Answer to reviewer 3

March 20, 2017

General comment:

I recommend this article for publishing if the comments are considered. The article is well-written and it is easy to read. The first two sections of the article are quite detailed, leading to sections that lack of substantial contributions to the general discussion by themselves, as they are too brief and without much analysis. Nevertheless, the article shows good results that are worthy to publish in spite the simple analysis with some flaws.

Major comments:

The analysis is focused only in some properties of the particles used and it misses other parameters that may give an important insight to the results, i.e. repose angles. Such properties of the materials could be included in the qualitative analysis, as Reitz and Jerolmack (2012) do.

We quantified the repose angle of our grains. It is presented in Table 1 in term of the friction coefficient, μ which represent the tangent of this angle. This friction coefficient appears in the definition of the threshold slope (eq. 11). We clarified it in the revised manuscript (caption of Table 1).

It is mentioned that the exposure/hiding effects are negligible because of the density difference between the particles. This is only if the sample is well-graded. There is insufficient information provided in order to neglect, or not, such effects. Also, mobility is accounted separately, for each material, so it seems irrelevant if in the final experiments are mixed, since the mobility may be affected by the other material sizes, not only by density. Even if you are able to provide evidence that it is in fact negligible, a re-writing of the paragraph could be helpful.

We agree, there exist no universal transport law accounting for the hiding/exposure effect. However, to estimate, at least qualitatively, the differential mobility of our grain we use transport laws of each species and neglect the exposure and hiding effect. We re-wrote the paragraph (page 6, lines 9-17).

There is a chapter called "Mass Balance". If you look at the equations, they are all in terms of volume. What about the packing conditions of the fan? And the packing conditions of the inlet? Why would those be the same? Is there a way to quantify the void between particles? I think that at least some assumptions should be made and explained.

Following your comment, we measured the porosity of our granular materials, and estimated the porosity of the deposit. We found that the porosity is constant, and, therefore, does not impact the mass balance. We added a paragraph and figure 5, to explain this in the revised manuscript (page 8, lines 15-29).

I found interesting the geometrical self-similarity shown in the article, but a quite more complex self-similar behavior is there. Certainly, with the results something else could be done.

We agree that this preliminary work only focuses on the first-order geometry of the deposit. To further the analysis of this self-similarity, we need to understand the physical origin of the fan's slope. For this, we need to improve the geometry of the feeding channel. An experiment is under way in our laboratory. We clarified this in the conclusion, page 18, lines 19-21.

A similar pattern to the one you show when cutting the fan radially, has been obtained by other authors in a 'quasi-two-dimensional' cell, e.g. Makse et al. (1997). It could be interesting to say something about that.

Makse et al. (1997) obtained stratification when the large particles' angle of repose was larger than the small one's. That is verified for Fig. 7, but what about the rest? Its quite interesting that the vertical cross section is not only segregated, but stratified. Could this be found in natural fans? If so, under which conditions? Since you performed experiments with silica volume concentrations ranging from 25% to 80%, maybe stratification depends of this parameter.

The cross section of figure 7 shows segregated deposits separated by a mixing zone with alternating layers of silica and coal. The extent of this mixing zone (30 % of the fan length) seems to be independent of the composition of the sediment mixture. Similar stratigraphic patterns are observed in natural fan deposits, and they are interpreted as a consequence of fluctuating water and sediment discharges (Paola et al., 1992; Clevis et al., 2003; Charreau et al., 2009; Whittaker et al., 2011; Dubille and Lavé, 2015). We were not aware of the experiments and model of Makse et al. (1997b,a), that indeed exhibit a similar pattern. We refer to them in the revised manuscript (page 10, lines 7-11).

The above leads me to another comment. It seems that you only analyzed experiment 2. What about the rest?

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

In general for a roughly 10 pages article, 6 sections is too much I think. If some sections are merged or taken as subsections it would give more significance to each section. As it is, seems that each section has nothing much to say, e.g. sections 4 and 5.

Following your comment, we merged sections 3 and 4, and sections 5 and 6.

Minor comments:

p3.line1: It it confusing the way you say that large grains are in the upper part and small ones deposit near its toe, as figure 2 shows the opposite. The system inverses the gradation?

The sentence you refer to describes the experiment of Reitz and Jerolmack (2012). In their experiments the mobility difference is driven by different grain size. In our study the mobility difference is controlled essentially by the density contrast. We clarified this in the revised manuscript (page 5, lines 14-15, and page 6, lines 1-6).

p3.line10: Routine seems something tedious, ordinary and repetitive, that has nothing special, therefore irrelevant. Another word could be better to start the chapter.

We changed routine to common.

p4.line1: Again the density. If the density difference prevails over grain size, then how is explained that mobility has nothing to do with density? If so, which difference is more relevant? Could be there an equilibrium?

We quantify the grain mobility in term of the critical Shields parameter. This parameter depends on shear stress, grain size, and density. Coal grains are larger and lighter than the silica grains. The value of the critical Shield parameter is lower for coal, which suggests that the density difference prevails over the grain size difference. Experimental observations confirm this (page 5, lines 14-15, and page 6, lines 1-6).

p4.line13: The number of channels is different from the number reported by Reitz and Jerolmack (2012). Is there a reason?

We suspect that, in our experiment, the presence of multiple channels is due to the 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 comparable, radial, distributive pattern of 4 to 5 channels. We clarified this point page 6, lines 20-22.

p4.line20: Silica proportion is introduced, is it of volume or weight? If such variable is introduced, maybe you could use a formula.

Silica proportion is introduced by volume. Following your comment, we added formulas to clarify this (page 8, lines 12-22).

p4.line24: To put explicitly eye-average, indicates subjectivity as results may change by repeating the analysis. The error by this process is considered?

We modified the sentence to avoid misinterpretation (page 7, line 4, and page 8, line 1).

p5.line5: "The observations confirm the scaling, thus..." Instead.

Done.

p5.line24: 32% and 55% of the fan length, Which fan length? Is is the average of all the experiments? Of each experiment?

We mean the percentage of the total fan length, from apex to toe. These values are for run 2. Values for the other runs are presented in Table 3. The value of the transition length is similar for all runs. We clarified the manuscript to be more specific about this (page 10, lines 17-21).

p5.line34: You say that the variability of sand-coal transition in the stratigraphy is because of channel avulsion. If you follow one of the major comments, then it is not because of that.

There are two main differences between our experiments and the experiments of Makse et al. (1997b). First, the slope of the deposit is much smaller than the angle of repose due to fluid entrainment. Second our experiment is three-dimensional whereas the experiment of Makse et al. (1997b) is two-dimensional. Furthermore figure 3 shows that the limit between the two fans is strongly dissected. Because sediments are deposited mostly within channels, we believe this pattern is the result of channel avulsion. We added a discussion in the revised manuscript page 11, lines 7-11.

p6.line9: The interpretation in the context of self-similar growth should be in the self-similar growth section.

Following your comment, we restructured our manuscript.

p8.line26: Why Chézy and that value? I understand that it is for simplicity, but still.

The Chézy coefficient is the simplest possible coefficient use in fluvial experiments to quantify fluid friction (Métivier et al., 2016). Its value depends on the channel shape and grain size. We chose the value of C_f according to Chow (1959).

p8.line27: The reference is wrong, it should be (Chow, 1959).

We corrected it.

p9.line20: Independently you consider the stratification analysis, you should say if deposits show stratification.

We added a sentence about stratification in the revised manuscript (page 17, lines 16-17).

p9.line21: If you make reference to your sediments in terms of mobility it is tricky as it is true all the time and it could include both silica and coal particles. Too obvious.

We clarified this in the revised paper.

p9.line30: It is not clear that it is possible to extend to a continuous size distribution. You did not say anything about the sample, where they well-graded?

We do not know whether the segregation mechanism could be extend to a continuous size distribution. But field observations 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). As a consequence, although this should be tested experimentally, we expect that the segregation occurs similarly for a continuous grain size distribution. We mention this in the revised manuscript (page 18, lines 4-11). Our sample are not well-graded, but they have distinct mobilities.

p10.line2-3: The discussion about the most challenging problem is not fair enough. You lack of evidence or references to sustain that.

We re-wrote the conclusion to clarify.

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