OPEN GEOMECHANICS

Review History for "The perpetual shearing of granular soils under low stresses using the stadium shear device"

Yang Liu, François Guillard, Benjy Marks, Pierre Rognon and Itai Einav 2022

Summary

The paper was sent to two reviewers — Dr. Ishan Srivastava (Reviewer A) and Dr. Emilien Azema (Reviewer B). The reviewers remained anonymous during the entire review process and the authors were anonymous for the reviewers. After the reviewing process was complete, all reviewers agreed to disclose their identity. In Review Round 1, the reviewers provided a series of comments for the authors and required a minor revision of the manuscript. In Review Round 2, the reviewers recommended the manuscript for publication. After the Review Round 2 the managing Editor decided to accept the manuscript for publication.

Review Round 1

Reviewer 1

Synopsis: The manuscript describes a stadium shear device (SSD) that enables perpetual shear strain experiments of granular materials with a uniform stress distribution. This device is reported to be a significant improvement over existing rheological devices that are either limited by the accumulated strain or involve non-uniform stresses, both of which complicate the rheological analysis. The authors analyze the stress response of various experimental soil samples and validate them against discrete element method (DEM) simulations. The authors carefully describe the implicit assumptions in their experimental analysis and test those assumptions through DEM simulations. In their experiments, they find weak rate-dependence of stresses for glass beads, whereas moderate rate-dependence is observed for angular particles. They also test the uniqueness of the critical state by conducting experiments from different starting void ratios.

The manuscript is very well-written and proceeds logically starting with a detailed description of the SSD device, followed by a description of the DEM simulations, and finally leading towards rheological results of various sand samples. Although DEM naturally lends itself to analyzing granular flows with uniform stresses and arbitrarily large accumulated strains, the ability to realize the same conditions in experiments is a significant advance in the field that will open up avenues for directly testing granular rheology.

My suggestions for improvement are listed below:

Major Comments:

1. The authors have chosen certain dimensions for their device. However, it is known that non-local effects can become important in confined granular flows (see works by Ken Kamrin and David Henann, among others).

Can the authors comment on the relevance of such non-local effects in their device? Particularly, the ratio of particle diameter to the distance between walls is the key parameter that governs the strength of non-locality. How can such effects be accounted for in their analysis?

2. Under what conditions do the authors expect the assumption of uniform stress to breakdown? It is well-known that granular materials flow in narrow shear bands near walls, and it is possible that the bulk of the material in experiments is either static or slowly creeping. Particularly, such flow banding can be enhanced at high interparticle frictions. The authors show the spatial variation of stresses from DEM for μ_s 0; however, I would like to see the same results for high friction (both sliding and rolling).

3. Similar to the comment above, the stress ratio s, which also denotes the "first normal stress difference" in fluid mechanics is known to increase with both interparticle friction and inertial number (see simulations by Srivastava et al., JFM, 907, A18 (2021) for a detailed analysis of normal stress differences). Have the authors tested the value of s at high friction and strain rates in their DEM simulations? Although this second-order effect is usually small, it is possible that it becomes significant for angular particles or materials with large size dispersity of particles. Because σ_{xx} cannot be measured directly, how will their rheological analysis adapt when s 1 assumption breaks down?

4. Based on comments #1-3, I think the authors should discuss the limitations of their setup in the manuscript, particularly likely scenarios where some of the implicit assumptions of the device and the stress analysis is likely to break down. Such a discussion will be highly informative to potential future users of their device.

Minor comments:

1. Do the authors observe stick-slip behavior at very small strain rates? Is there a lower limit to the inertial number below which stick-slip will dominate the stress response? How does the device handle such stick-slip behavior?

2. Right above equation 12, did the authors mean "... bounded by rigid walls in the x and z direction, and periodic in the y direction."?

3. The authors report 10s of shearing in their DEM simulation. How much of a total strain does that correspond to?

4. In first para of Sec. 2.5, the authors describe the coefficient of friction as μ_s. Shouldn't this be μ_s^p?

Reviewer 2

I have now reviewed the manuscript entitled "The perpetual shearing of granular soils under low stresses using the stadium shear device" submitted to the journal "Open Geomechanics". In this paper the authors introduce a new experimental device allowing to shear "indefinitely" granular systems under a given pressure. Reaching, what we call "the residual state" by means of experiments is clearly challenging since, in general terms, very long deformations are necessary but often impossible to reach with classical apparatus (such triaxial) because of finite dimension of such systems. We can nevertheless mention "couette systems" in which perpetual rotation are imposed but inducing radial dependencies on stress properties that must be subtracted. In this sense, the new apparatus presented here allows, to a large extent, to get rid of these difficulties. Furthermore, numerical simulations, using DEM, are also performed in order to test and validate some of the hypothesis done in the experimental method. Finally, some results are presented regarding the effects of particle shape or particle size distribution on the measured strength properties in the residual state.

To be completely honest, I must confess that I am not an experimenter. So, I read with interest the parts 2.1-2.2 which seem to me to be clear and to give all the necessaries information describing the experimental method. I can follow all the explanations. Nevertess, I am not sure/able to detect if there are any questionable elements in these sections. Concerning the numerical part, it would be nice to specify that the authors use "soft-dem" approaches (which is the case of the implementation in the MercuryDPM code I think). The authors mention that they use a spring/damper type contact law (which is typical of soft-dem) but of linear type. However, in 3D, the contact law between two spheres according to Hertz's law is non-linear and results in a 2/3 dependence between force and penetration at contact. It would be nice to motivate this choice and this simplification. Could this simplification have an effect on the choice of the assumptions that have to be verified? The authors introduce the dimensionless number I to quantify the state of the system that will be simulated. However, with soft-dem methods at least one additional dimensionless parameter must be introduced which is the normalized stiffness: $P*$ k/Pd^2. The authors clearly mentioned that, by varying P they have made sure that I is constant. But in same time $P*$ must change and therefore the global behavior of the particles could vary. However, the values of the stress ratio measured at I 0.001 and I 0.01 (in Figure 5b) are closed to values already published for spherical particles and the error bars seem to show that the effect of P (and thus $P*$) has little effect. Thus, it would be good to only specify/mention that the variations of P between 1000 and 16000 remain "acceptable" values for the use of the soft-DEM in this context.

Then, in Sec.3 and 4 the authors present some parametric studies by comparing numerical and experimental data as well as discussing about particle shape, particle size effects or the effect of the initial packing fraction. It might be nice here to put some pictures of the samples. It is not clear to me what "nepean river" looks like, or to see the differences in size/shape for polydisperse case? In this section, Figures 7, 9 and 12 are very nice since we can see clearly that packing fraction and stresses are reaching a well-defined constant state after a very long deformation. It would be nice also to present these figures (or at least to mention in the text) in terms of percent of deformation.

Concerning the effect of particle size distribution, the authors found that: "the shear strength is found to increase with grain size polydispersity. Which seems to be aligned with a previous work of "Simoni and Houlsby [2006]". I would like to suggest to the authors to be more "careful" about this statement. Indeed, several DEM-based numerical results seem to show the opposite, that the shear strength is independent of the grain size distribution. In this case the grain shapes are perfectly controlled and identical in each size class. On the contrary, laboratory experiments show that the resistance increases, decreases or remains constant as a function of the grain size distribution depending on the nature of the materials used. Here non-trivial couplings between shape and size could explain these apparently contradictory results. And so, in the state of the results presented in this article, it does not seem clear to me to attribute the increase in (residual) shear strength to the grain size alone (this is, in fact, still an open question). Finally, the authors show that the shear strength in the residual state increases as the shape moves away from that of a spherical shape, which is consistent with results from the literature and shows that the new device developed is also able of capturing such details.

In conclusion I think that this is a good article and after addressing the few remarks and questions planted in this report, it could be accepted as regular article.

Author Response

We thank the reviewers for their careful and thoughtful comments. We have made significant revisions to the manuscript that seek to address these comments. The complete reviews are contained below in italics, with our responses and actions listed accordingly.

Synopsis: The manuscript describes a stadium shear device (SSD) that enables perpetual shear strain experiments of granular materials with a uniform stress distribution. This device is reported to be a significant improvement over existing rheological devices that are either limited by the accumulated strain or involve non-uniform stresses, both of which complicate the rheological analysis. The authors analyze the stress response of various experimental soil samples and validate them against discrete element method (DEM) simulations. The authors carefully describe the implicit assumptions in their experimental analysis and test those assumptions through DEM simulations. In their experiments, they find weak rate-dependence of stresses for glass beads, whereas moderate rate-dependence is observed for angular particles. They also test the uniqueness of the critical state by conducting experiments from different starting void ratios.
The manuscript is very well-written and proceeds logically starting with a detailed description of the SSD device, followed by a description of the DEM simulations, and finally leading towards rheological results of various sand samples. Although DEM naturally lends itself to analyzing granular flows with uniform stresses and arbitrarily large accumulated strains, the ability to realize the same conditions in experiments is a significant advance in the field that will open up avenues for directly testing granular rheology.

My suggestions for improvement are listed below:

Major comments

The authors have chosen certain dimensions for their device. However, it is known that non-local effects can become important in confined granular flows (see works by Ken Kamrin and David Henann, among others). Can the authors comment on the relevance of such non-local effects in their device? Particularly, the ratio of particle diameter to the distance between walls is the key parameter that governs the strength of non-locality. How can such effects be accounted for in their analysis?

Response:
This is a very relevant point. The current state of knowledge on non-locality allows us to provide some elements of discussion, while leaving some questions open.

As a general comment, several works on non-locality pointed out the existence of a cooperative length diverging as the inertial number tends to zero. As a result, non-local effects are expected to play some role in any finite-size experimental configuration, provided that the inertial number is low enough. While we cannot quantify this effect with certainty in our experimental configuration, some insights can be obtained from the numerical results presented in Rognon & al., 2015, JFM by considering the following:

- Our experimental device involves some plane shear within the belt with a wall distance H of 124mm. We used grain of diameter d ranging from of 0.425mm to 3.35mm. This leads to a ratio H/d ranging from 291 to a minimum of 37. The explored inertial numbers are greater than 0.001.
- Rognon & al (2015, JFM) numerically studied non-local effects in a very similar geometry: plane shear between two rough walls, with inertial number greater than 0.001. They found that non-locality induced a drop in effective viscosity near the walls. However, they reported that the effect of that drop on the shear stress measured at the wall, and the subsequent $\mu(I)$ curves, was very small for system of size H $20d$. In smaller systems ($H \approx 20d$), they reported a noticeable decrease in effective friction μ, and an increase in solid fraction.

This analogy tends to indicate that non-local effects should not affect the stresses or solid fraction in the SSD, even with the biggest grains (H/d 37). However, the work or Rognon & al. considered quasi-2D disks which are different from the 3D angular grains used in our experiments. Whether and how much the dimentionality and grain angularity exacerbate non-local effects in not known.

Action:
We have introduced this discussion in the new section 4.5

Under what conditions do the authors expect the assumption of uniform stress to breakdown? It is well-known that granular materials flow in narrow shear bands near walls, and it is possible that the bulk of the material in experiments is either static or slowly creeping. Particularly, such flow banding can be enhanced at high interparticle frictions. The authors show the spatial variation of stresses from DEM for μ_s 0; however, I would like to see the same results for high friction (both sliding and rolling).

Response:
There is indeed much evidence for shear localisation in configurations other than plane shear (e.g. cylindrical Couette or ring shear) that include an intrinsic stress gradient strictly due the boundary geometry. In plane shear flows without gravity, numerical simulations (for instance those of da Cruz 2005, PRE): (i) confirmed the absence of stress gradient that the continuity equation for momentum predicts, and (ii) indicated an absence of shear localisation across a range of grain friction coefficients. Experimental plane shear flows such as our previously established 2D stadium shear confirmed the absence of a strong shear localisation provided the friction with the base plate was negligible.

Our 3D SSD was designed in order to replicate this plane shear configuration and avoid stress heterogeneity and the associated shear localisation. Nonetheless, the friction between the flowing material and the top and bottom walls may impact the shear and normal stress distribution in the x direction, which can in turn induce shear localisation. In the limit of a very large system in the x direction (very large gap between the belt), we would expect a significant "stress screening effect" in which the shear stress and normal stress in the x direction would decrease toward the centre of the flow, possibly leading to stress gradients near the walls. Similarly, a narrow gap and a tall system could induce a Janssen effect in which the vertical normal stress would decrease from the top to the bottom walls. Accordingly a system with an aspect ratio of the order of 1 appears to be best suited to mitigate such stress gradients.

Action:
We have introduced this discussion in the new section 4.5

Similar to the comment above, the stress ratio s, which also denotes the "first normal stress difference" in fluid mechanics is known to increase with both interparticle friction and inertial number (see simulations by Srivastava et al., JFM, 907, A18 (2021) for a detailed analysis of normal stress differences). Have the authors tested the value of s at high friction and strain rates in their DEM simulations? Although this second-order effect is usually small, it is *possible that it becomes significant for angular particles or materials with large size dispersity of particles. Because σ_{xx} cannot be measured directly, how will their rheological analysis adapt when s 1 assumption breaks down?*

Response:
We thank the reviewer for this interesting work of which we were not aware. We have performed additional DEM simulations at elevated interparticle friction values (up to 1.0) and the first normal stress difference follows qualitatively the behaviour described in Srivastava et al. We have not performed an exhaustive analysis of this parameter space, but have identified that in general the assumption of $s \approx 1$ can be improved by increasing the interparticle friction.

Action:
We have added the following text to section 2.5: While these values are approximately unity for the investigated DEM samples, we expect that the values will increase slightly with increased inter-particle friction and/or inertial number, as reported in Srivastava et al. [2021]. DEM measurements in the stadium shear geometry confirm these observations. For values of I 10^{-2}, increasing the inter-particle friction tends to increase the level of agreement with the assumption of $s \approx 1$.

Based on comments #1-3, I think the authors should discuss the limitations of their setup in the manuscript, particularly likely scenarios where some of the implicit assumptions of the device and the stress analysis is likely to break down. Such a discussion will be highly informative to potential future users of their device.

Response:
We agree.

Action:
We have introduced this discussion in the new section 4.5

Minor comments:

Do the authors observe stick-slip behavior at very small strain rates? Is there a lower limit to the inertial number below which stick-slip will dominate the stress response? How does the device handle such stick-slip behavior?

Response:
We did not observe any clear stick-slip behaviour occurring in the device in the regimes we explore. Even at the lowest velocities the shearing of the materials seems to be continuous from the signals we recorded and our observations. There are probably regimes where such effect could dominate and prevent reliable measurements, but we did not encounter them. It is therefore difficult to suggest a limit for the inertial number range at which these instabilities would occur, as it would be strongly affected by the overall rigidity of the device, the properties of the tested material, and the strain rate and confining pressure applied.

Right above equation 12, did the authors mean "... bounded by rigid walls in the x and z direction, and periodic in the y direction."? The authors report 10s of shearing in their DEM simulation. How much of a total strain does that correspond to?

Response:
Yes, thanks for spotting this, it should indeed be "x and y directions". Also, 10s of shearing in the DEM corresponds to a wide range of nominal shear strain, and we have added the full range of values tested to the text.

Action:
Text fixed as suggested. We have also added the following text:
The total distance sheared is in the range of 0.005 to 30 m, depending on the case studied.

In first para of Sec. 2.5, the authors describe the coefficient of friction as μ_s. Shouldn't this be μ_s^p?

Response:
The notation is fine. We confirm it should be μ_s at that place, being the friction coefficient between grains and the top and bottom plates.

Action:
Text clarified to distinguish μ_S^p from μ_s.

Reviewer B:

I have now reviewed the manuscript entitled "The perpetual shearing of granular soils under low stresses using the stadium shear device" submitted to the journal "Open Geomechanics". In this paper the authors introduce a new experimental device allowing to shear "indefinitely" granular systems under a given pressure. Reaching, what we call "the residual state" by means of experiments is clearly challenging since, in general terms, very long deformations are necessary but often impossible to reach with classical apparatus (such triaxial) because of finite dimension of such systems. We can nevertheless mention "couette systems" in which perpetual rotation are imposed but inducing radial dependencies on stress properties that must be subtracted. In this sense, the new apparatus presented here allows, to a large extent, to get rid of these difficulties. Furthermore, numerical simulations, using DEM, are also performed in order to test and validate some of the hypothesis done in the experimental method. Finally, some results are presented regarding the effects of particle shape or particle size distribution on the measured strength properties in the residual state.

To be completely honest, I must confess that I am not an experimenter. So, I read with interest the parts 2.1-2.2 which seem to me to be clear and to give all the necessaries information describing the experimental method. I can follow all the explanations. Nevertess, I am not sure/able to detect if there are any questionable elements in these sections. Concerning the numerical part, it would be nice to specify that the authors use "soft-dem" approaches (which is the case of the implementation in the MercuryDPM code I think). The authors mention that they use a spring/damper type contact law (which is typical of soft-dem) but of linear type. However, in 3D, the contact law between two spheres according to Hertz's law is non-linear and results in a 2/3 dependence between force and penetration at contact. It would be nice to motivate this choice and this simplification. Could this simplification have an effect on the choice of the assumptions that have to be verified? The authors introduce the dimensionless number I to quantify the state of the system that will be simulated. However, with soft-dem methods at least one additional dimensionless parameter must be introduced which is the normalized stiffness: $P*\ k/Pd^2$*. The authors clearly mentioned that, by varying* P *they have made sure that* I *is constant. But in same time* $P*$ *must change and therefore the global behavior of the particles could vary. However, the values of the stress ratio measured at* $I\ 0.001$ *and* $I\ 0.01$ *(in Figure 5b) are closed to values already published for spherical particles and the error bars seem to show that the effect of* P *(and thus* $P*$*) has little effect. Thus, it would be good to only specify/mention that the variations of* P *between* 1000 *and* 16000 *remain "acceptable" values for the use of the soft-DEM in this context.*

Response:
Many thanks for these important suggestions. Indeed, the contact between two spheres is best described by the non-linear Hertz law. In polydisperse systems individual particles often involve many more contacts, and thus one can question the applicability of Hertz law, especially when tangential forces are simultaneously applied. Furthermore, in natural material particle roughness can often govern the observed relationship between the force and deformation at contacts. Moreover, while these details are surely critical for determining bulk elasticity, their influence on the mobilised friction remains questionable. For these reasons we opt for simplicity. Nevertheless, future studies are recommended to explore these details in the context of the SSD that probe into these potential (likely second order) effects on the interpretation of the results. Similarly, the point of potentially considering the normalised pressure/stiffness as an additional dimensionless number for the description of granular flows is valuable. But as the referee correctly assessed, this is more so the case for systems under substantially larger pressures than those studied in this paper.

Action:
We have added a discussion and reasoned the simplification of the contact law and the dimensionless number at the end of Sec. 2.4, along the above response.

Then, in Sec.3 and 4 the authors present some parametric studies by comparing numerical and experimental data as well as discussing about particle shape, particle size effects or the effect of the initial packing fraction. It might be nice *here to put some pictures of the samples. It is not clear to me what "nepean river" looks like, or to see the differences in size/shape for polydisperse case? In this section, Figures 7, 9 and 12 are very nice since we can see clearly that packing fraction and stresses are reaching a well-defined constant state after a very long deformation. It would be nice also to present these figures (or at least to mention in the text) in terms of percent of deformation.*

Response:
Thanks for the nice idea to add the pictures. The reason for not presenting figures 7, 9 and 12 in terms of percent deformation is that we did not want readers to assume the deformations are homogeneous throughout the sample. Nevertheless, it is true that such a detail would clarify the largeness of the imposed deformation.

Action:
Added figure 6 with images of the samples. Comments on the (now stated value of the) large deformation required to achieve critical state have been added in Sec. 4.1 and 4.2.

Concerning the effect of particle size distribution, the authors found that: "the shear strength is found to increase with grain size polydispersity. Which seems to be aligned with a previous work of Simoni and Houlsby [2006]". I would like to suggest to the authors to be more "careful" about this statement. Indeed, several DEM-based numerical results seem to show the opposite, that the shear strength is independent of the grain size distribution. In this case the grain shapes are perfectly controlled and identical in each size class. On the contrary, laboratory experiments show that the resistance increases, decreases or remains constant as a function of the grain size distribution depending on the nature of the materials used. Here non-trivial couplings between shape and size could explain these apparently contradictory results. And so, in the state of the results presented in this article, it does not seem clear to me to attribute the increase in (residual) shear strength to the grain size alone (this is, in fact, still an open question). Finally, the authors show that the shear strength in the residual state increases as the shape moves away from that of a spherical shape, which is consistent with results from the literature and shows that the new device developed is also able of capturing such details.

Response:
Thanks for this important insight. This is absolutely a fair point. Obviously, the reviewer is right that the interpretation of the effect of polydispersity must be taken more carefully in light of insight from DEM simulations, and the inevitable coupling of grain shape and size in natural sand.

Action:
A discussion was added at the end of Sec. 4.4.

In conclusion I think that this is a good article and after addressing the few remarks and questions planted in this report, it could be accepted as regular article.

Review Round 2

Reviewer 1

I commend the authors for comprehensive and well-considered responses to my comments, and for undertaking the necessary revisions. I recommend the publication of the revised manuscript in its current form.

Editorial Decision

At this point in the review, Reviewer 2 had not yet responded to a request to review the revision while Reviewer 1 had already recommended the publication of the revised manuscript for publication. The editor reviewed the response of Reviewer 1 and the authors' responses to the comments of Reviewer 2 and made the decision to accept the revised manuscript for publication.