Comments from reviewer

The manuscript presents different approaches of characterizing particle flows down an incline: small-scale experiments with glass/plastic beads, large-scale experiments using granular material of sand/gravel, and the respective representation of the smallscale experiments using simulations based on Discrete Element Method (DEM) with the commercial code PFC2D. The motivation of the work is to provide an insight of the flow process and segregation of debris flows, although no consideration of fluid is present. Six types of particles have been used in the small-scale experiments, three of them are made of glass and the other three are made of plastic. 68 tests with a fixed inclination of 45 degrees have been carried with different mixtures of particles in the flowing mass: mono-disperse mixtures, mixtures with two types and also mixtures with three types, but no sensors where installed to measure flow velocities or depth. The main investigation was concerning the segregation process and the role of particle size and density in it. Afterwards, 2-D numerical simulations where carried out to model the effect of segregation and also the presence of a jump with two types of particles, with no mention of the contact law or governing equations. The calibration process was not shown in the paper and only qualitative comparisons with the small-scale experiments were carried out. The presence of a jump was found to slightly modify the flowing velocity, leading to dissipation of kinetic energy of the flow. Finally, large scale experiments were carried out in longer flume with granular material consisting of sand/gravel with different sizes (five samples). A long qualitative description of the results claiming that the segregation process is well presence with same observations as the small-scale experiments (ex: inverse grading phenomenon). Discussions and conclusions are then presented. Some parts are written with a poor level of English which makes it hard for the reader to follow. In order to decide on the publication of this paper in NHESS, I would like to highlight the following points:

First, the paper is titled ‘debris flow’, although a more appropriate name would be ‘dry particle/granular flow’, since no presence of fluid is considered. Such a presence would greatly influence the flow behavior and change its kinematics. The dry granular flow considered in the study, with the relatively big size of particles chosen could better resemble a rock avalanche than a debris flow. Moreover, the study focuses mainly on segregation process and effect of particle size, and this should be reflected in the title.

Second, the introduction mentions lots of statistics concerning the previous slope failures in China with no proper referencing (Lines 30-42). The same applies to the figures of previous events (e.g. Fig 1) where no reference is cited. In addition, when speaking about previous studies of debris flows, too much details are given that are unnecessary (e.g. the location of USGS flume). So many numbers are given concerning the geometry of previous flumes but no proper conclusions/open challenges of their work are presented (Lines 89-102). Previous work on modeling debris flow where detailed too much with no added values (see for example equations 1 and 2 which are not used in the script afterwards), especially that these models where not based on DEM, which is the core of the present paper. On DEM studies, the authors failed to present a proper scientific literature review of the previous studies on granular flows modeling with DEM, and wrote instead a brief paragraph (Lines 132-137) on that with no highlight of what still needs to be done on this subject, Especially DEM simulations of segregation process. For the small-scale lab experiments, the discerption of the carried out tests lack clarity and is found to be confusing for the reader (Lines 181-203). Furthermore, the quality of the images showing snap shots of the flow process is poor with no enough brightness, which makes it hard to drive strong conclusions. In addition, very often statements are made with no solid proof or measurements (e.g. lines 240-244 and lines 256-262). Such statements could be taken as assumptions to explain certain phenomenon but not as affirmative statements. More importantly and unfortunately, the experiments where carried out with no sensors to characterize flow depth and flow velocity (flow velocity could however be back calculated from the High speed camera). More importantly, for DEM simulations, the section starts with mentioning studies on the run out which is not in scope of the paper (lines 286 – 288). In addition, no proper presentation has been given concerning the used contact law or the governing equations. Is it purely elastic? Elasto plastic? Elasto viscoplastic?. Particle are created in the model using the 'rain method' with no description of what it means: how are particles generated and at what time/condition do authors consider the sample to be in quasi-static condition and open the gate? Moreover, authors assume that their model is calibrated only by qualitative comparison with the experimental data. Very long description of the apparent 'agreement' between the model and experiment is detailed although such agreement is hard to judge because it is only qualitative and because of the low quality of experimental images. For a calibration to be justified, a more in-depth comparison with flow velocity and depth should have been carried out between model and experiment. There is also no presentation of the most sensitive parameters of the model that needed calibration. Such a calibration process is crucial for the understanding of the model's results. It might be the case that same results could be obtained with more than one set of parameters. Strong arguments are presented concerning the flow regime and whether it is inertial or frictional, although no concrete measurements exist to calculate Froude number using velocity and height. In Fig 12, it is not clear which velocity is it (in flow direction, the norm of the velocity vectors .. etc). The only paragraph that is supported by quantitative measurements is in lines 400-415, although results of Fig 13 are hard to read due to its poor quality.

The large-scale experiments were also carried out with proper quantification of flow velocity and height through sensors. Authors depended on qualitative description of the segregation process which is harder to judge that the colored particles in the smallscale experiments. Authors claim that the results are similar to those of many previous studies, although it is not supported by quantifiable evidence.

The discussion part, which is supposed to take one step deeper in the analysis of results, included only a mixture of the abstract, the previously presented literature and a repetition of what have already been said in the previous section concerning the segregation phenomenon and the energy dissipating function of the jump. Claims concerning the savage number are not supported with measurements.

Reply

The authors would like to thank the reviewer for the constructive comments, based on which a revised manuscript has been prepared to address the comments. Some of the questions as raised are not precisely the aim of the present study, and practically outside the capability of the computational technique/numerical model that is available up to the present. As mentioned in the conclusion of the revised manuscript: "The authors have chosen flexible spherical rubber beads as well as rigid glass beads for the laboratory. The segregation process as found from the laboratory test is actually similar to that in the field tests using non-spherical sand. Through such selection, it is clearly demonstrated that particle size distribution is a very critical factor in the segregation process, and it appears that it is more critical than particle shape or stiffness." The main work from the present study is more on qualitative than quantitative, though we also aim to produce useful quantitative results from DEM, but this is not the main theme of the work, as this is usually achieved by tuning the micro-parameters (previous papers by the authors as well as many research papers on DEM).

The authors would like to reply the comments as follows:

1. The title of the paper has been reworded to "Comparisons between Laboratory and Field Test and Distinct Element Analysis of Dry Granular flows" for clarity. There is however two very major limitations in the study : time and money. We need to pay for the workshop and the field equipment as well as various personnel working on site (the test site is in China, and further expenses are required for our research personnel's travelling and living allowances). Furthermore, we have only 1 month time in the test, from preparation to actual field tests. We are now trying to secure more fund for the next stage of works, for which the wet tests will be conducted. If fund is secured, we will go to the next step, and the new results will be presented under the title of debris flow.
2. The authors have removed many unnecessary case history/statistics, and have added the references for some of the given cases.
3. Eqs.(1) and (2) are removed. Thanks to the comments.
4. With reference to the DEM works, the authors would like to emphasize that the

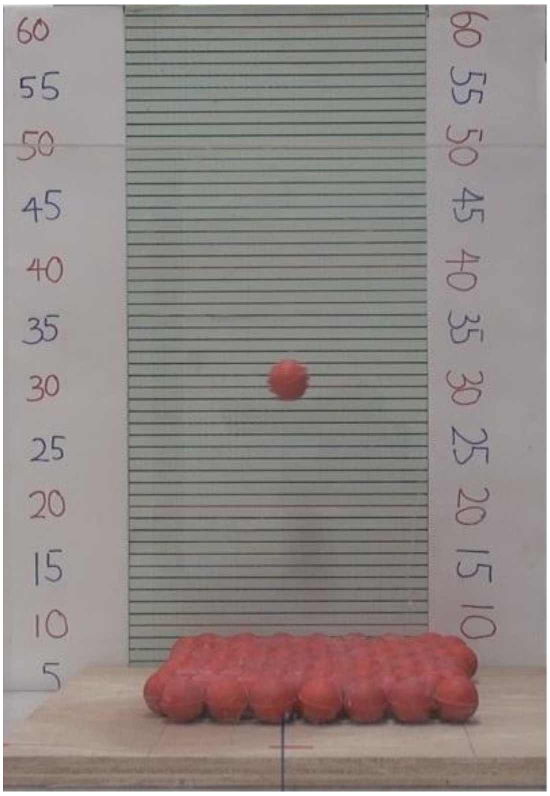
most important result from the present study is that particle size distribution is the most important factor in the segregation process. The DEM analysis is carried out to illustrate that such phenomenon is also found from numerical analysis, but it is not the main theme of the work. The authors have published series of paper on DEM previously, and the limitations of DEM on quantitative study is well understood. In the revised manuscript, the authors have also included

5. We are sorry that some of the photos are not sharp enough, but qualitatively the flow and segregation process can be seen. To help the readers, we have provided the email address, through which the authors will provide the movie files for all interested readers.
6. The comments as given in lines 240-244 and 256-262 are based on that from DEM analysis, and this is mentioned in the revised manuscript. Just from the high speed camera and video, many internal phenomena are still difficult to be understood, and this is also the reason for carrying out DEM studies. Even though DEM cannot provide good quantitative results, qualitatively the results are still reasonable.
7. Flow velocity can be traced back from the high speed camera photos and the movie, but we do not present the results here because it is not the main theme of the present study. Moreover, due to the limitations of DEM to provide sufficient accurate quantitative results, we have no intention to present the flow vector in the first draft of the paper, but prefer to spend more effort on the segregation process. Every year, there are debris flows in Hong Kong, and segregation is always found. The title of the paper is also reworded to “Laboratory and Field Test and Distinct Element Analysis of Dry Granular flows and segregation process” to better reflect the main theme of this paper.
8. For the DEM analysis, the materials are taken as elastic, and the contact model is a simple elasto-plastic model based on that by Zohdi T.I. (2007) as given in PFC2D. It is mentioned in the revised manuscript. Sorry for not mentioning about these information.
9. The authors agree that an in-depth comparisons between flow velocity, depth, run-out and other results should be done to justify the agreement between DEM and test results. The authors have not done that because: (1) this is not the main theme of the present work, as mentioned previously; (2) a good matching can always be found by tuning the micro-parameters in DEM, which is the approach used by authors’ papers and many other researchers’ papers; (3) the authors have actually tried “limited” tuning in the micro-parameters, and qualitatively the same flow and segregation process are found. The authors are fully aware of the lack of quantitative study in the present work, and hopefully it will be the next step of

work, where water is added in the test. There is however two very major limitations in the study : time and money. We need to pay for the workshop and the field equipment as well as various personnel working on site (the test site is in China, and further expenses are required for our research personnel's travelling and living allowances). Furthermore, we have only 1 month time in the test, from preparation to actual field tests. We are now trying to secure more fund for the next stage of works, for which the wet tests will be conducted. We hope to carry out more detailed study about the flow process, and at that stage we will try to concentrate more on the quantitative study.

10. Referring to the parameters, many of the parameters are given in Table 3 are based on our friction tests, deposition tests and rebound tests, and sample photos are attached herewith

deposition test



rebound test

11. As mentioned previously, we have actually carried out limited tuning of the micro-parameters (not shown) in our internal studies. Since the flow and segregation are practically not affected by the change of the parameters (but the actual value of the flow velocity, run-out … are affected), we have not included these results in the paper, and discuss one of the most important issues which are relevant to that in Hong Kong-segregation.
12. The flow process and segregation process from laboratory and field tests are similar in many respect – largely controlled by the particle size distribution. We can see clearly about that, and only limited photos are shown to limit the length of the paper. Any interested reader can get the complete set of photos from the authors if necessary. Again, the authors have not attempted to compare the laboratory and field test results quantitatively, as they are not comparable directly.
13. We have not done anything on the Savage number, as this is not the main theme of the study.