Answer to reviewer 1

March 20, 2017

I think this is a good paper and would recommend it for publication with minor revisions.The manuscript could really use more background on field observations of alluvial fans, particularly the threshold versus transport theories of fan slope, and a discussion on how well the experiment results reflect and can be applied to real-world observations. There were a number of errors in grammar and general sentence structure. I note a few of these in the technical corrections, but the paper could use a read-through and edit by one of the native English-speaking authors.

[1] *Alluvial fans often have a single main channel, rather than many radiating from the apex. The experiments of Reitz and Jerolmack (2012) behaved similarly to real fans, with multiple channels occurring only briefly during avulsions. The experiments for this study never had fewer than 4 channels. Why is this, and how are the results applicable to real alluvial fans if they differ in this regard?*

Some alluvial fans display a radial distributive pattern where all channels are active at the same time (Hartley et al., 2010). We suspect that the spread into multiple channels is 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 19-21.

[2] *Stock et al (2008) report a similar distance between the proximal and distal fan, but that median grain size of gravel deposits remained constant for the upper 70% of the fan. Some discussion of this would be useful.*

In our experiments, the position of the transition, hence the change in slope, depends on the proportion of silica and coal in the mixture. This accords with the observations of Stock et al. (2008), on four alluvial fans of the Mojave Desert in California, where the slope and the gravel fraction decrease with distance from fan head. We included this observation in the conclusion on page 18, lines 9-10.

[3] *How does the 32% length for slope transition compare with real-world fans? A couple possible sources are a databases of alluvial fans: Saito and Oguchi (2005) for humid fans, and perhaps reviews by Blissenbach (1954), Anstey (1965), or Hooke (1968) for arid fans.*

The length of the transition is constant in all of our experiments. To our knowledge, only Miller et al. (2014), studied this transition in the field. They showed that it is proportional to the fan length. We edited the text to include this reference page 10, lines 20-21.

[4] *You make a few references to "run 2", and it gave me the impression that you only did your analyses for that single run. Assuming you mean to say that you are using run 2 for your figures as an example, I suggest adjusting the text to make this clear (if you did only do analyses for run 2, please explain why).*

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

[5] *Was all of the material transported as bedload, or was some portion able to transport as suspended load? Did material deposit outside of the main channel? In the distal sections (the coal only section) of the fan was flow channelized? A shift from dominantly channelized flow to dominantly overbank flow downstream might affect your assessment of fan slope being controlled by the sediment grain size. Reitz and Jerolmack (2012) report extensive overbank flow during avulsions on their experimental fans, and similar behavior has been noted for fans based on field observations (e.g. Field 2001 "Channel avulsion on alluvial fans in southern Arizona"), did your fans feature similar behavior?*

In our experiments, bedload is the dominant transport mode. Only a small amount of fine coal is transported as suspended load, and overbank flow occurs temporarily during avulsions. Neither process seems to control the fan morphology, although we cannot be positive about that. We mentioned this in the revised manuscript page 6, lines 22-24.

[6] *In the conclusion you note that you can estimate the sediment flux that fed the fan. While this may be true for your experimental fans, the grain size distribution and flux of sediment feeding alluvial fans is essentially never constant, so when examining alluvial fan surfaces we are only really understanding the depositional processes responsible for constructing the upper few meters of the fan. In addition, fan surfaces can be reworked, masking the formative process (de Haas et al, 2014). Some discussion of this and a description of how well your results can be applied to alluvial fans in the field would be very helpful.*

Multiple processes can alter the fan surface and add to the primary mechanisms that control its growth (de Haas et al., 2014). Our simple experiments concentrate on basic processes. Therefore we agree that our results cannot be transposed directly to natural systems. We clarified this page 18, lines 19-23.

[7] *The many sections of the paper seem a bit convoluted. The flow of the paper would be better sections 3-6 were merged into something like "experimental setup", "model runs", and "math analyses (or something)". As it is now the division of sections 3 and 4 (as well as 5 and 6), seem a bit arbitrary, and the lines at the end of each section offering a preview of the next section are awkward. I would also suggest adding a "Notation" section as a reference for the different variables used in your equations.*

We added a Notation section in the appendix. We also removed excessive sections (3 and 4 are now merged, as well as 5 and 6). We agree that the paper could follow the plan you propose (set-up, observations and theory). However, we prefer to introduce the hypotheses upon which the theory is built (radial symmetry, self-similarity, etc.) one after the other, when our observations support them. We have significantly edited the manuscript to clarify its structure. We hope this new version is easier to read.

Line by line comments and some technical corrections (page.line):

1.14: I think the reference here is supposed to be "Blair and McPherson (1994)-Alluvial Fan Processes and Forms". There is a new version of this book chapter from 2009 (in book "Geomorphology of Desert Environments") (the ref list has anotherBlair/McPherson paper from 1994)

Done.

2.5: "Perfect cone" is only the case for purely debris flow fed fans. See Williams et al (2006) "Aspects of alluvial fan shape. . ." (Williams et al also report that fluvially-fed alluvial fan slope-distance profiles (e.g. your figure 6b) follow an exponential fit, rather than two distinct slopes with a transition zone).

We modified the text page 2, lines 7-9, to avoid the confusion you mention.

2.6: "Possible explanations for this curvature. . ." adding into this sentence that you are talking about the "transport" and "threshold" theories fan slope would clarify other parts of the paper where you refer to threshold theory.

Done.

2.21: ". . .no clear consensus. . ." some more detail/background on this would be helpful.

We clarified this in the revised manuscript, page 3, lines 10-12.

3.14-26: this paragraph was hard to follow. See a few examples below:

3.16: rephrase sentence to "When unmixed, we find that for the same shear stress τ, the flux of coal grains is larger than that of silica grains (Fig.2)"

Done.

3.19: rephrase sentence to: "The shear stress required to move large grains in the mixture is lower than it would be in a system of only large grains, because they protrude more into the fluid."

Done.

3.21 "larges grains" change to "large grains".

Done.

3.22: ". . .different densities. . ." what about different diameter grains? Does this have an effect?

Both grain size and density affect the mobility of our sediment. We measured the critical Shields parameter, and found a higher value for silica (small grains, large density), than for coal (large grains, low density). This indicates that the density contrast exerts a primary influence on mobility. This is confirmed by experimental observations (page 5, lines 13-15, and page 6, lines 1-6).

4.4: Is the "impervious wall" vertical?

Yes it is, we added it in the revised manuscript.

4.13: See comment above. Alluvial fans typically have a single channel emanating from the apex which splits further downstream. The avulsion process appears different than the experiments of Reitz and Jerolmack (2012).

Some alluvial fans display a radial distributive pattern where all channels are active at the same time (Hartley et al., 2010). 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 19-21.

4.19: "Coal is deposited on the banks": is coal ever deposited overbank?

Overbank deposits are relatively rare, and the sediment is deposited mostly in the thalweg or on the banks. We clarified this point in the manuscript (page 6, lines 22-24).

4.26: "during run 2". Did you only examine the boundary on run 2? Or do you mean to say that Fig 3a is of run 2? If the former, why not for other runs? 4.31: Same comment as for line 26.

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

5.19: When describing similarities between profiles, do you mean all radii for a single fan, or across fans for all experiments?

We mean all radii for a single fan. We have clarified this sentence in the manuscript (page 10, line 9).

5.25: See commend above (comparing the apparent match of sediment distribution change and slope transition with observations by Stock et al (2008))

See our answer to your second comment.

7.3: the font for the variables in equations 7 and 8 is different

We checked the font of the variables.

7.20: The sentence phrasing and grammar in this paragraph could use some editing.

We did our best to improve the English of the manuscript.

7.24: Some background on the "threshold" vs "transport" theories for alluvial fan morphology would be useful.

We added a discussion on the threshold theory page 13, lines 20-22, and page 14, lines 1-2, in order to provide this background.

8.33: Do you mean to say with "sediment discharge relative to water discharge" (i.e. sediment flux)?

Both are true for a given water discharge, we have clarified this point in the manuscript (page 16, line 20).

9.20: The Conclusions section could also use some edits to grammar and sentence structure.

We edited the conclusion of the revised manuscript.

9.32: References here ("...in our experiments")

Done

9.32-33: "As a consequence, we expect that the geometry of the final deposit (location and slope of the transition and proximal and distal slopes) allows us to estimate the relative flux that built the fan." This sentence seems like a big jump. The sediment supply to alluvial fans is not constant as it was in the experiments. Perhaps you mean to say the relative flux for the most recent fan deposits?

We agree. We have changed the text in the conclusion (page 18, lines 4-11).

Figures:

Table 2: replace $g/min-1$ *for sediment discharge with* $Lmin-1$ *(is silica fraction volume or mass...in eqn 3 it is volume) so that it uses the same units as Qw (and is easier to visualize V/V).*

Done.

Fig. 3: Would it be possible to adjust contrast on photos of the fan (e.g. fig 3) so that it is easier to discern the silica against the coal?

Unfortunately, the quality of the pictures does not allow us to improve the contrast much.

Fig 6/7: I suggest using the same horizontal scale for these two figs.

We used Fig 7 to introduce the rescaling with R_c. In the revised version, we have added the physical scale accords to your suggestion.

Fig 7: Specify which run this is from (presumably run 2?)

Done.

Figure 10: this figure could probably be merged with Fig 3.

We have tried merging them, but this obscured the resulting figure.

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