DIPARTIMENTO di SCIENZE
Viale G. Marconi, 446
00146 - ROMA

Riccardo Reitano
Univ. Roma Tre

To: Dr. Fabien Graveleau
Referee, *Earth Surface Dynamics*

Reference: Response to referee comments to the manuscript *" Erosional response of granular material in landscape models"* [Paper esurf-2020-35] by R. Reitano, C. Faccenna, F. Funiciello, F. Corbi, S. D. Willett.

Dear Dr. Fabien Graveleau,

we would like to thank you for the corrections and the useful comments. We include hereafter, point by point, the reply (in *dark red italic* text) to all the Referee comments. We also upload the revised version of our manuscript. We hope the manuscript is now ready for the publication in *Earth Surface Dynamics*.

With my kindest regards,

Riccardo Reitano, on behalf of all authors.

Referee – Fabien Graveleau

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

Main Comments

1) 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.

We modify the text in agreement with what has been proposed by the Reviewer. Now the measurement for density and permeability in Table 2 show an accuracy of 10 kg/m^3 and 0.1 m s^{-1} or m^2, respectively.

As specified by the author, the density measurements here proposed refer to particle density (the name is now corrected in Table 2). As it is extremely difficult to control the amount of water inside the models during the runs, we decided to show only the particle density.

2) 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 ?

We agree with the Reviewer's comment. In the revised manuscript (lines 129-131) we have clarified this point.

3) L139 : "while the mechanical properties seem to show a common trend."

What do you mean by "mechanical properties" ? Not clear.

We agree with the Reviewer's comment. Since we better specified this concept in sections 4.2 and 4.3 of the manuscript, we removed this sentence.

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

Four tests per material have been made. We added this information in the revised version of the text (line 132).

5) 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).

The porosity has been calculated in both dry and wet conditions. This information is highlighted in the revised version of the manuscript (lines 152-154).

6) 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?

We think that the reasons why the values are different may lie in the handling technique, due to the sensibility of porosity values to this. Because even if the approach for measuring porosity is different respect to Graveleau et al. (2011), we also compared our values with the ones obtained measuring the weight of the same volume of material water-saturated and after drying it in a air oven. This last approach is the same used in Graveleau et al. (2011).

7) L214 : "Erosion and sediment discharge are computed with ad-hoc MATLAB

algorithms."Any reference ?

Following the Reviewer's suggestion, we added in the revised version of text (lines 221-222) a part in which we suggest looking into the repository (https://dataservices.gfz-potsdam.de/panmetaworks/review/d63845bbb81e2460d1e1e69dfbb0a189719d5d4de9364314c30d814e3695d1e9/) for any code has been specifically written for this paper.

8) 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 !

We agree with the Reviewer's comment. In the revised version of the manuscript (lines 389-396) we have compared our results with Graveleau et al. (2011) and Strak et al. (2011).

9) 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 ?

We agree with the Reviewer's comment. We have added a discussion about the erosion threshold in section 4.3 (lines 469-481). As far as the angle for CM2 and its comparison with MatIV are concerned, we performed a better estimation of the surface slope using MATLAB, correcting the previous results. The results now show comparable values (line 286).

10) 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.

We thank the Reviewer for pointing out this missing information in Fig. 7. The figure is now revised according to the Reviewer's suggestion.

11) 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.

We agree with the Reviewer's comment. Indeed, that comparison has been already made in section 4.1. We have added a few lines also in the revised version of this section to better show the comparison (lines 386-388).

12) "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).

We are not sure about the reason why such a difference. We can speculate that the differences are mainly due to the diverse experimental approaches (e.g., handling technique, apparatus, materials etc.) adopted in the two works. However, only ad hoc tests could confirm this idea. Hence, due to the uncertainties of the case, we decided to avoid commenting this specific point.

13) 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.

We agree with the Reviewer, so we added a comment about the reasons behind the normal stresses adopted in the work (lines 129-131).

14) 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.

In agreement with the reviewer's comment, we add a comment about the erosion threshold in section 4.3 (lines 469-481).

15) 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.

In agreement with the Reviewer's suggestion, we provided the needed changes to this section, which now includes: 1) the meaning of Hack's Law and related constants; 2) ranges in the lab and nature; 3) the *meaning of values of "h" larger than 0.5. We think that now the readers can better understand the meaning of the analyzed parameters.*

16) L454-455 : "Values of h greater than 0.5 are typically interpreted as relative to basin elongation with increasing size".

What about "c" ?

With the aim of making the section 4.4 more clear, we have only provided natural range for the constant "c", focusing on the meaning of "h" in terms of drainage network geometry.

17) L482-484 : "Both ksn_MOD and ._MOD (Fig. 12) are generally comparable with data coming from natural compilations (e.g. Kirby and Whipple, 2012). The matching of ksn and . 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 ksn or . could be quoted for instance. The geological contexts of selected natural examples could be also mentioned.

We agree with the Reviewer. Now Figure 12 includes field data. Natural compilations are also proposed in the text (e.g. Kirby and Whipple, 2012).

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

We thank the Reviewer for pointing out this error in the figure. The stream extraction coming from Topotoolbox also extracted a numerical artifact at the bottom of the box. We fixed these streams in the revised version of the manuscript.

18) 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.

We agree with the Reviewer's comment and we have changed the revised figure accordingly.

Last but not least, in the revised version of the manuscript we have edited every grammatical error/mistyping the reviewer has highlighted. We have also added the references proposed by the Reviewer.

References

- Graveleau, F., Hurtrez, J., Dominguez, S. and Malavieille, J.: A new experimental material for modeling relief dynamics and interactions between tectonics and surface processes, Tectonophysics, 513, 68–87, doi:10.1016/j.tecto.2011.09.029, 2011.
- Kirby, E. and Whipple, K. X.: Expression of active tectonics in erosional landscapes, Journal of Structural Geology, 44, 54–75, doi:10.1016/j.jsg.2012.07.009, 2012.
- Strak, V., Dominguez, S., Petit, C., Meyer, B. and Loget, N.: Interaction between normal fault slip and erosion on relief evolution: Insights from experimental modelling, Tectonophysics, 513(1–4), 1–19, doi:10.1016/j.tecto.2011.10.005, 2011.