**<u>Reviewer #2: Ellen Chamberlin</u>**:

General Comments

The manuscript presents novel and interesting results from a new cellular model of river avulsions in a foreland basin setting that explicitly parameterizes the influence of abandoned channels in channel pathfinding. Although several models of river avulsion exist, there has been a lack of attention to floodplain topographic controls on flow routing, and this manuscript presents an interesting and logical new parameterization of the ways in which abandoned channels might control avulsion pathways. They treat abandoned channels in three ways: 1) their repulsion caused by the elevated topography of alluvial ridges; 2) their attraction because of channelized flow paths; 3) different modes of abandoned channel healing, depending on abandoned channel deposition and erosion. Model results show that all tested aspects of abandoned channels control avulsion location, especially whether avulsions are occurring in the proximal or distal reaches of the fan. They also show, most interestingly, that active lobes and lobe switching – that is, when avulsions occur in a clustered region of the fan before switching to a new, clustered region – only occurs under certain abandoned channel healing conditions. This suggests that abandoned channel topography exerts a strong control on the locus of avulsions and also the tendency for some fluvial fans to have active lobes. This is a valuable modeling contribution that sets the stage for field and remote sensing work to gather more real-world data about the role of abandoned channels to test some clearly defined model predictions. At the beginning of the manuscript, they also characterize the amount of abandoned channels on three fluvial megafans, to show that these are indeed prevalent features in modern environments. Overall, this is a well-written manuscript with exciting findings; the comments below are mostly minor questions or suggested revisions to improve the clarity of the manuscript before publication, although there are a few more significant comments, especially about the default channel healing mode used in most of the model runs.

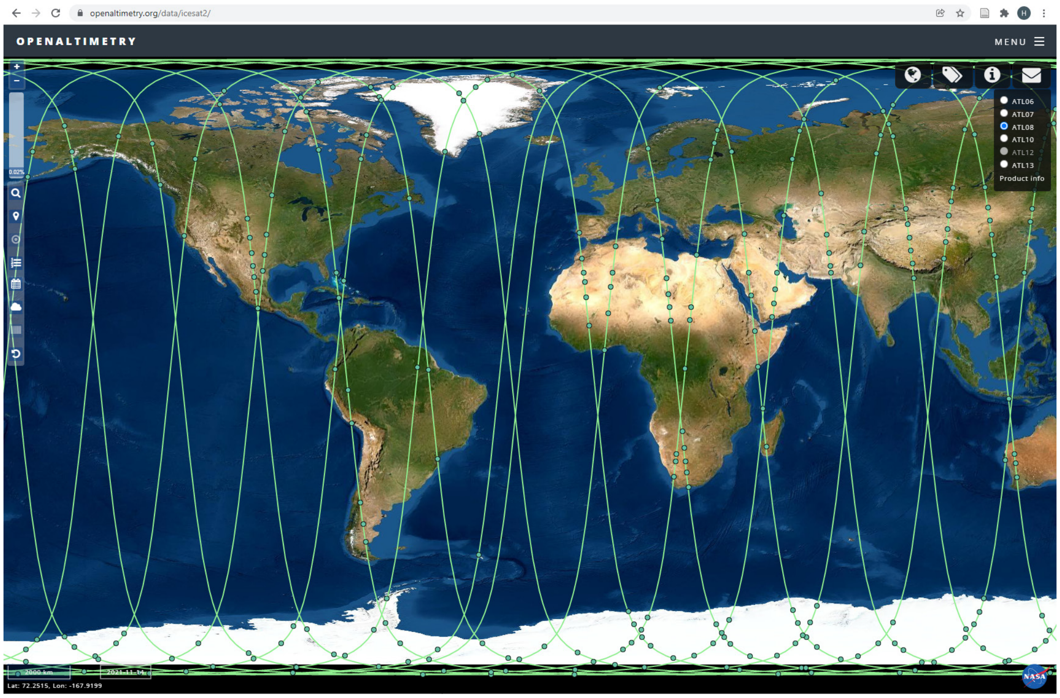
- Dr. Chamberlin has provided a thorough and useful review. Common themes that arose in the review were i) a need to move more of the background or literature review information from later sections to an earlier part of the manuscript, ii) extensions to the discussion and greater interpretation of some results, particularly lobe switching, and iii) additional explanation or justification of some of the model design decisions. We have been able to adopt most of these changes, particularly through the introduction of our new section 2 that collects background information and motivation and moves it earlier in the manuscript. We also took the opportunity to replace the literature review that used to be in the discussion with a model sensitivity section as well as additional interpretation of the results and applications or predictions regarding the stratigraphic record. Finally, we added to our design justifications throughout. We appreciate the reviewer's careful attention and thoughts, as well as their kind words.
- Note: line numbers referenced by the reviewer refer to the original manuscript, while (unless otherwise explicitly stated) line numbers in our responses refer to the revised manuscript with tracked changes accepted.

Specific Comments

**Base run and validation**

In section 4.1, the authors validate the model results by comparing a baseline run to 2 modern megafans. However, it is unclear why the strike and dip-section profiles are compared to two different megafans. Is the dip-section of the model inconsistent with the Taquari fan? Is it important to have reasonable consistency in both dimensions? A lot of the main conclusions of this paper are about the proximal-distal location of the avulsions, so the length-to-width ratio of the fan area does seem like an important property. Also, for the along-strike comparison with the Taquari fan, the Y-axis values are comparable but the x-axis values are very different; the plots make them look the same, but the axis values are different. How important is this difference? Additional text should be added to address this, and ideally the dip and strike profiles should be compared from the same fan system or systems.

- The reviewer notes an important limitation in the ICESat-2 data we used for our validation. ICESAT-2 data are limited to ~north-south oriented tracks (see two following images). As a result, we are unable to measure both along-strike and along-dip orientations for any one individual fan, necessitating the use of two fans. Within any individual fan, the model can reasonably recreate the length-to-width ratio of fan areas. In searching for a fan by which we could compare multiple along-strike cross sections, we encountered several obstacles. Unfortunately, the spacing between transects in ICESat-2 is rather wide (order kilometers), meaning that only very large (i.e., greater than the width of the simulation in our model) fans can have multiple along-strike transects measured, and there are relatively few of these that are generally oriented east-west with a minimum of human landscape disturbance and entirely continuous data without data gaps due to cloud cover on the day of measurement. Recognizing this limitation, we had believed that a reader would still benefit from seeing that we “recreat[e] the change in along-strike profiles” as one moves downstream. As a sort of disclaimer, we mentioned that the reader should “[n]ote the exceptional vertical exaggeration in the Taquari along-strike profiles”. With that said, we now recognize that this could confuse readers, and to avoid confusion would require adding a section about limitations in ICESat-2 data, which would distract from the point of the (already long) paper. As such, and because the content is not necessary for the core conclusions of our model, we have simplified the figure to only present the along-dip comparison to the Pucheveyem and updated the text accordingly in lines 467-476.





## Abandoned channel healing mode in the first 3 sets of model runs

Lines 374-387 describe four series of model runs that are then analyzed in the results section. If I understand this correctly, series 1-3 were all run in far-field directed healing mode, and thus the effects of the repulsion and attraction parameters were tested only with the far-field healing. However, in the fourth series of model runs, the authors show that the depositional and erosional healing modes cause major (and interesting!) changes in avulsion behavior. Do the impacts of the attraction and repulsion parameters still occur under the depositional/erosional channel healing modes, or are they only important variables under the far-field directed healing mode? It would be good to see some model runs added (perhaps just a limited subset of the model space) that address this question. Is the channel healing mode more important than the attraction/repulsion rules? Also, what is the justification for using the far-field directed mode as the default mode? Is there evidence to suggest that this is the most common/reasonable healing mode in modern floodplains? The authors mention that this is the least computationally intensive, but it would be good to see scientific justification for this as the default mode.

- The reviewer raises a great point in that our experiments on abandoned channel repulsion and attraction use the far-field directed healing mode, and we do not show corresponding results using the other two healing modes. The reasons for this are multiple. As a scientific justification, we began with far-field directed because it is the only healing mode that is truly able to completely "heal" topography, i.e., erase the topographic memory of both abandoned channel highs and lows after finite time. Of the three options, this is the closest to the endpoint of diffusion on an infinite plane. More practically, these results do hold for the other healing modes, but their effects are much more difficult to measure and display quantitatively as these two modes do not achieve dynamic equilibrium. As such, the effects these processes have on the location and abruptness of the transition between proximal and distal domains would have to be observed transiently during the first few Myr of simulation as progradation is ongoing. As such, for any specific distance from the mountain-front, measurements of spatial avulsion frequency necessarily integrate over both a distal and proximal phase of varying durations. It is also worth mentioning that all three modes are equally computationally intensive, but all three are far less computationally intensive that employing true 2D diffusion on the landscape at each timestep. This was our intended point of communication in our justification of far-field healing in section 4.4 (lines 444-446).

**Model mass balance variations based on healing mode**

Because of the way this model is set up, mass balance is not constrained between model runs, and that is not an impediment to the analysis. However, the different healing modes shown graphically in Figure 4 have very systematic differences in mass balance that might have big impacts on the model results. For example, the depositional only mode of healing would require a much larger sediment input to the floodplain than the far-field directed or erosion-only modes. There is no mention of this in section 4.5 or in the discussion, but I think this difference in mass balance between the model types warrants some analysis. If equivalent amounts of sediment were added into the proximal floodplain in the far-field directed healing model runs, even outside of the abandoned channels, wouldn't that also cause lobe switching? In other words, to what degree are these results caused by the abandoned channel healing versus just accumulation of more or less sediment in the floodplain around the active channel belt?

- This is a keen observation. We do believe that each mode confers a different amount of sediment and a different preservation potential for abandoned channel topography. This is likely inextricable from abandoned channel healing, considering how much of the floodplain is constituted by abandoned channel topography (Fig 2). We have elaborated on the origin and nature of lobe switching in the results (614-627, 632-637), and added to our discussion (693-697) with content about what lobe switching may or may not say about abandoned channel healing.

**Re-organization of introduction & background**

The introduction section of this manuscript is very short (3 brief paragraphs!) and does not give enough background to set up the hypotheses or the model set-up. Other sections of the paper (specifically section 2.2.2 and Lines 611-625 of the discussion) would be better suited to this introduction, so that more specific background about abandoned channels and cellular modeling of avulsions was provided to the reader before they continue on in the paper. Also, the topic of avulsion reoccupation is not mentioned in the introduction, but the observations from the modern and the ancient that channels commonly reoccupy previous channel courses is critical to the motivation for this paper. There should be at least some description of the evidence for channel reoccupation of abandoned courses in the introduction here.

- On the one hand, we stylistically prefer a concise introduction, and felt that doing too-detailed of a literature review in this section would interfere with the "flow". Additionally, we felt the need to demonstrate that abandoned channels are prevalent on fluvial megafans before discussing why they should be considered in modeling efforts. On the other hand, multiple reviewers have made the same suggestion, and it is a reasonable one. In retrospect, we admit it's a bit odd to have that much of a paper's justification in the Discussion. As such, we've added a new section between the introduction and remote sensing sections (2) titled "previous modeling works" in which we motivate the future work. Thank you for the suggestion.

**Discussion section revisions**

As noted with detailed line numbers in the following section, a lot of the discussion reads more like background about avulsion models and justification of the model rules used in this paper, rather than contextualizing these novel results. I think the background information and justification of model parameters should be moved earlier in the manuscript, and a more detailed analysis of the results could be added to this discussion section – especially thinking about their broader implications. For example, based on the prevalence of lobe-switching in modern fans, does this suggest that the deposition/erosion-only healing rules are most consistent with modern observations? Additionally, what are the implications for stratigraphic analysis of avulsion patterns? In systems with clearly clustered avulsions (such as the Ferris Formation; see Hajek et al. (2010)), does that suggest abandoned channels were attracting avulsing flow more than in randomly distributed systems (e.g., the Williams Fork Formation; see Chamberlin et al. (2017))?

- We have moved much of the discussion to a new background information section (2). In its place, we have reworked and added additional content explaining and discussing predictions for how different avulsion emplacement "rules" could reflect different parts of either parameter space or positions on/off of a fan in our models (670-688), with a particular mention of lobe switching (679-681). The predictive power of lobe switching on abandoned channel healing mode is now mentioned (693-696). We have included a brief reference to connect the work with the Ferris and Williams Fork formations to the end of section 6.4 (724-727) as suggested; thank you for the improvement.

Technical Comments

Section 2.2 - Megafan floodplain topography discussion: the organization/title of this section is odd, because this is really the background needed for the model set-up, not a discussion of the field results. I think almost all of the content in this section would be better in a background section about avulsion set-up, initiation and pathfinding that would come before the remote sensing section.

- We have added a background information section (2) that will hopefully help address concerns about the order in which model set-up is presented.

Figure 3: missing caption

- Correct. Fixed!

Line 140-141: There is also good evidence from the rock record that avulsion reoccupation of previous channel courses is common – e.g., see references in Chamberlin and Hajek (2015).

- Added (62-64, 212-214). Much appreciated.

Line 161: There are several studies that have observations of oxbow lake sedimentation rates, which is a type of abandoned channel sedimentation. These studies should be cited and discussed here. (for example, Wren et al., 2008, “The evolution of an oxbow lake in the Mississippi alluvial plain”).

- Added, thank you. Lines 88-90, 413-415.

Line 163: The language “if one assumes that abandoned channels do heal” is confusing – what would another option be? Over geologic time, they must heal, right?

- I agree with you, but in conversations with colleagues, it seems that not everyone is convinced. In aggradational basins they believe that the healing timescale of abandoned channels may be very long relative to the burial timescale, meaning that in a functional sense abandoned channels would not heal.

Line 168: The details in the discussion should be moved up front into a background section.

- Done, added section 2.

Table 1: Maybe “numerical” or “cellular” model is more clear than “non-experimental” in the table title?

- Done (242).

Line 263: This is an interesting way to code channel healing! Can you add some more explanation – maybe here, maybe in the justification for the model set-up – about the mechanistic justification for these different healing modes? In other words, for the depositional only healing, how would that actually work? Via overbank sedimentation? Via temporary reoccupation of the abandoned channels during floods? I know that this model is not attempting to resolve those processes, but some general outline of how each of these healing mechanisms could be possible would be helpful for thinking about the implications.

- This information was previously located elsewhere in the manuscript (old 2.2.2). You are correct that it's difficult to resolve the processes, or even to evaluate which should be more common, which is why we treated it as an experimental parameter. With that said, it's certainly true that it'd be helpful for a reader to have some sort of physical mechanism in mind, so I've moved it to a more fitting place with other abandoned channel healing considerations in the new Previous Modeling Efforts section (2), and added some additional context.

Line 333: I'm surprised by this decision to have floodplain aggradation independent of active channel position. Why are they decoupled? Additional justification of this choice would be helpful, because this would have a big influence on superelevation dynamics. This might be a point that would be good to add into the discussion and a direction for future modeling work.

- This comment was similar to the comment provided by Anonymous Reviewer #1, and a detailed response can be found in our reply AC1 under the reply to comments on Lines 331-332. We reproduce it here in brief: we make the assumption that the diffusion distance perpendicular to the flow direction (Pizzuto 1987) is contained within a single cell. More specifically, we assume that the coarse grains of the suspended load (very fine sand & silt) are not transported beyond the grid cell during a flood, instead contributing to alluvial ridge or levee formation. Assuming quartz grains in water, application of Stokes' law yields a settling velocity of ~0.005 m/s for very fine sand (75 um) and ~0.0005 m/s for silt (25 um). If we assume the flood is 1 m deep and there is an aggressive overbank velocity of 0.5 m/s, then very fine sand and silt would travel ~100 m and ~1000 m, respectively, before settling. The same exercise with clay-sized particles yields transport distances >10 km, which supports our uniform component of overbank sedimentation representing the sum total of all processes that deliver sediment far from the active channel (e.g., washload in overbank flooding, pluvial flooding, overland flow).
- With that said, I think that there is likely some rich behavior to explore specifically regarding spatially variable abandoned channel healing. The rates, and maybe even modes, of healing could be easily justified to vary with distance from the channel (hydraulic connectivity, washload differential deposition in topographic lows, erosion/reactivation potential during overland floods), or with distance from the mountain-front (micro-climatic or vegetation differences, increasing washload downstream, etc). These explorations are beyond the scope of this study, but we have ensured that a brief discussion of these factors is included in section 6.3 (708-711).

Line 341: I think you mean equation 12, not 17

- Correct, thank you.

Line 347: Put the detailed explanation of other models in the background, not discussion, and then this line can be removed from this paragraph.

- Done.

Figure 4: this is a very helpful figure that I referred to many times when reading this!

- Thank you for the kind words!

Table 3: I think presenting model run parameters based on figure # is confusing. It would be more straightforward to include a table that shows the parameter ranges for each of the four series of model runs.

- Agreed. I've updated the table to remove reference to figures to 'see parameter ranges', and instead put ranges in the table itself (438). I've also redesigned the table entirely as per RC3's suggestions.

Lines 472-474: Interesting! Is there any evidence for this in modern systems, that avulsion nodes cluster immediately downstream of previous avulsion nodes? This would be an interesting point to expand on in the discussion.

- This is an interesting suggestion, but we know of no way to test this idea with modern data.

Figure 12: Make dashed lines have a bigger dash, they are hard to distinguish from solid lines. Also, typo in the first part of the caption – missing "on" before (a).

- Thank you for the catch; the dashing was supposed to be the same form as in Figures 10 and 11 but I missed it. Updated, as is the typo in the caption.

Line 592: Abandoned channels impact the compensation rule in the Chamberlin and Hajek models because they leave an elevated abandoned alluvial ridge, so they cause repulsion away from the abandoned channels in the compensational rules.

- Thanks for the clarification! Added to the sentence, which is now in section 2 (75-78), and clarified that it is moreso the role of upstream flow routing that isn't necessarily resolved.

Lines 596-598: The clustered mode in the Chamberlin and Hajek (2015) model randomly selects a channel location within the clustering zone; it only selects the lowest location when an elevation threshold above the far-field floodplain has been reached.

- Ahh, my misunderstanding. I removed the erroneous connection. Thank you.

Paragraph beginning at line 611: I think this would be better suited as background material.

- Done (see response to “Re-organization of introduction and background”)

Lines 637-647: This material would be helpful for justifying the healing rules used, and thus could be presented earlier in the paper.

- Done (see response to “Re-organization of introduction and background”)

Section 5.3: These are interesting predictions focusing on the proximal-distal location of avulsion nodes! I think adding predictions about channel healing & lobe-switching behavior would be very helpful too. Are there characteristic types of fans that show lobe switching? Is there something about the floodplain aggradation style on those fans that influences abandoned channel healing? That would be really cool to explore.

- We have added some additional analysis on lobe switching to section 5.5 (614-637), and additional discussion to section 6.3 (690-714). We agree that this is an exciting research direction to pursue in the future, and have tried to add predictions and guidance that could help precipitate or facilitate this work.