Review of Manuscript entitled "*Erosional response of granular material in landscape models*" by R. Reintano et al.

The manuscript from R. Reintano *et al.*, exposes an experimental modelling approach that aims at investigating the influence of material composition on the style of landforms (mostly drainage systems) when the material is submitted to rainfall erosion. This works goes a step beyond than an early work (Graveleau *et al.*, 2011) that first published a mixture of granular materials suitable to be deformed under a compressive device (and actually, also in extensive and strike-slip settings) and eroded below an artificial rainfall device. In their manuscript, Reintano *et al.*, do no deform their experimental materials, but they carefully analyzed the patterns of erosional landforms in their set of experiments. Technically, their device reproduces roughly a classical boundary conditions in terms of slope (15°) and precipitation rate (25-30 mm/h), but a significant improvement is the accurate topographic monitoring of the model evolution, and the use of such topographic dataset in terms of erosion law parameters. In total, the authors tested 6 different materials : 1 is a raw material (i.e., 100% Silica Powder), 4 are mixtures of Silica Powder + Glass microbeads and PVC, and a last one tested crushed quartz.

The results from Reintano et al.'s concern first the characterization of the properties of the different components. It is basically the geometrical properties of grains, their chemical composition, their frictional properties (both at peak and stable friction conditions), porosity and permeability. All these properties have been measured with robust devices and repeated several times. Results are in good agreement with already published values, which allows to address later in the discussion the origin of erosional behaviors observed for each materials.

Then concerning model results, the experimental device designed by the authors allows to carefully explore the morphometry and erosion laws observed for their experimental landforms and to investigate the control played by material composition. Particularly, DEM analysis allowed to confront experimental results to Hack's and Flint's law, together with quantifying precisely sediment outfluxes. Erosion law parameters are quantified and compared to nature.

In addition, through the analysis of dimensionless parameters (for time notably) extracted from erosion laws, this work provides a novel quantitative understanding to address scaling issues and discussions about the similarity of experimental landforms to natural counterparts. Time scaling is particularly investigated. It is addressed in another way than in Graveleau et al. (2011) publication but provides similar values. As a whole, the results obtained in this manuscript confirms that Silica Powder is a central component required to develop highly interesting and powerful experimental landforms. Several mixture composition appear suitable to develop morphotectonic experiments.

In conclusion, this manuscript is a real valuable contribution that allow to goes a step forward in the demonstration that morphotectonic experiments should be considered as a useful tool to investigate active tectonics and relief dynamics questions. My comments and suggestions of corrections are really minor (see below). That's why I consider that this work definitely deserves publication at EGU Earth Surface Dynamics journal and ask for **minor revision**.

With my best regards and all my congratulations to the authors,

Fabien Graveleau
Université de Lille

Below are detailed comments on each section of the manuscript.
Suggestions are written in blue and some corrections appear in red.

TITLE :

OK, it fits well the content of the paper.

ABSTRACT

OK, it summarize well the content of the research.

1 - INTRODUCTION

L50-51 : Add one reference about S. Ouchi's work, for instance Geomorphology 2011 or 2015 ;

Ouchi, S. (2011). "Developement of experimental landforms by rainfall erosion and uplift." The Journal of the Geological Society of Japan 117(3): 163-171.
Ouchi, S. (2015). "Experimental landform development by rainfall erosion with uplift at various rates." Geomorphology 238(Supplement C): 68-77.

L52 : Add references by Guerit et al., 2016,

Guerit, L., S. Dominguez, J. Malavieille and S. Castelltort (2016). "Deformation of an experimental drainage network in oblique collision." Tectonophysics **693, Part B**: 210-222.

L57 : Cite here the reference of Bonnet & Crave (2006) since they tested (Fig.2) the effect of different mixtures of silica powder and glass beads on morphology.

2 – EXPERIMENTAL APPROACH

L73 : I would cite here Lohrmann et al., JSG, 2003.

Lohrmann, J., N. Kukowski, J. Adam and O. Oncken (2003). "The impact of analogue material properties on the geometry, kinematics, and dynamics of convergent sand wedges." Journal of Structural Geology 25(10): 1691-1711.

P4 Table 2: Regarding the accuracy of measurements, I am always questioned about the usefulness (and significance) to provide values with what appears to me as (over)accurate.
For instance, is this really significant to provide density measurement at a 1kg/m3 accuracy ? I know that devices like the one used in this work (helium pycnometer) can account for such accuracy, but is it that significant for granular materials for which handling techniques is so important in controlling the grain packing and therefore density ? To me, I would limit the accuracy of density to 10kg/m3.
This is the same for the accuracy of frictional properties. In the literature, ring shear apparatus are able to provide really accurate estimations of cohesion and frictional angle. Is it worth since granular material are sensitive to handling techniques and compaction ? In the manuscript, concerning frictional values, the authors remain in the order of magnitude of 0.1 kPa for cohesion, which is fine for me.

Density seems to correspond to "particle" (or specify) density ; that is the density of grain. So why specifying "dry" in table 1 ? It is also surprising to have such density values of grains for mixtures.
Wouln't be worth to document the "apparent" or "bulk" density of the whole material ? That is the density of the material including grain and air.

L124 : Eq. 7 is quoted but it might be Eq.1.

L127 : *"In this work the normal stress applied are in the range 25-200 kPa".*
This is normal stress applied in the Casagrande direct shear box. What about the range of normal stress in the erosional experiments ? Since the box is 5 cm deep, it should be in a range lower than the range of values applied by the Casagrande box. Would it be possible to add a few comment on this point ?

Note : In Graveleau et al (2011) we did test in a Casagrande Direct shear box but we finally decided to disregard this apparatus (and build a proper one) because the range of normal stress Casagrande box requires / could applied to the tested material was largely over the range of normal stress actually acting inside the morpho-tectonic box. As pointed later by Reintano et al., it is true that it entails that the failure envelope is steeper at such low normal stress, but we presume that it was more in the range of what is actually occurring in our models.

L134 : "φ" is mentioned in the text but it is "Φ" is Table 2. Please harmonize.

L139 : "*while the <u>mechanical properties</u> seem to show a common trend*."
What do you mean by "mechanical properties" ? Not clear.

How many tests have been made per materials to obtain the average frictional properties ?

L141 : Is porosity calculated for dry materials of water-saturated materials ? I presume it is dry…
Because they are not the same between dry and wet conditions and differences can be slight or large depending on materials (Graveleau, PhD, 2008 ; p298). This is notably the case for Silica powder where compaction is apparently more efficient when the material is wet than dry (it is at least what we observed for the Silica Powder used in the device in Montpellier).

L148 : Value of porosity for GM (0.26) is remarkably low compared for instance with Graveleau et al. (2011) who obtained 0.36, although D50 are not that different (88 µm for Graveleau et al., 98 µm for Reintano et al.). Any explanation of this ?

L157 : "*The permeability values for mixes are then in the order of 10-13 m^2*."
The averaging comment is appreciated, because, as a recall to what has been written above, proposing (in table 2) values of permeability with a 0.01 accuracy might be excessive. To me, the order of magnitude need to be conserved ; at least, 0.1 accuracy should be enough.

L173 : Eq. 10 is quoted but it might be Eq. 4.
L175 : Eq. 10 and 11 are quoted but it might be Eq. 4 and 5.

L181-182 : *"For the erosional behavior of the composite material, the ratio between precipitation rate and infiltration capacity appears to be the main factor controlling the geomorphological response."*
Yes, definitely. This recalls me a figure in my PhD (Fig.V.I p.435). It is good to mention this point and explain it here.

L214 : "*Erosion and sediment discharge are computed with ad-hoc MATLAB algorithms*." Any reference ?

L230-248 : I have much appreciated this temporal scaling analysis. It would be worth to discuss and compare a bit the results obtained here analytically by the authors (1 min ⬄ 3 800 – 38 000 years) to what Graveleau et al. (2011) published (1s ⬄ 100-300 years so 1 min ⬄ 6 000 – 18 000 years) in a tectonically quiescent context or others (Strak et al., 2011) published in a tectonically active extensional setting (1s ⬄ 65-375 years so 1 min ⬄ 3 900 – 22 500 years). Which is remarkably in the same range !

3 – RESULTS

L276-277 : "*The planar surfaces developing close to the lowermost side of the experiment have a slope of about 12°"*
This angle could be rattached to a threshold angle for detachment as Lague et al. (2003) or Graveleau et al (2011) mentioned. Comments could be welcome. Compare to Graveleau et al.'s values for MatIV (8°) which is significantly lower. Why ?

L281-282 : "*The planar surfaces have a slope of 13° and 15° for the lower and upper surface, respectively*".
Idem. Comments for the lower slope. Suggestion for the reasons why it is higher than form CM2 ?

L289-290 : "*The planar surfaces that form at the end of the experiment have a slope between 9° and 10°,…"*
For SM1, this value of the lower angle is the same as Graveleau et al. (2011- for SilPwd (10-10.3°). So OK.

L303 : "*As a knickpoint separating…"*
Please, locate knickpoints by an arrow on Fig 7 to be sure to look at the good place along the profile.

L307-308 : "*we can observe the propagation of the erosion wave from the bottom to the top*."
Please locate the "erosion wave" on the figure.

L313-315 : "*Sediment discharge plotted over time shows always two main phases (Fig. 8): phase I, fast removal of material from the model; phase II, slower removal of material with a lower discharge rate that is kept constant until the end of the experiment."*
A parallel with Fig10 in Graveleau et al (2011) obtained differently (by weighing output sediments) can be made. The shapes of the curves are the same. It is interesting to observe that the same shape is obtained with two different techniques.

L320-321 : " *while phase II is 6 g.min*$^{-1}$*…*"

Surprisingly, it is about twice larger than the value Graveleau et al. (2011) obtained (2.8g/min) for MatIV at a 15° slope (cf figure 10 in their paper). Any idea why it is twice higher ? Maybe this comment could come in the discussion (about L375-379)

L323 : *"…is linked in time to the stationary conditions in the morphological evolution of the experiment."*
This sentence is not totally clear. Please reformulate to be sure the audience will understand.

L327 " … that decrease**s**…"

L335-336 : "*The erosion and incision are light reach a depth lower than 2/2.5 cm.*"
Sentence not clear.

DISCUSSION

L341 : "*Respect to other works on the same topic …"*
Add reference from Bonnet & Crave, 2006.

L364-367 : "*The authors settled the tests at lower normal stresses than our measurements (< 5 kPa and < 250 kPa, respectively). The Mohr-Coulomb failure criterion shows that when low normal stresses are applied to the sample, the failure envelope tends to steepen, inducing values of internal friction angle higher than if measured at higher normal stresses (Schellart, 2000). This could explain the differences in results."*
Yes, I agree. As mentioned above, a short comment on the reason why the authors choose to measure the frictional properties under normal stress conditions that might be higher than in their model could be welcome.

L400 : "… for the develop**ment**…"

L420: "*and the erosional and mechanical response of the mix strongly change*."
Which is in agreement with measurements of frictional properties. And cite Table 2.

L431 : "… the *sdr* decrease**s**."

L441 : "… the *sdr* strongly decrease**s** in both phases"
L435-446 : A comment on the magnitude of the lower slope of the model in terms of erosion threshold and tentatively in terms critical shield stress might be worth here. I think the authors have the expertise to likely propose something. This could be related to the mechanical strength of the materials the authors measured.s

L448 : Eq. 11 is quoted but it might be Eq. 5 (or 13 ?).

L451 : Eq. 11 is quoted but it might be Eq. 13.

L455 : "*Our value for h are…*"
"In our models, calculation of h are …"

L456 : *"…with SM1 and CM2 that show values that are lower with respect to the other models."*
"…with SM1 and CM2 showing values that are lower with respect to the other models."

L452 – 462 : This paragraph is really interesting and worth in the publication but present phrasing renders the understanding not easy. I would suggest the authors to rephrase this section to be more straightforward.
BTW, it is surprising that CM1 and CM3/CM4 have similar value of "c" but very different shape of basin.

L454-455 : *"Values of h greater than 0.5 are typically interpreted as relative to basin elongation with increasing size"*.
What about "c" ?

L464-466 : "*We must point out that our models are not meant to simulate specific landscapes, but to explore how material properties influence landscape development. Despite the unavoidable limitations and simplifications of the model, it is tempting to compare the experimental and natural data*".
This sentence should appear earlier in the text, typically at the beginning of 4.4 section since it concerns also this paragraph on Hack's law.

L411-472 : Figure 12 could be cited here.

L482-484 : "*Both k_{sn}_MOD and θ_MOD (Fig. 12) are generally comparable with data coming from natural compilations (e.g. Kirby and Whipple, 2012). The matching of ksn and θ between models and nature supports future development and application of the analog materials tested in this study for modeling landscape evolution."*
What is written here is of course really important and should be strengthened to become more convincing for the audience. Presently, this comment is certainly "too short" to convince. Values of k_{sn} or θ could be quoted for instance. The geological contexts of selected natural examples could be also mentionned.

CONCLUSION
Conclusion is OK but :

L491-492 : "*granular materials and mixes of them deform following Mohr-Coulomb criterion. Adding GM and PVC to SP smooth the deforming properties of the SP, allowing for the formation of brittle structures;*"
I don't think this could be said in the conclusion since deformation has not been tested in the paper. In addition, SP also deforms brittlely (see fig 7.c in Graveleau et al., 2011). It is more the bulk strength of SP and the tectonic style (intense fracturing) that prevent SP to be a good analogue for both morphology and deformation.

L508 : "*Even if our findings…"*
I would not reduce the impact of this contribution (mostly at the end of the conclusion) but simply said that this publication goe a step forward in characterizing the erosion properties (and the origin of difference) for mixture of granular materials.
I would remove "Even if".

REFERENCE

I did not checked the accordance between the text and the reference list because I thrust the editorial board tools to do it properly and efficiently.

FIGURES

Figure 7 :
For CM3 and CM5, it is surprising to see the T2 profile above the T1 profile.

Figure 8 :
Data points should be indicated along the curves.

Figure 10 :
Phase I and II could be merged on a single diagram to emphasize the decrease in sediment discharge rate between the two phases.