Review History for "Removal of the membrane penetration error from triaxial data"

Andrzej Niemunis & Lukas Knittel *2020*

Review Round 1

Reviewer 1 (Anonymous)

This was a very interesting study, but I felt could have been made a little more accessible to the average reader. Fewer acronyms, a little more explanatory text and more variables appearing in the ?essential? notation would help. I also thought that the use of ?isochoric? was not so helpful (I had to look it up!). The use of the word ?signal? was a bit odd for what were test data. As an experimentalist, I thought that the authors might have mentioned a bit more the issues that more typically affect us. I suppose that the comment that the undrained shear strength would be overestimated was quite correct, but was maybe a bit overstated; I guess that it is not often that we would design with an undrained shear strength in a soil that has a grain size large enough to suffer from membrane penetration issues. However, the focus of the paper on cyclic loading is entirely justified. Unfortunately the example they gave in Section 5 was not that clearly explained and the authors could do more to improve accessibility, maybe adding some words to explain the meaning of the negative corrected p? values and the meaning of the shift of the stress path. It might also have helped to show what does go wrong if corrections are made to the data at very low p? values. Has the effect of the membrane stiffness on the radial stress been accounted for? I suppose that for drained loading we would use only local strain measurements or for shearing we might restrict ourselves to a conventional constant effective cell pressure stress paths, and hope that the membrane pressure component would not alter and so could be ignored. I guess that is not quite true as the void ratio changes and the membrane span between particles alters. Can the authors comment on the validity of this approach? Fig.1 typo site not side?

Reviewer 2 (Anonymous)

General comments

The author(s) present a very interesting and thorough paper about membrane penetration effects on the measured results of cyclic triaxial tests on coarser grained materials. A great deal of work has obviously been put into developing the correction model.

The next to last paragraph in the Introduction states that the MP is known for affecting the triaxial data for soils with d20 > 1 mm. The d20 of the material tested herein (Appendix A) was less than 1 mm (less than 0.54 mm, perhaps closer to 0.4 mm), yet the corrections shown were enormous. Why? There are more parameters that would affect MP besides d20, like gradation (fines content) of the material, membrane elasticity, membrane thickness, etc. None of this is mentioned or considered herein.

This reviewer does not believe the magnitude of the correction shown in this paper. The sand material tested was not particularly coarse, but the corrections were enormous. This paper essentially precludes all cyclic triaxial testing on sand, which cannot be the case. More verification is required to show these enormous corrections being realistic. This is the most important comment by this reviewer. This could be done, for example, by repeating some of the tests presented with thicker or double or triple membranes, where membrane penetration effects would be less.

This reviewer does not feel the method used for applying the correction in the paper is correct or properly/adequately justified. The corrected data goes to negative p', which is unrealistic. How can there be MP with negative p'? How can there be data in negative p' at all? Then the corrected curve is shifted to match the endpoints of the stress paths. This moves the first part of the corrected curve into a region of p' which is higher than the specimen ever actually experienced. Are the stiffnesses used here realistic for the higher p'? Then the p'o for the start of the corrected curve is used for normalization of CSR in Figure 7. This point was never experienced by the test specimen and is somewhat coincidental dependent on the criterion chosen for cropping the cyclic test results.

An alternative (and possibly preferred) way of applying the above correction is to find how many cycles the corrected curve takes to get to the end of the cropped measured data. This is more analogous to Figure 4, where for example the result at the end of three cycles undrained (measured) corresponds to 2.5 cycles isochoric (corrected), at p = 40. This is not necessarily correct because some of the upper and lower portions of the corrected curve in Figure 7 would cross the failure lines at low p', which is not possible. This actually shows a serious limitation of the corrections calculated and applied herein. For the evaluations shown in Figure 7, the isochoric curves could be considered approximately having half the number of cycles when starting at the same point and then attaining the end of the undrained (measured) curves (ignoring crossing failure lines). The blue CRS curves in Figure 7 would then be shifted horizontally to the left to half the number of cycles. The resulting red corrected curves would be very different from the red curves shown in Figure 7, being higher in the plot, positioned on either side of the lower blue curve and not converging to a more common trend. The reviewer is not saying this is correct but shows that different conclusions can be obtained with different ways of evaluating the correction.

The language is generally acceptable but could be improved in some locations. In addition, the paper should be in third person, without the use of "we" and "us". There were some German (?) words, like "und" in the last line of the first column on page 4 and "mit" in Equation 23. Figure 9 should say "grain" instead of "corn". "Isochoric" used herein is for constant volume, but this is usually for the condition of changing temperature (and thereby pressure), a thermodynamic process. Temperature is not a parameter in this paper. This reviewer has not seen this term used in geotechnical aspects before. Perhaps a better term should be used to describe constant volume of the soil particles.

Specific comments

- Introduction, last paragraph, first line. Grains penetrate into the membrane. Not to be misunderstood as penetrating through the membrane when only saying "penetrate the membrane".
- Notation. From the definition p = (?a + 2?r)/3, it is understood that primes are not used to define effective stress herein, but rather that "tot" is used to define total stress by ?tot = ? + u. Why then is prime (') used in p'o on the vertical axis of Fuigure 2, p' on the horizontal axis of Figures 5 and 7, and for ?' in Appendix A?
- Figures 2, 5 and 7. With test specimen h = d, it is presumed that frictionless end platens were used. Say this. If not, then test specimen h should have been about 2d.
- Equation 2. Give units for parameters, like stress for kMP. Give numerical examples of kMP or membrane penetration volume change. Surely, this volume change is small for the soil tested herein.
- Equation 3. Define parameters not given in Notation, like Km, n, Ka, Kw and Ks.
- Equation 4 and 5. These equations have the same kMP. Is kMP for undrained stress change (Equation 5) the same as for drained stress change (Equation 4)? For the former it would seem the kMP is influenced by the stiffness of the soil in that MP volume change is the negative of the soil volume change. For the latter it would seem that the MP volume change (membrane penetrating into voids on the specimen surface) is independent of soil stiffness. Should kMP be much higher for the undrained case? Could this possibly explain (at least some of) the surprisingly enormous undrained correction results herein?
- Page 4, last paragraph before 3. It seems the authors assume that membrane penetration for undrained loading is completely reversible for undrained unloading (relaxation). This may not be the case if, for example, there is some slipping between the membrane and the grains as the membrane penetrates into voids on the specimen surface. With

slip the MP effect is not elastically reversible. Could this also possibly explain the surprisingly enormous undrained relaxation corrections herein?

- Equation 7. Notation defines q strain to be the radial strain subtracted from the axial strain. This is a shear distortion strain. Where does the 2/3 come from in Equation 7? Explain this.
- Equation 29. How can this equation be solved? It seems both $\varepsilon^*_{\text{vol}}$ and ε_{vol} are unknown. It is not obvious that ϵ_{vol} is the negative of the MP volume change any more when you have added the softening u/K_m term in the equation.
- Appendix A. State what the relative densities of the test specimens were. Why are $\sigma_1'^{\text{av}}$ and $\sigma_1'^{\text{ampl}}$ referred to when they are not used anywhere else (or given numbers)? What is the volume change measured by a differential pressure transducer in these undrained tests? Is this during consolidation?
- Figure 8. How can the external load cell have no friction if connected to a piston going into the cell?

Author Response

We are really grateful for the valuable comments. They have considerably improved the original manuscript. Changes in the manuscript have been emphasized with a blue color.

Response to Reviewer 1

Fewer acronyms, a little more explanatory text and more variables appearing in the 'essential' notation would help. I also thought that the use of "isochoric" was not so helpful (I had to look it up!). The use of the word 'signal' was a bit odd for what were test data.

We agree with everything: there are less acronyms and more explanations in the text by now. All remaining abbreviations are explained. The adjective "isochoric" is quite convenient in the context of the MP, however. A short explanation is added. The word "signal" is replaced by "data".

As an experimentalist, I thought that the authors might have mentioned a bit more the issues that more typically affect us. I suppose that the comment that the undrained shear strength would be overestimated was quite correct, but was maybe a bit overstated; I guess that it is not often that we would design with an undrained shear strength in a soil that has a grain size large enough to suffer from membrane penetration issues.

Indeed, we had an error in the material parameters (wrong h_B caused twice too large stiffness) and this error magnified the correction. Thank you very much for this valuable hint.

Unfortunately the example they gave in Section 5 was not that clearly explained and the authors could do more to improve accessibility, maybe adding some words to explain the meaning of the negative corrected p' values and the meaning of the shift of the stress path.

We have added more explanations and commented on the determination of the material parameters. As an alternative to the Mathematica script we offer the Matlab script as well.

It might also have helped to show what does go wrong if corrections are made to the data at very low p' values.

We discuss the limit of h at $p \to 0$ showing that the combination of the Bauer's stiffness $K(p)$ combined with the Nicholson formula for k_{MP} must eventually lead to $h \to \infty$.

Has the effect of the membrane stiffness on the radial stress been accounted for?

It is indeed a good opportunity to estimate this stress here. We have added a short derivation based on the boiler formula in the footnote on page 3 showing that the effect is negligible.

I suppose that for drained loading we would use only local strain measurements or for shearing we might restrict ourselves to a conventional constant effective cell pressure stress paths, and hope that the membrane pressure component would not alter and so could be ignored.

We agree, the MP is of importance for the drained tests only tests with the pore water system used for the volume measurement. If the radial stress σ_r is constant the MP-effect vanishes.

I guess that is not quite true as the void ratio changes and the membrane span between particles alters. Can the authors comment on the validity of this approach?

In our paper the MP effect is assumed proportional to changes in σ_r. We agree that the membrane may cause other problems (being a prestressed elastic element of the system) but we would not describe all of them as the MP.

Fig.1 typo site not side?

We do mean the left-hand "side" here.

Response to Reviewer 2

General comments - Reviewer 2

The next to last paragraph in the Introduction states that the MP is known for affecting the triaxial data for soils with $d_{20} > 1$ mm. The d_{20} of the material tested herein (Appendix A) was less than 1 mm (less than 0.54 mm, perhaps closer to 0.4 mm), yet the corrections shown were **enormous**. Why?

Indeed, we had an error in the granular stiffness h_B. It was evaluated during isotropic unloading and the resulting stiffness was unnecessarily increased. This issue is explained in the footnote on page 7. This too large stiffness magnified the MP correction quite strongly. We are really obliged for this hint. We have also mentioned this issue in the acknowledgements.

There are more parameters that would affect MP besides d_{20}, like gradation (fines content) of the material, membrane elasticity, membrane thickness, etc. None of this is mentioned or considered herein.

We inserted this remark into the text, thank you. Our MP parameters have been determined directly, comparing the results from samples of different diameters. We neither relied on the Nicholson's correlation of the MP with d_{20} nor had enough data to improve this correlation. We agree, however, that such research would be of practical importance.

This reviewer does not believe the magnitude of the correction shown in this paper. The sand material tested was not particularly coarse, but the corrections were enormous. This paper essentially precludes all cyclic triaxial testing on sand, which cannot be the case. More verification is required to show these enormous corrections being realistic. This is the most important comment by this reviewer. This could be done, for example, by repeating some of the tests presented with thicker or double or triple membranes, where membrane penetration effects would be less.

Indeed, we had an error in the granular stiffness h_B and this error magnified the correction quite strongly. We are really obliged for this hint. We have also mentioned this issue in the acknowledgements.

This reviewer does not feel the method used for applying the correction in the paper is correct or properly/adequately justified. The corrected data goes to negative p', which is unrealistic. How can there be MP with negative p'? How can there be data in negative p' at all? Then the corrected curve is shifted to match the endpoints of the stress paths. This moves the first part of the corrected curve into a region of p' which is higher than the specimen ever actually experienced. Are the stiffnesses used here realistic for the higher p'? Then the p'_0 for the start of the corrected curve is used for normalization of CSR in Figure 7. This point was never experienced by the test specimen and is somewhat coincidental dependent on the criterion chosen for cropping the cyclic test results.

Of course, p cannot be negative and the MP-corrected stress path should be cropped after a smaller number of cycles N_f. The question is, how to account for this reduction of N_f in CRR $= \frac{1}{2} q^{\text{ampl}} / p_0$. By definition, the stress ratio CRR $= \frac{1}{2} q^{\text{ampl}} / p_0$ leads to liquefaction after exactly $N_f = 10$ cycles.

An alternative (and possibly preferred) way of applying the above correction is to find how many cycles the corrected curve takes to get to the end of the cropped measured data. This is more analogous to Figure 4, where for example the result at the end of three cycles undrained (measured) corresponds to 2.5 cycles isochoric (corrected), at $p = 40$.

We agree, one possible method could be

1. conduct several tests with different values of $\frac{1}{2}q^{\text{ampl}}/p_0$
2. correct all their N_f values
3. interpolate between corrected N_f values for CRR = $\frac{1}{2}q^{\text{ampl}}/p_0$ at exactly $N_f = 10$

The proposed method is the fictitious increase of p_0 shifting the corrected stress path so that the final stress of the corrected and the original path coincide. An advantage of the latter is that it can be applied a posteriori to a single CRR test with $N_f = 10$ removing the MP-error. A disadvantage is the unknown true behaviour of the sample in the vicinity of increased p_0. A discussion of this methodology is added in Section 6.

This is not necessarily correct because some of the upper and lower portions of the corrected curve in Figure 7 would cross the failure lines at low p', which is not possible. This actually shows a serious limitation of the corrections calculated and applied herein.

The portion of the corrected stress path lying beyond the failure is completely fictitious.
We have mentioned this in Section 6 so that the readers should not be confused.

For the evaluations shown in Figure 7, the isochoric curves could be considered approximately having half the number of cycles when starting at the same point and then attaining the end of the undrained (measured) curves (ignoring crossing failure lines). The blue CRS curves in Figure 7 would then be shifted horizontally to the left to half the number of cycles. The resulting red corrected curves would be very different from the red curves shown in Figure 7, being higher in the plot, positioned on either side of the lower blue curve and not converging to a more common trend. The reviewer is not saying this is correct but shows that different conclusions can be obtained with different ways of evaluating the correction.

This is a good idea, thank you. We have addressed this point in Section 6 “Discussion”

The language is generally acceptable but could be improved in some locations. In addition, the paper should be in third person, without the use of "we" and "us". There were some German (?) words, like "und" in the last line of the first column on page 4 and "mit" in Equation 23. Figure 9 should say "grain" instead of "corn".

Everything is changed as desired: there is not a single "we" or "us" or "our" left!

“Isochoric” used herein is for constant volume, but this is usually for the condition of changing temperature (and thereby pressure), a thermodynamic process. Temperature is not a parameter in this paper. This reviewer has not seen this term used in geotechnical aspects before. Perhaps a better term should be used to describe constant volume of the soil particles.

We have explained this term, where it was used for the first time. The notion ”isochoric” is short and it emphasizes the difference undrained ↔ isochoric = constant-volume, essential in this paper.

Specific comments - Reviewer 2

Introduction, last paragraph, first line. Grains penetrate into the membrane. Not to be misunderstood as penetrating through the membrane when only saying ”penetrate the membrane”.

Done!

Notation. From the definition $p = (\sigma_a + 2\sigma_r)/3$, it is understood that primes are not used to define effective stress herein, but rather that "tot" is used to define total stress by $\sigma^{tot} = \sigma + u$. Why then is prime (') used in p_0' on the vertical axis of Fuigure 2, p' on the horizontal axis of Figures 5 and 7, and for σ' in Appendix A?

Indeed there is no need for prime in σ' or p'; already removed, thank you.

Figures 2, 5 and 7. With test specimen $h = d$, it is presumed that **frictionless end platens** were used. Say this. If not, then test specimen h should have been about $2d$.

In order to reduce the frictional effects each end plate was equipped with a layer of grease and a rubber disc. The system compliance was determined in preliminary tests on a steel dummy and subtracted from the measured values.

Equation 2. Give units for parameters, like stress for kMP.

All special units for empirical formulas are explicitly given.

Give numerical examples of k_{MP} or membrane penetration volume change. Surely, this volume change is small for the soil tested herein.

We mention $k_{MP} \approx 1000p$ in Appendix D, where all parameters are discussed.

Equation 3. Define parameters not given in Notation, like K_m, n, K_a, K_w and K_s.

The list of symbols has been strongly extended.

Equation 4 and 5. These equations have the same k_{MP}. Is k_{MP} for undrained stress change (Equation 5) the same as for drained stress change (Equation 4)? For the former it would seem the k_{MP} is influenced by the stiffness of the soil in that MP volume change is the negative of the soil volume change. For the latter it would seem that the MP volume change (membrane penetrating into voids on the specimen surface) is independent of soil stiffness.

k_{MP} does not depend on the stiffness of the sample. Only the roughness of the surface and the properties of the membrane influence the MP.

Should k_{MP} be much higher for the undrained case?

This is true for the MP but not for k_{MP} which is just the proportionality factor between the effective stress rate $\dot{\sigma}_r$ and the strain rate $\dot{\varepsilon}_{\text{vol}}^{MP}$. The rate of *effective* stress $\dot{\sigma}_r$ is quite small in saturated undrained tests and so is $\dot{\varepsilon}_{\text{vol}}^{MP}$. The factor k_{MP} is identical, however.

Page 4, last paragraph before 3. It seems the authors assume that membrane penetration for undrained loading is completely reversible for undrained unloading (relaxation). This may not be the case if, for example, there is some slipping between the membrane and the grains as the membrane penetrates into voids on the specimen surface. With slip the MP effect is not elastically reversible. Could this also possibly explain the surprisingly enormous undrained relaxation corrections herein?

Yes and no. Yes, because there may be indeed an irreversible portion in the MP and we neglect this fact. No, because we have found a much trivial explanation of this enormous correction, the parameter h_B was evaluated upon isotropic unloading and $K(p)$ was unnecessarily doubled. Nevertheless, it is very kind of you to help us with such hints. We really appreciate all these interesting ideas to solve the problem of the excessive MP correction.

Equation 7. Notation defines q strain to be the radial strain subtracted from the axial strain. This is a shear distortion strain. Where does the 2/3 come from in Equation 7? Explain this.

The factor 2/3 in $\dot{\varepsilon}_q = \frac{2}{3}(\dot{\varepsilon}_a - \dot{\varepsilon}_r)$ is obligatory and should have been written also in the Notation. This factor follows from the comparison of powers (work conjugated variables). We require

$$\sigma_a \dot{\varepsilon}_a + 2\sigma_r \dot{\varepsilon}_r = p\dot{\varepsilon}_{\text{vol}} + q\dot{\varepsilon}_q \tag{1}$$

with Roscoe invariants $p = \frac{1}{3}(\sigma_a + 2\sigma_r)$ and $q = \sigma_a - \sigma_r$.

Equation 29. How can this equation be solved? It seems both $\varepsilon_{\text{vol}}^*$ and ε_{vol} are unknown. It is not obvious that ϵ_{vol} is the negative of the MP volume change any more when you have added the softening u/K_m term in the equation.

The strain rate $\varepsilon_{\text{vol}}^*$ can be directly measured via the pore water system. It is the increment of volume of pore water which squeezed out from the sample. Eq (29) is general. Under undrained conditions we have the special case $\varepsilon_{\text{vol}}^* = 0$ and hence $\varepsilon_{\text{vol}} = -\dot{\sigma}_r / k_{MP} + \dot{u}/K_m$. This expression is used in Eq. (33). We have improved the text in this fragment o be better understandable.

Appendix A. State what the relative densities of the test specimens were. Why are $\sigma_1'^{\text{av}}$ and $\sigma_1'^{\text{ampl}}$ referred to when they are not used anywhere else (or given numbers)? What is the volume change measured by a differential pressure transducer in these undrained tests? Is this during consolidation?

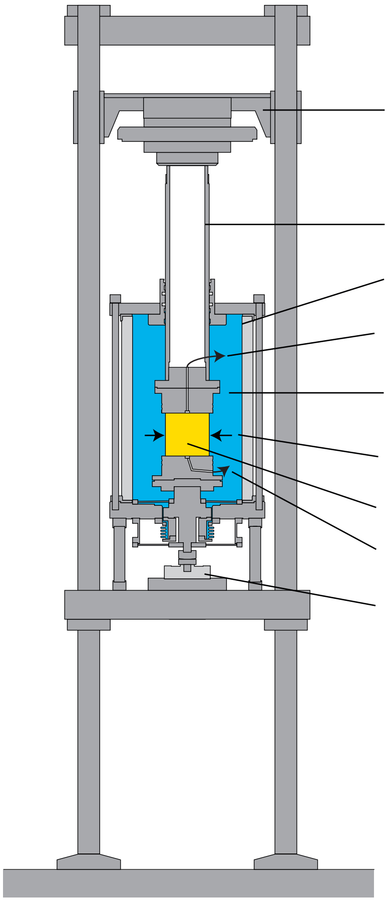
We have added some information about the relative density D_r in the current version.

Figure 8. How can the external load cell have no friction if connected to a piston going into the cell?

We use triaxial cells with a special loading mechanics. The bottom platen of the sample is moving. The frictionless loading piston introduces the vertical force via a pressure joint and a cavity filled with water onto the head plate. The idea is not new and was described by Thomas Scherzinger in his PhD thesis in 1991 p. 30.

The comment of our senior scientist and experimentalist:

Around 1985 a new generation of triaxial testing devices for long term tests with strong variations of deformation rate as well as cyclic deformation had been designed and built up at the IBF. A design goal was that these new devices should be used for various strain levels within a test: from small to high strain rates as well as cyclic strain levels. High quality load cells and pressure transducers as well as displacement transducers are necessary to achieve this purpose. The quality concerns long term stablility, high resolution, low hysteresis, small nonlinearity and uniqueness. Load cells placed inside the triaxial cell (TC) like (nowadays) the devices from MTS for rock mechanics testing are not applicable since the internal parts of the load cells (strain gages, etc.) are in contact with the isolating pressure fluid. In soil mechanics non isolating water in the triaxial cell inhibits this type of application. Load cells inside the TC for soil mechanic applications did not fit the requirements for this purpose. Only high quality load cells could fulfill the requirements. They had to be mounted out side the triaxial cell due to their requirements for mounting and the absence of water.



A general problem had to be eliminated. The pressure dependent friction and hysteresis of the connection between seal and moving piston cause unpredictable errors and for this application, they are not acceptable. Purely elastic behavior of the piston guide system is required. Figure 1 shows how this problem was tackled. See section between [to pw-system] and [load cell for σ_a *without friction]. A corrugated stainless steel bellow (BE) separates the TC under pressure and the surrounding under atmospheric pressure. The bellow is mounted between the bottom plate of the TC and the piston (PI). The PI is beam supported inside and outside of the TC. The system of BE and PI has a very low spring rate along the vertical axis and is stiff along all other axes. The required vertical deflection is mainly given by the deflection of the load cell and is in general < 0.5mm. The corresponding maximum force is about some N. The hydraulic cross-section is adapted to the diameter of the specimen. This reduces the contribution of the cell pressure to the measured vertical load. The spring rate and the influence of cell pressure are calibrated. The measured force values are corrected using these parameters and the measured cell and pore*

Review Round 2

Reviewer 2 (Anonymous)

I was happy with the authors' replies and changes.

Reviewer 3 (Anonymous)

Presentation of the paper

Presentation of the paperThis manuscript presents a procedure to take the effects of membrane penetration (MP) into account for different triaxial testing scenarios and boundary conditions in course and medium-coarse granular soils. Series of cyclic triaxial tests were performed on specimens with d=h=10cm and d=h=15cm prepared by moist tamping method. Overall, this paper is well written and structured and I have the following comments:

General

Abstract: The abstract is too short, needs to be extended to reflect the whole contents of the manuscript. I suggest rewriting the abstract with focus on what this study is about, why this study is important, include major findings, about your method, the code and verification procedure. Introduction: The introduction needs reworking. As authors have mentioned on page 2, last paragraph, MP has interesting literature and we, as a reader, want to know that literature, like the previous studies and proposed methods which take MP into account. And we want to know what are the gaps in the literature in MP? And how you are taking this and what is the contribution of this study? For example, in one of your references (Page 2), Wichtmann et al. (2019), they mentioned that the effect of MP is negligible for their material. Therefore, when is the correction for MP crucial? You may want to consider removing or shortening the less important part, (like the first paragraph in Page 2). Besides, throughout the paper, there are several statements without references (mentioned below).

- Page 2, "For reliable predictions of cyclic stability of loose deposits and their slopes, undrained cyclic triaxial tests are necessary?. Is this the only tests that could be performed for this purpose?"
- Please provide a reference for equation 1 and also for "If Nf = 10 undrained triaxial cycles with the stress amplitude qampl are applied starting from the isotropic effective pressure p0 then liquefaction is expected".
- Page 2, first column, last paragraph, please arrange the references in chronological order.
- Page 2, Please provide a reference for "During the evaluation of the CRR a difference between undrained and isochoric conditions due to the MP may be of importance". "For fine materials, the influence of the MP is negligible but for coarse-grained materials it is essential to purify the data from the MP-errors. The MP is known for affecting the triaxial data for soils with d_{20} > 1 mm."
- Page 2, If there are, please provide a reference for: "The isochoric conditions may be also spoiled by the compressibility of the pore fluid due to poor saturation."
- Page 2, Please check the punctuation: "This problem is not discussed here, however. Poor saturation can be easily detected."
- Figure 2, how authors relate the different CRRs to only MP, in which the Dr varies from 0.38 to 0.45.
- For pore water pressure and air pressure, I suggest using two different variables, like uw and ua.
- Figure 2, caption: please consider removing marker from caption as you have them on the plot.
- Page 3, "The true strain rate is defined on the relative volume change of the envelope (a cylinder or a barrel), that encompasses all grains." Please consider rewriting this sentence to be more understandable.
- Page 3, "A closer look at the boundary surface reveals that grains are moved slightly outwards and pore water is pressed slightly inwards the initial boundary surface." How the authors made this closer look?

- I suggest providing a photo from your specimen at the consolidation stage to give an idea about MP in your tests.
- Figure 3, the caption is too long. Please consider shortening the caption, but explain the text.
- Page 4, Please provide a reference for: “During an undrained triaxial compression test, q > 0, loosely compacted samples show relaxation p’ < 0, due to restricted contractancy.”
- Page 4, How authors verify this: “This effect is better known as the pore pressure build-up, although the increase u? is actually a consequence of the relaxation and not vice versa.”
- Page 5, Please make sure that Figure 4 is mentioned in the text.
- Page 6, It seems that the title for 4.4 should be “correction of drained? instead of ?correction of undrained”.
- Page 6 (and the rest of the text), we do not have CRR tests; we have a CRR parameter which can be determined through a cyclic test
- Page 7, Please provide a reference for “The conventional definition of cyclic liquefaction is based on the strain amplitude which is required to surpass $\varepsilon_q^{\text{ampl}}$ = 10% (some authors propose even $\varepsilon_q^{\text{ampl}}$ = 15%).”
- This criterion could not be a suitable definition for the liquefaction. This criterion could be considered as “cyclic failure criteria” which does not necessarily mean liquefaction.
- It is common to use Excess pore water pressure ratio (ru) to define liquefaction. Why authors do not use this parameter to define liquefaction.
- Page 7, I am not sure what authors mean by saying “However, in order to reach $\varepsilon_q^{\text{ampl}}$ = 10% several cycles of similarly large amplitude need to be applied.”
- Please update your list of symobls for equation 39.
- Please provide a reference for equation 39.
- Page 9, Please provide the loading frequency as you are performing stress control tests: “The cyclic loading was realized by means of a spindle gear system and a frequency of 0.005 mm/min.” Please provide the loading frequency as you are performing stress control tests.
- Page9, Appendix B, please check if you are using a correct symbol for volumetric strain (i.e. ε^{MP})
- Page9, Appendix B, first column, last paragraph, please add size between “grain distribution”

Author Response

Response to Reviewer 3

Presentation of the paper

The abstract is too short, needs to be extended to reflect the whole contents of the manuscript. I suggest rewriting the abstract with focus on what this study is about, why this study is important, include major findings, about your method, the code and verification procedure.

The abstract is twice longer by now and the required information is supplied.

General critics - Reviewer

The introduction needs reworking. As authors have mentioned on page 2, last paragraph, MP has interesting literature and we, as a reader, want to know that literature, like the previous studies and proposed methods which take MP into account. And we want to know what are the gaps in the literature in MP? And how you are taking this and what is the contribution of this study?

The following text has been added:

Unfortunately, most authors deal with empirical formulas for the MP. They are based on fragmentary data and can hardly be considered as proven in the sense of statistics. Correlations of the MP with the grain size distribution alone and without asperity seem disputable. Only few authors **???** *deal with the accumulation of the MP during cyclic tests. The proposed overall MP-factors can be applied to the end results only. To best knowledge of the authors no incremental procedure to eliminate the MP-effect from the measured paths has been proposed as yet. This article tries to fill this gap providing a general approach to the MP based on the incrementally linear constitutive formalism. Nevertheless the empirical formulas can still enter the calculation via the parameter* k_{MP}*, see Eq. (2).*

For example, in one of your references (Page 2), Wichtmann et al. (2019), they mentioned that the effect of MP is negligible for their material. Therefore, when the correction for MP is crucial?

It is a really good question and it would be good to know the answer judging by the grain size distribution only. Wichtmann's investigations were carried out on a different material, which has different CRR values and an fine contents F_C = 1,5 %. The membrane used was obtained from a different manufacturer. Unfortunately we cannot give a better explanation.

You may want to consider removing or shortening the less important part, (like the first paragraph in Page 2). Besides, throughout the paper, there are several states without references (mentioned below).

We have removed the first 24 lines of the introduction as suggested.

Page 2, "For reliable predictions of cyclic stability of loose deposits and their slopes, undrained cyclic triaxial tests are necessary". Is this the only tests that could be performed for this purpose?

One can perform a simple shear (DSS) a true triaxial test, torsional test and perhaps some in situ investigation including CPT at best with piezocone etc. However we restrict ourselves to triaxial data here. We have pointed it out in the title.

Please provide a reference for equation 1 and also for "If N_f = 10 undrained triaxial cycles with the stress amplitude q^{ampl} are applied starting from the isotropic effective pressure p_0 then liquefaction is expected".

We cite Vaid 1990 as the author of the definition (1).

Page 2, first column, last paragraph, please arrange the references in chronological order.

As we attempted to change the sequence of references the LaTeX-style-file of OPEN GEOMECHANICS wrote them back in the alphabetic order. Perhaps they want it so in this journal. We to leave it up to the editor. Editor: Alphabetic order corresponds to the style adopted for the journal.

Page 2, Please provide a reference for "During the evaluation of the CRR a difference between undrained and isochoric conditions due to the MP may be of importance". This has been shown in Section 5. We have mentioned this in the text by now.

"For fine materials, the influence of the MP is negligible but for coarse-grained materials it is essential to purify the data from the MP-errors. The MP is known for affecting the triaxial data for soils with d_{20} > 1 mm."

Page 2, If there are, please provide a reference for: "The isochoric conditions may be also spoiled by the compressibility of the pore fluid due to poor saturation."

Under undrained conditions the deformation of the pore fluid allows for the deformation of the skeleton. At least Skempton must have been aware of this, as he proposed the parameter B. We cited Skempton at the end of reasoning on this subject.

Page 2, Please check the punctuation: "This problem is not discussed here, however. Poor saturation can be easily detected."

Indeed, this phrase sounded strange. We have removed the first sentence from the text completely.

Figure 2, how authors relate the different CRRs to only MP, in which the D_r varies from 0.38 to 0.45.

There is indeed a strong scatter of the results and one cannot attribute it to the MP alone. However the Fig. shows no clear (monotonic) dependence on D_r.

For pore water pressure and air pressure, I suggest using two different variables, like u_w and u_a.

The difference between the *absolute* water pressure u_w and the atmospheric air pressure u_{atm} is denoted as u and this u is termed "water pressure" in soil mechanics. It would be wrong to rename u to u_w.

Figure 2, caption: please consider removing marker from caption as you have them on the plot.

There is indeed some redundancy. We have removed the markers if they could irritate the readers.

Page 3, “The true strain rate is defined on the relative volume change of the envelope (a cylinder or a barrel), that encompasses all grains.” Please consider rewriting this sentence to be more understandable.

We hope it is more clear by now.

Page3, “A closer look at the boundary surface reveals that grains are moved slightly outwards and pore water is pressed slightly inwards the initial boundary surface.” How the authors made this closer look?

It is evident in Fig. 3a that shows the magnified grains and the roughened membrane. The increased wrinkles of the membrane means that that grains are moved to the left and pore water moved to the right with respect to the boundary seen as a straight vertical line.

I suggest providing a photo from your specimen at the consolidation stage to give an idea about MP in your tests.
Unfortunately the membrane is not transparent and one cannot see the grains analogously to Fig. 3a.

Figure 3, the caption is too long. Please consider shortening the caption, but explain the text. There is indeed some redundancy comparing the caption and the text. We have removed the last 4 lines of this caption.

Page 4, Please provide a reference for: “During an undrained triaxial compression test, $\dot{q} > 0$, loosely compacted samples show relaxation $\dot{p} < 0$, due to restricted contractancy.”

A reference to Skempton’s parameter A has been added.

Page 4, How authors verify this: “This effect is better known as the pore pressure build-up, although the increase u is actually a consequence of the relaxation and not vice versa.”

One can verify it using a simple shear test on a dry sample keeping the constant volume. A footnote has been added.

Page 5, Please make sure that Figure 4 is mentioned in the text.

done, thank you.

Page 6, It seems that the title for 4.4 should be “correction of drained” instead of “correction of undrained”.

This section pertains to undrained but not saturated and hence not isochoric samples.

Page 6 (and the rest of the text), we do not have CRR tests; we have CRR parameter which can be determined through cyclic

It is a jargon ”undrained tests” for tests of undrained samples and ”undrained cohesion” for cohesion of undrained samples. We have changed ”undrained CRR tests” to ”undrained cyclic tests” which is an abbreviation for tests of samples under cyclic loading and under undrained conditions. We hope that it is clear for the readers of Open Geomechanics.

Page 7, Please provide a reference for “The conventional definition of cyclic liquefaction is based on the strain amplitude which is required to surpass " $\varepsilon_q^{\mathrm{ampl}} = 10$ % (some authors propose even $\varepsilon_q^{\mathrm{ampl}} = 15$ %).”

The following authors use identical definition: Lee (1969), Towhata (2008) and Wichtmann (2019) and we have added this information in the text.

This criterion could not be a suitable definition for the liquefaction. This criterion could be considered as “cyclic failure criteria” which does not necessarily mean liquefaction.

There are two kinds of cyclic failure: cyclic mobility (for symmetric cycles, $q^{\mathrm{av}} \approx 0$) and progressive failure for (for asymmetric cycles, $q^{\mathrm{av}} \neq 0$). For symmetric cycles a small p in comparison to q^{ampl} means that a butterfly-like cycles will soon occur. The correction of the MP during large butterfly-like cycles is imprecise and therefore we have excluded these cycles from the evaluation.

It is common to use Excess pore water pressure ratio (r_u) to define liquefaction. Why authors do not use this parameter to define liquefaction.

As already mentioned the build up of pore pressure is a secondary phenomenon to the relaxation of the effective stress. A liquefaction is a general notion. It is also possible in dry granulate materials. One may think of wheat in a silo subject to a strong air stream from below.

Page 7, I am not sure what authors mean by saying "However, in order to reach $\varepsilon_q^{ampl} = 10$ % several cycles of similarly large amplitude need to be applied."

Hopefully, it is more clear now.

Please update your list of symbols for equation 39.

done

Please provide a reference for equation 39.

This is our proposal to redefine the point of liquefaction. We have made it clear in the text.

Page 9, Please provide the loading frequency as you are performing stress control tests: "The cyclic loading was realized by means of a spindle gear system and a frequency of 0.005 mm/min." Please provide the loading frequency as you are performing stress control tests.

Sorry for this. We have changed frequency → rate.

Page 9, Appendix B, please check if you are using a correct symbol for volumetric strain (i.e. ε_{MP})

done, thank you

Page 9, Appendix B, first column, last paragraph, please add size between "grain distribution"

done, thank you.

Editorial Decision

At the end of Review Round 2, the managing Editor has decided to accept the revised version of the manuscript for publication.