OPEN GEOMECHANICS

Review History for "Introductory consideration supporting the idea of the release of elastic waves in hysteretic soil"

Piotr Kowalczyk and Alessandro Gajo *2023*

Summary

This paper was sent to three reviewers: Prof Saif Alzabeebee (Reviewer 1) one anonymous reviewer (Reviewer 2) and Dr Max Wiebicke, The University of Sydney (Reviewer 3). The reviewers remained anonymous during the entire review process. After the reviewing process was complete, Reviewer 3 agreed to disclose their identity. The paper went through a single round of review. In this Review Round, all three reviewers provided a series of comments for the authors. Reviewer 1 recommended that revisions were required while Reviewer 2 recommended that the authors rework the manuscript and resubmit it for review. In Review Round 2, both Reviewers recommended the manuscript for publication with no reservations. At this stage the managing Editor decided to accept the manuscript for publication.

Review Round 1

Reviewer 1 (Saif Alzabeebee)

The followings should be addressed:

- Some grammar issues need to be fixed. Such as Line 260 (is adopted). Thus, please proofread the paper.
- Regarding the soil constitutive models, the authors need to explain in more details to show how the stiffness dependency on stress level is modelled. Also, validation of these soil models is needed to provide confidence in the results.
- Lines 320-321: how the calibration has been done. Details should be added.
- Section 2.2: how the authors derived the soil parameters to enable a fair comparison? Please discuss.
- Also, list of properties of the soil models used in the analysis for Section 2.2 should be added in a table.
- Properties of the soil models used in the analyses of Section 3 should also be added in a table.

Reviewer 2 (Anonymous)

This manuscript presents a new reason for the presence of high frequency content in wave propagation through nonlinear hysteretic material like soil. The authors use simple numerical experiments (1D wave propagation through soil with/without reflections) to conclusively demonstrate the generation of elastic waves upon unloading. The authors demonstrate that these elastic waves add to the high frequency content from the distortion of input sine waves. The authors have also compared the results with experiemental results for wave propagation through dry sand available from past research. Overall, the topic is interesting, the analysis presented is detailed and the paper is well written.

I have a few minor suggestions regarding the ordering of content in the manuscript, that may help the readers under the manuscript better.

1. While the discussion presented in section 3.2 is quite clear and the presence of the unloading elastic waves are noticable due to the trapping of energy in the system, the same is not very clear in the semi-infinite soil column. Statements such as lines 459-462 seem very vague. These do become lot more convincing from the discussion of arrival times presented in Fig. 12 and 17. So, I would recommend either removing the part about semi-infinite soil column or moving it after the discussion on soil columns with 0.8 m height.
2. In section 3.2, first a linear elastic response is presented followed by increasing levels of nonlinear response. However, then in fig 15 and 16 the response for a low amplitude wave motion is presented, which is very close to the linear response. I would suggest moving this discussion before fig. 7 for continuity.
3. In fig 9., the stress-strain behaviour at the top of the soil column looks very close to a linear elastic response. Some discussion on why hysteretic behaviour is much smaller at the top would be helpful. Does this also imply that most of the unloading elastic waves are generated closer to the base of the soil column? In other words, does the thickness of the hysteresis loops have any correlation to the ampltitude of unloading elastic waves?

Reviewer 3 (Max Wiebicke)

This manuscript attempts to explain high frequency oscillation motion that is observed in dynamic small scale experiments and numerical simulations when soil is loaded by harmonic motion. The authors explain these oscillations with elastic waves in the soil that are released upon unloading. They carry out different numerical analysis on semi-infinite and finite soil columns that are subject to sinusoidal input motions with simple constitutive models to introduce this concept. Furthermore, they compare their analysis with two different experiments on soil columns as well as an experiments on a group of piles.

The paper is written very extensively: the concept is explained in detail and so are the numerical simulations and the comparison with the experimental works from the literature. To my feeling, the way the text is structured does lead to a limited readability. The paragraphs as well as the individual sentences are very long and should be shortened (split): e.g. page 8 consists of only 2 paragraphs, of which the second is not finished. There are some sentences spanning 10 lines, which makes the information much more nested and complicated to unpick than needed.

general comments:

- could you please define the steady state for your problem in the beginning of the paper? I had trouble understanding a steady state for transient loading, but found your explanation in lines 1130 ff. Please also define in coda part as the part without harmonic input motion in lines 524 ff.
- you say (e.g. in the abstract or in lines 793ff) that the elastic waves are release due to soil inherent non-linearity and irreversibility in a form of material hysteresis. Yet, you observed them also with the linear elastic model in the first simulation, which is linear and reversible and cannot show material hysteresis. I might have overseen a point here, but could you please elaborate that?
- you also always refer to the elastic waves as unloading waves. Are they only released upon unloading or at every load reversal. Assuming you define unloading as $\dot{\gamma} > 0$, they would not be released upon switches to $\dot{\gamma} > 0$. I understand Figure 4 and the following, such that these elastic waves are released at every load reversal, but I might be misinterpreting these plots.
- Fig 10. The comparison with the experiments of Durante: when the loading from the bottom stops, there is still movement at the bottom plate in the experiment (at least I guess, the resolution is not great) but not in the simulation. is there movement in the experiment and if so, why did you decide to omit it in the simulation?

- You claim, that a drawback of the comparison with the experimental data from [Durante, 2015] for the Tolmezzo earthquake input is that you only had the filtered results. However, you thank Durante in the Acknowlegdements for the raw results on the pile test. Could you not get the raw results for this tests as well?
- Could you add some sort of brief summary of what you learned from the simulations with different amplitudes/frequencies? Could be a table for example. It is hard to keep track in light of the detailed analysis, especially as a ready that possibly only scans the document once.

specific comments:

- lines 352 ff: you mentions multiple models that could also capture these elastic waves when they show a hysteretic behaviour upon load reversal. that is in fact not true for many hypoplastic models (also elasto-plastic ones), unless they get some additional concept, e.g. intergranular strain. The von Wolffersdorff model for example is only hysteretic when you cross 0 shear stress, but not if you stay at positive shear stresses. so it is not hysteretic for load-unload cycles in general.
- Fig. 1 and 2. please plot the sinusoidal input functions for users, that want to recreate your results. You could also give the analytical expression.
- Fig 3: legend of the figure: emp[h]irical
- lines 537 ff: "apparently much less expected response". Please elaborate what you expected or omit this part.
- Figures of the spectral responses. Consider using different line width, styles, etc to increase the visibility of the results. It is really hard to inspect the graphs in some figures.
- lines 682 ff. I cannot see a unloading-reloading loop in Figure 13 c). This is ratcheting as experienced for example by hypoplastic models with intergranular strain. (see Niemunis and Herle, 1997)

Author Response

We thank the Reviewers for their comments and remarks which allowed us to clarify and improve the quality of our work. All the answers to the comments are given below in [black] colour. Similarly, the changes in the revised manuscript are shown in blue colour. We hope these changes address successfully the comments of the reviewers.

Reviewer 1

The followings should be addressed: 1. Some grammar issues need to be fixed. Such as Line 260 (is adopted). Thus, please proofread the paper.

Authors' reply: The lines 228-229 in the revised manuscript have been updated now. Moreover, the manuscript has been proofread and more corrections have been introduced, including those regarding improving the general readability of the manuscript as requested by the Reviewer E.

2. Regarding the soil constitutive models, the authors need to explain in more details to show how the stiffness dependency on stress level is modelled. Also, validation of these soil models is needed to provide confidence in the results.

Authors' reply: In the simplified model (equation 1 in the revised manuscript), the stiffness dependency on the stress level has been modelled by simple assumption of modifying the maximum allowable shear stress along changing depth as per equation 2 in the revised manuscript. Note that the equation 2 uses the actual depth rather than the stress level (for example mean effective stress p'). Nevertheless, the modification of the maximum allowable shear stress $\tau_{\lim}$ as a function of the square root of depth can be considered equivalent (in terms of the evaluated initial G_0 shear stiffness) to the common empirical formulas (for example Hardin and Drnevich, 1972) expressed as a function of a square root of mean effective stress. In more detail, if equation 1 (in the revised manuscript) is rearranged to the form:

$$\frac{\dot{\tau}}{\dot{\gamma}} = \frac{(\tau_{\lim}(d) - \tau)^2}{2 \cdot B \cdot \tau_{\lim}(d)}, \text{ where } \tau_{\lim}(d) = \frac{\sqrt{d}}{\sqrt{d_{\max}}} \cdot \tau_{\lim}$$

The soil initial shear stiffness $G_0(d)$ with changing depth of between 0 and 0.8m can be calculated when substituting $\tau = 0$, B=0.00016, τ_{lim}=6000Pa, $d_{\text{max}} = 0.8$m. The initial G_0 profile has been plotted in Figure 2a in the revised manuscript where it is compared with the empirical formula (Hardin and Drnevich, 1972) in the following form:

$$G_0 = \frac{3230 \cdot (2.97 - e)^2 \cdot \sqrt{p'}}{(1+e)}$$

With the input values being: e=0.9, p' [in kPa] computed at various depths from the assumptions of ρ=1332 kg/m3 , K_0=0.5 as evaluated in the experiment (Durante, 2015). Note that the details of the calibration and validation of the simplified model and the advanced elastoplastic model are shown in Appendix A and in Appendix C, respectively. The authors have updated the manuscript in lines 271 to 275 to justify the chosen approach regarding stiffness dependency on the depth level for the simplified model (eq. 1). Thank you for requesting this clarification.

3. Lines 320-321: how the calibration has been done. Details should be added.

Authors' reply: The simplified model has been calibrated to reflect in an approximate manner: 1) realistic initial G_0 profile (i.e. as per Hardin and Drnevich, 1972) and thus reliable evaluation of elastic wave velocity and the soil natural frequencies in the numerical studies of soil in a flexible soil container, and 2) realistic shear stiffness degradation G/G_0 in simple shear deformation, i.e. such as is expected to be dominant in flexible soil containers under horizontal shaking as per Dietz & Muir Wood (2007). Note that due to the simplified formulation of the constitutive model (eq. 1 in the revised manuscript) the actual G_0 changes with the size of the hysteresis, thus our calibration method can be considered valid for loading cases resulting in a relatively narrow hysteresis. More details of the chosen calibration method have been now added to the revised manuscript in lines 283-296 of the revised manuscript and in the added new Appendix (See Appendix A in the revised manuscript).

4. Section 2.2: how the authors derived the soil parameters to enable a fair comparison? Please discuss.

Authors' reply: Regarding the methodology on deriving soil parameters, we have updated that now in Section 2.1.1 in lines 283-296 to discuss that in more details. Moreover, to address the difference in the relative density between the experimental works of Durante (2015) (Dr=0.25-0.4) and Dar (Dr=0.75) described in Section 2.2, the calibration of the constitutive model (eq. 1) used for the analysis of the single case of Dar's experiment (Fig. 13 in the revised manuscript) has been updated to account for denser sand. To this aim, the model parameters where set to B=0.0002, τ_{lim}=15000Pa ensuring the expected increased stiffness characteristics of dense sand. Further details of the calibration of the constitutive model defined by equation (1) have been shown in Appendix A.

5. Also, list of properties of the soil models used in the analysis for Section 2.2 should be added in a table.

Authors' reply: We have added Table 1 with the model parameters in the revised manuscript (see lines 423- 425 in the revised manuscript).

6. Properties of the soil models used in the analyses of Section 3 should also be added in a table.

Authors' reply: The properties of the soil models used in the analyses in Section 3 have been added in Table 1 for the simplified soil constitutive model, and in Table C.1 in Appendix C for the advanced soil constitutive model.

Reviewer 2

This manuscript presents a new reason for the presence of high frequency content in wave propagation through nonlinear hysteretic material like soil. The authors use simple numerical experiments (1D wave propagation through soil with/without reflections) to conclusively demonstrate the generation of elastic waves upon unloading. The authors demonstrate that these elastic waves add to the high frequency content from the distortion of input sine waves. The authors have also compared the results with experiemental results for wave propagation through dry sand available from past research. Overall, the topic is interesting, the analysis presented is detailed and the paper is well written.

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due to the trapping of energy in the system, the same is not very clear in the semi-infinite soil column. Statements such as lines 459-462 seem very vague. These do become lot more convincing from the discussion of arrival times presented in Fig. 12 and 17. So, I would recommend either removing the part about semi-infinite soil column or moving it after the discussion on soil columns with 0.8 m height.

Authors' reply: In fact, in the numerical study of the semi-infinite soil column the soil elastic waves cannot be observed since no wave reflections are allowed. We introduced this part of our numerical studies in the previous version of the manuscript to show a short introduction on how unloading waves propagate in a column described by a hysteretic constitutive model. Namely to show their erasing effect on the shear strain resulting in strain discontinuity, with some numerical (very high frequency) oscillations occurring in the finite element computations due to this strain discontinuity. Both those observations can also be seen in the studies of the finite soil column, however in this case those effects overlap with the release of soil elastic waves. For this reason, we introduced in the previous version of the manuscript a study on the semi-infinite column to separate all those observations from each other and gradually introduce those concepts. Nevertheless, the presence of this part of the manuscript has been reconsidered now and as suggested by the Reviewer removed from the revised manuscript as probably not essential when presenting the idea of soil-released elastic waves.

2. In section 3.2, first a linear elastic response is presented followed by increasing levels of nonlinear response. However, then in fig 15 and 16 the response for a low amplitude wave motion is presented, which is very close to the linear response. I would suggest moving this discussion before fig. 7 for continuity.

Authors' reply: We agree with the Reviewer's observation that the response computed on Figures 15 and 16 in the previous version of the manuscript is very close to the linear response. These two figures have been moved now before Figure 7 in the revised manuscript which will hopefully increase the readability of the revised manuscript.

3. In fig 9., the stress-strain behaviour at the top of the soil column looks very close to a linear elastic response. Some discussion on why hysteretic behaviour is much smaller at the top would be helpful. Does this also imply that most of the unloading elastic waves are generated closer to the base of the soil column? In other words, does the thickness of the hysteresis loops have any correlation to the ampltitude of unloading elastic waves?

Authors' reply: As the Reviewer noticed the hysteretic behaviour is much smaller at the top than at the base of the soil column. The top of the column is a 'free end' where the boundary condition is that the stress and strain are zero. Note that in our manuscript we plotted the results for the top of the column from the middle Gauss point of the first element at the top of the soil column, thus, where some shear strain is still induced as slightly below the actual top of the soil column. Nevertheless, it is much smaller than the shear strain at the base, thus, the response appears to be almost linear as noticed by the Reviewer. We have revised the manuscript in lines 551- 558 to justify the much smaller hysteresis at the top of the soil column. Thank you for this remark as this allows to clarify our work. Regarding the thickness of the hysteresis loops and its effect on the amplitude of the released elastic waves, we agree that there is a correlation. In fact, the thickness of the hysteresis loops throughout the soil column increases with the increasing amplitude of the input motion and this results in the increased amplitude of the released elastic waves. To make this point more explicit, we plotted stress-strain curves on Figure 8 with the same strain and stress range as the stress-strain curves on Figure 12 to allow direct comparison between the 0.137g and 0.2g input motions. One can see how the increased amplitude of elastic waves can be related to the hysteresis being wider. Note that regarding where exactly elastic waves are released in the soil column has not been shown in our work. It can be speculated that this is at the base when sudden changes in stiffness (from loading to unloading) take place on load reversals, or, alternatively, at the top when waves are reflected from the free end of the soil column. We have highlighted these observations in the summary of the free field studies in lines 787- 803 of the revised manuscript.

Reviewer 3

This manuscript attempts to explain high frequency oscillation motion that is observed in dynamic small scale experiments and numerical simulations when soil is loaded by harmonic motion. The authors explain these oscillations with elastic waves in the soil that are released upon unloading. They carry out different numerical analysis on semi-infinite and finite soil columns that are subject to sinusoidal input motions with simple constitutive models to introduce this concept. Furthermore, they compare their analysis with two different experiments on soil columns as well as an experiments on a group of piles.

The paper is written very extensively: the concept is explained in detail and so are the numerical simulations and the comparison with the experimental works from the literature. To my feeling, the way the text is structured does lead to a limited readability. The paragraphs as well as the individual sentences are very long and should be shortened (split): e.g. page 8 consists of only 2 paragraphs, of which the second is not finished. There are some sentences spanning 10 lines, which makes the information much more nested and complicated to unpick than needed.

Authors' reply: We have now revised the manuscript with the aim of simplifying the text. To this aim, we have shortened some long paragraphs, reduced the length of chosen compound nouns, split some long sentences into two shorter ones or changed them into shorter and more concise. Moreover, the text on pages 6-7 in the revised manuscript has been divided into more paragraphs. We have also shortened the title of the manuscript, added additional headings to the manuscript, and a short summary on the free field numerical studies, all of which will hopefully allow further increase in the general readability of the manuscript.

general comments:

- could you please define the steady state for your problem in the beginning of the paper? I had trouble understanding a steady state for transient loading, but found your explanation in lines 1130 ff. Please also define in coda part as the part without harmonic input motion in lines 524 ff.

Authors' reply: The definition of a sort of 'steady state' has been introduced in the first place in the manuscript where we refer to it (i.e. lines 484-487). Moreover, we have corrected the lines 458-459 of the revised manuscript to be clearer on the explanation of the coda part of the motion.

- you say (e.g. in the abstract or in lines 793ff) that the elastic waves are released due to soil inherent non-linearity and irreversibility in a form of material hysteresis. Yet, you observed them also with the linear elastic model in the first simulation, which is linear and reversible and cannot show material hysteresis. I might have overseen a point here, but could you please elaborate that?

Authors' reply: We show the introductory case with a linear elastic material to show where elastic waves are released in a simpler case. However, note that the release of elastic waves in case of a linear elastic material is of a non-repetitive pattern (i.e. the sort of 'steady state' is not reached), thus it is of less interest in this work when comparing with the experimental works in flexible soil containers where regular patterns of response are observed (i.e. ω, 3ω, 5ω etc). We continue the rest of our numerical studies with a hysteretic material, since the patterns of the computed responses are of repetitive character, i.e. ω, 3ω, 5ω, as per typical experimental data. Thus, more on the release of elastic waves in linear elastic material is not shown in this work.

- you also always refer to the elastic waves as unloading waves. Are they only released upon unloading or at every load reversal. Assuming you define unloading as $\dot{\gamma} > 0$, they would not be released upon switches to $\dot{\gamma} > 0$. I understand Figure 4 and the following, such that these elastic waves are released at every load reversal, but I might be misinterpreting these plots.

Authors' reply: In fact, we mean here that the presence of elastic waves can be observed following each of the load reversals as can be seen per the computations shown in Figure 4 and the following figures. Note however, that our numerical studies in this manuscript do not show exactly at which time moment (in the loading cycle following each load reversal) or where (which depth of the soil column) the elastic waves are released. Saying 'at every load reversal' could suggest it is at the base of the soil column when the load changes direction. We have also reconsidered using the term 'unloading elastic waves' which may possibly lead to similar conclusions, thus in the revised manuscript we refer simply to soil elastic waves released in nonlinear soil and their amplitude related to the hysteresis thickness (or in other words change from loading to unloading stiffness), with only some speculation where these waves could be released (see lines 759-803 in the revised manuscript). Thank you for this remark leading to further clarification in the revised manuscript.

- Fig 10. The comparison with the experiments of Durante: when the loading from the bottom stops, there is still movement at the bottom plate in the experiment (at least I guess, the resolution is not great) but not in the simulation. is there movement in the experiment and if so, why did you decide to omit it in the simulation?

Authors' reply: The high frequency signal recorded by accelerometers after the loading stops is due to the electric current and is not representative of the actual ongoing motion at base. For this reason, we have not included this high frequency signal in the computations. We have revised the lines 606-611 in the updated manuscript to clarify this aspect.

- You claim, that a drawback of the comparison with the experimental data from [Durante, 2015] for the Tolmezzo earthquake input is that you only had the filtered results. However, you thank Durante in the Acknowlegdements for the raw results on the pile test. Could you not get the raw results for this tests as well?

Authors' reply: To our knowledge, the raw experimental data of the recorded accelerations, provided to us in excel files was filtered out at the stage of data acquisition when obtaining the experimental measurements, thus there is no unfiltered experimental data (note that the experimental data shown on Fig. 9 and Fig. 15 in the revised manuscript is also filtered). Certainly, this aspect could be addressed in future experimental works.

- Could you add some sort of brief summary of what you learned from the simulations with different amplitudes/frequencies? Could be a table for example. It is hard to keep track in light of the detailed analysis, especially as a ready that possibly only scans the document once.

Authors' reply: To address this comment, we have introduced a summary text in lines 754-803 to ensure that the most important observations for the analysed input motions are listed there. In addition, we added headings ahead of each new input motion to make following the manuscript slightly easier.

specific comments:

- lines 352 ff: you mentions multiple models that could also capture these elastic waves when they show a hysteretic behaviour upon load reversal. that is in fact not true for many hypoplastic models (also elasto-plastic ones), unless they get some additional concept, e.g. intergranular strain. The von Wolffersdorff model for example is only hysteretic when you cross 0 shear stress, but not if you stay at positive shear stresses. so it is not hysteretic for load-unload cycles in general.

Authors' reply: In fact, from our observations and previous numerical analyses of experimental setups (Author, 2020), it is possible to see the generation of soil elastic waves also when using other soil constitutive models, e.g. the well-known Dafalias-Manzari (2004) elastoplastic model. When referring to the hypoplastic models we meant, in fact here the hypoplastic soil model (Von Wolffersdorff, 1996) with the intergranular strain (Niemunis & Herle, 1997). We have now corrected this information in lines 328-329 of the revised manuscript. Thank you for this observation.

- Fig. 1 and 2. please plot the sinusoidal input functions for users, that want to recreate your results. You could also give the analytical expression.

Authors' reply: We have now revised Fig 1 to show also the corresponding input motion used to obtain the stress-strain curves.

- Fig 3: legend of the figure: emp[h]irical

Authors' reply: We have updated the figure now. Thank you for spotting this spelling mistake.

- lines 537 ff: "apparently much less expected response". Please elaborate what you expected or omit this part.

Authors' reply: We have removed this statement from the manuscript.

- Figures of the spectral responses. Consider using different line width, styles, etc to increase the visibility of the results. It is really hard to inspect the graphs in some figures.

Authors' reply: We have revised figures of spectral responses in order to increase the visibility of the results. To this aim, blue, thick and dashed line was plotted for the spectral evaluation of the base input motions.

- lines 682 ff. I cannot see a unloading-reloading loop in Figure 13 c). This is ratcheting as experienced for example by hypoplastic models with intergranular strain. (see Niemunis and Herle, 1997)

Authors' reply: Note that the small unloading-reloading loop (shown in Fig. 12c in the revised manuscript) takes place around 0.36s time where a 'jump' in shear strains at the top of the soil column can be seen (Fig. 11b in the revised manuscript). The amplitude of the shear strain in the loop (Fig. 12c) is around 4·10-6 and it is the same in Figure 11b (at the time of 0.36s). We have revised the caption in Figure 12 in the updated manuscript to make our point clearer. Hopefully, this will convince the Reviewer and the Readers that the feature shown on Fig. 12c is an unloading-reloading loop and not ratcheting.

Editorial decision

After reading the response from the author, the managing Editor decided to accept this work for publication without further review.