. I would present the information accordingly.

The comment is also raised by the other reviewer. Please see our response above (the first reviewer's comment for L462). We revised the last conclusion point that this is a potential view to understand the avulsive patterns of modern river systems and sand

bodies which should be supported by more field confirmation.

Throughout the text: Alluvial ridges and levees are used interchangeably here. However, I think they can be associated with different timescales and processes. It would be good to define each and use them consistently.

Thank the reviewer for the suggestion and this is also raised by the other reviewer. Please see our response above (the reviewer's 5th key suggestion). We revised the manuscript to use the correct term between alluvial ridges and levees and updated the limitations of the current model as considering only levee deposits on the alluvial ridge in the new model limitation section (Sect. 4.6).

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