Answer to A. Piliouras

March 20, 2017

Overview: The authors report on physical experiments of alluvial fans with a bimodal grain size mixture, relating the grain size transition to a slope transition and comparing their experimental results to those predicted by threshold-channel theory. They conclude that the fans in their experiments grew in a self-similar manner, such that the fans maintained a consistent geometry and their growth could be described by a simple mass balance. However, the fan slope was significantly higher than that predicted by threshold channel theory, and the authors do not really provide a convincing argument for why this might be so. In general I find the paper to be well-written and wonderfully concise, although I do think some elaboration is required on the points outlined below. I recommend this paper for publication pending the following minor revisions.

Major comments:

The introduction is somewhat lacking. First, it would be helpful to relate the concepts discussed both in the intro and the present experiments to the natural environment and studies of natural fans. Second, the experiments need to be placed in a broader context to highlight why they are significant and how they advance our knowledge of alluvial fan dynamics and/or stratigraphy. This should also be revisited in the conclusions.

We added more references about fan morphology in the introduction (page 2, lines 11-13, and 19-20, and page 3, lines 24-25). We also discussed the applicability of our experiments in the conclusion (page 18, lines 14-23).

Your results and conclusions would be stronger by including discussion of all experiments, not just Run 2. Some of your figures seem to have other experimental data in them, but since the paper never discusses anything other than Run 2, there is somewhat of a disconnect between the text and the figures.

Run 2 was used to illustrate our method and results, but we analyzed all runs. We clarified this throughout the revised manuscript.

The discussion needs a paragraph on limitations of the experiments, particularly in their applicability to natural systems. You state in the Appendix that you did experiments with laminar flow. What, if anything, does this imply for your ability to relate these experiments to nature? How might the dynamics, geometries, and/or stratigraphies of fans created with different flow conditions differ, if at all? I do not mean to imply that you need a full discussion of hydraulic scaling (you don't), but I think that a few sentences discussing your limitations and applicability will make non-experimentalists more receptive to your ideas.

We use laminar flows only to calibrate the transport laws, and measure the critical Shields parameters of silica and coal, we clarified this in the revised manuscript (page 6, lines 24-55, and page 19, lines 17-20). Nevertheless, we acknowledge that our results do not directly apply to natural systems, and we added a paragraph on limitation of the experiments in the conclusion (page 18, lines 14-23).

Regarding the lack of correlation between Qs and slope that comes out of the threshold channel theory analysis. Could this be because the flow is not always channelized and the deposit is not entirely formed by channels? You state in the first paragraph of section 6 that the deposit should have the same slope as the channels, but I'm not sure that this is true. I would guess that many alluvial fan and fan delta experiments, particularly thinking about those of Reitz and Jerolmack, are built by a combination of channelized and overbank or sheet flows. In that case, the overall deposit slope does not necessarily reflect the slope of a channelized flow, but perhaps that of some combination of processes. If your fan is partially formed by sheet flow or overbank deposits and not entirely formed by channels, which I suspect is likely true, then can you comment on the applicability of threshold *channel theory in trying to describe a deposit that is not and should not be the same as the bed of a channel? This may explain why the slope of the fan is quite a bit higher than the predicted threshold channel slope.*

When looking at our experiments, we could see the grains moving only in channels (page 6, lines 20-24). From this observation, we hypothesize that the sediment is mainly deposited within the channels or on the banks. The primary transport mode is bedload, and overbank deposition only occurs transiently during avulsions. Moreover, we do not show terraces on the DEM. Thus, although we don't know how overbank flows may affect the slope of the deposit, we suspect their effect to be minor. We clarified this page 13, lines 19-20.

Minor comments:

Page 2

Line 11: Consider adding "alluvial fans can be easily produced and boundary conditions can be easily controlled."

Done.

Line 14: Consider providing some examples of "the deposit responds by adjusting its morphology."

We added a reference to the work of Muto and Steel (2004) in the manuscript (page 2, lines 20-21) as an example of this adjustment.

Line 25: Replace "At variance" with "In contrast".

Done.

Lines 25-27: You first state that Guerit et al., 2014 proposes that Qw, Qs, and grain size act independently to influence slope, but then claim that they conclude that slope depends on Qw and grain size. These sentences are in conflict and the language either needs to be adjusted to resolve it or you need to better explain the results of their study and in what ways, specifically, Qw and grain size influence slope.

Guerit et al. (2014) used uniform sediment, and did not explore the influence of grain size but focused on the respective influence of the water and sediment discharges. They found that, for a given grain size, Q_w is the first-order control on the slope, to which Q_s adds only a perturbation. We clarified in the revised manuscript (page 3, lines 12-17).

Line 29: Omit the comma after "moderately".

Done.

Page 3

Line 10: Include more references of experimental alluvial fans.

Done.

Line 18: Omit erroneous s in "each type of grain".

Done.

Lines 19-20: "The shear stress required to move large grains in a mixture is lower than it would be in a system of uniform large grains because the grains protrude more into the fluid."

Done.

Line 21: "a higher shear stress in a bimodal mixture because they are partially shielded from the flow by neighboring large grains".

Done.

Line 27: "Below a critical shear stress".

Done.

Page 4

Line 3: Reference your experimental setup figure here.

Done.

Also consider, rearranging Section 2 to start with this information regarding your setup and procedure. This will allow you to start broader and then narrow down to the details, which will be make it read a bit more easily.

Done.

Lines 4-5: Rearrange these clauses/sentences to this order: "At the back of the tank,. . . which the fan leans. At the wall's foot, . . . concentrating along it. The three other sides . . . evacuate water."

Done.

Line 6: Does the standing water, however shallow, influence anything about the fan's growth or toe geometry?

Standing water induces the formation of a fan delta (Powell et al., 2012). Yet, as the immersed part of the fan does not represent more than 1% of the total fan volume we can neglect it in the mass balance, and therefore in our analysis. We clarified that in the revised manuscript page 4, lines 10-11, and page 5, lines 1-2.

Line 7: Is your header tank a constant head tank? If so, say so.

Yes it is a constant-head tank. We clarified the text.

Line 11: "reaches its bottom" is vague. What is "its" referring to here? The tank? Where is the bottom? Rephrase.

Its refers to the tank bottom. We rephrased it (page 6, lines 18-19).

Line 13: Why did your experiments have five or six channels at a time? This seems in contrast to many other fan experiments, particularly with those of Reitz and Jerolmack. What are the possible causes and implications of this?

Hartley et al. (2010) have shown that alluvial fans can display a radial distributive pattern where all channels are active at the same time. We suspect that the multiple channels are due to sediment discharge (Stebbings, 1963; Métivier et al., 2016). In the experiments of Reitz and Jerolmack (2012), channels are not active simultaneously but they define a radial distributive pattern of 4 to 5 channels. We clarified this point page 6, lines 20-22.

Line 19: Mustn't you also have silica deposited overbank to make the deposit shape depicted, or is silica really that narrowly deposited in the thalweg? In that case, if silica exists over much of the proximal deposit, then do the channels migrate to visit almost every point on the proximal fan in order to get that distribution or silica?

Looking at our experiments, we observe that silica is indeed deposited in the thalweg. Avulsion process, however, allows the channel to visit every point of the fan. We clarified this in the manuscript page 6, lines 22-24.

Line 24: I suggest changing the language of "eye-averaging," as it does not make your observation convincing.

Done, page 7, line 4, and page 8, line 1.

Line 28: You need an extra sentence or two here to explain your image processing methods, particularly in your rescaling/stretching and error/accuracy estimates.

We added an extra sentence to explain our image procedure (page 8, line 3-5).

Line 31: Why are you only reporting on Run 2?

We analyzed all the runs and used run 2 to illustrate our method and results. We clarified this throughout the revised manuscript.

Line 33: Again, your measurements of accuracy are unclear. Is the 19% a standard deviation? A variance? Some roughness measurement?

The 19% value corresponds to a standard deviation, we added this precision in the revised manuscript (page 8, line 10).

Page 5

Line 15: Reword to state that either your precision is better than 1mm or your error is less than 1mm.

Done.

Line 17: "This property suggests that we can compute".

Done.

Line 18: "profile of the fan with minimal error" or "with minimal loss of information".

Done.

Line 22: "We find that the slope plateaus to a value of about 0.29 near the apex and to about 0.10 near the toe."

Done.

Line 23: "transition between these slopes is smooth,"

Done.

Lines 23-24: Why are you calling this a "characteristic length?" You are only examining one experiment, or does this hold for more experiments? Please clarify.

The length of the transition is the same for all runs. We clarified it in the revised manuscript (page 10, lines 19-20).

Line 24: "55% of the fan length from the apex".

Done.

Line 26: Here you restate $R \approx 0.62$*, which closely coincides with 0.55. This would be even more convincing by stating R with the error you already have* $R = 0.62 \pm 0.04$*. Also, do you have an error on the inflection point distance 0.55? Should be stated here, if so.*

Done.

Line 28: Replace "cohesive" with "intact" to avoid confusion surrounding cohesive sediment.

Done.

Line 30: “whereas coal concentrates at the fan toe.”

Done.

Line 31: Replace “smeared” with “irregular” or “gradual” or “fluctuating” etc.

Done.

Line 31: ”It” is vague. Rephrase to “The transitional zone shows alternating layers”.

Done

Page 6

Line 4: Insert equals signs for slope: (slope = 0.29), (slope = 0.10).

Done.

Lines 5-6: “Finally we define the transition line, which joins this intersection to the origin and passes through the alternating stratigraphic layers in the transition zone.”

Done.

Lines 6-7: “more mobile sediment (coal) lying below the less mobile one (silica).

Done.

Line 7: Replace “steady climb” with “upward migration”.

Done.

Equation 3: Define phi in text.

Done.

Page 7

Line 11: How and why do you “adjust” the proximal and distal slopes?

We use a linear fit to quantify the slopes of the deposit. We explained this in the revised manuscript (page 13, lines 10-11).

Line 13: Your calculated silica fraction in the deposit matches that put in during experiments. Do you account for porosity in the deposit since your input flux is likely just a mass or solids volume flux? The porosities of the coal and silica are likely different, and I would expect this to influence the overall deposit volume and volume partitioning between coal and silica.

Following your comment, we measured the porosity of our granular materials and estimated the porosity of the deposit. Within uncertainties, they are the same. We added a paragraph and Fig.(5) to explain these measurements in the revised manuscript (page 8, lines 15-29).

Line 21: Replace “type of sediment they flow onto” with “bed sediment composition”.

Done.

Line 30: Replace “ramify” with “bifurcate”.

Done.

Line 31: "threshold-channel theory slope predictions."

Done.

Page 8

Line 5: "cross sections per channel per measurement strip".

Done.

Line 5: "channels and their widths in each bin over the runs."

Done.

Line 6: "distance from the apex".

Done.

Line 26: "we approximate Cf with".

Done.

Equation 10: Define all variables in text.

We have carefully checked that all variables are defined in the text of our manuscript. In addition, we added a Notation section in the appendix where all variables used are defined.

Page 9

Lines 5-13: You provide a few possible explanations for departing from theory, but you need more discussion to provide a physical reasoning for why you think this is.

Guerit et al. (2014) modeled the influence of sediment discharge on the fan profile, and showed that the resulting fan slope is steeper than the threshold slope. Experiments are under way in our group to test this hypothesis. We modified the text to include this discussion page 16, lines 27-33, and page 17, lines 1-2.

Line 18: This section ends fairly abruptly.

We agree, we edited the revised manuscript (page 17, lines 8-10).

Lines 29-30: How does this straightforwardly extend to different grain size distributions?

Indeed, it might not be so straightforward. Field observations, however, are encouraging: some have shown a strong correlation between changes in slope and grain size or sand fraction (Bull, 1964; Blair, 1987; Blair and McPherson, 2009; Miller et al., 2014; Stock et al., 2008). We mentioned this in the revised manuscript (page 18, lines 4-10).

Page 10

Line 7: "both mechanisms" this is vague. Which mechanisms?

We clarified this in the revised manuscript (page 18 , lines 28-32).

Line 8: omit comma after "deposit".

Done.

Appendix A

Does threshold channel theory hold for laminar flow? Either a brief statement of affirmation or a brief discussion on any assumptions on this front is required.

Seizilles et al. (2013) have shown that the threshold theory indeed applies to laminar flows, but we only use laminar flows to measure the critical Shields parameter anyway. We clarified this in the revised manuscript (page 19, line 17-20).

Figure 1 Assign (a) and (b) to parts of figure. On your schematic, the text says there is also a trench at the downstream or rightmost edge, but it is not depicted here.

Done.

Figure 2 (a) This graph is somewhat confusing (particularly the vertical axis), as it is not the typical way that people in our community show grain size distributions, although I acknowledge it is mathematically accurate. Consider replotting as a "percent finer than."

Done.

Figure 3 (b) Label Rc, Rs.

The picture is rescaled there is no Rc and Rs but $\mathcal{R} = \dfrac{R_s}{R_c}$ and 1.

"The 26 pictures are each 10-minutes apart."

Done.

Figure 5 "only two sample radii 5 degrees apart".

Done.

Table 3 Are these the characteristics at the end of each run? If so, say so. Run 5 has a drastically different R than all other experiments and much higher error. Why? It is still unclear why you only discuss Run 2 in the paper.

These are characteristics at the end of each run, Run 5 has a drastically different $\mathcal{R}$ because it involves a mixture of 80% of silica. As a consequence, the silica-coal transition is more distal than in the other experiments. However, on Fig. 10, this run does not appears as an outlier. The error is due to fluctuations of the silica-coal transition. We clarified this it the revised manuscript (Table 3).

Figure 10 Consider rephrasing measurement "bins" rather than strips. Also applies to text.

Done.

Figure 11 The number of channels appears to decrease past the transition zone, but you claim that channels do not rejoin downstream. So do they just lose definition and you cannot detect them? This needs to be clarified.

Yes, they are more difficult to detect in the distal part of the fan, due to the poor contrast. We clarified this in the manuscript (page 16, lines 4-5).

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