Author response to review

We thank the editor and the two anonymous reviewers for their constructive comments and suggestions. We thank RC1 for the review opinion that 'I support for publication of this work' and RC2 for the review opinion that 'I suggest this paper to be published after some revisions. We believe that in addressing the issues that these reviewers have raised, the paper will be considerably improved.

In the following we respond to the reviewers' comments. To facilitate easy assessment, we colour coded our responses into neutral (blue), agreement (green), partial agreement (yellow) and disagreement (red). We hope that our responses convince the editor to ask for revisions.

RC1: 'Comment on hess-2021-359', Anonymous Referee #1, 07 Sep 2021

General comments on the methods:

The methodology is quite articulated and includes the integration of several models and mathematical formulations. It was not easy to follow the presentation of the method. For example, two models are presented for post or pre-strain response. While after reading the whole manuscript I (probably) understood the reason for this, the way the models are integrated is only explained in section 2.2.5 after the models are presented in 2.2.3-4. There is an itemized list, presenting the whole method step by step, but this comes only in section 3. I suggest presenting a flowchart presenting the general idea of the whole methodology at the beginning of section 2, to clarify the explanation and presentation of the methods.

We agree. The method outline (currently Section 3.2) will be merged with the theoretical coverage of the previously developed methods (as also requested by RC2). We will add a flow chart to aid the reader's understanding.

To me it is not intuitive to understand the meaning of a pre-strain water response. After reading the whole manuscript I understand that the authors are offering here a quantitative interpretation to a phenomenon that is not clearly understood and, in these terms, I completely support their work. However, all the discussion related to this point, should be given earlier (see e.g., section 4.2 495-505) to give the reader the possibility to properly assess and understand all the assumptions. On this note, the caption of figure 3 distinguishes confined from semi-confined, and this is related to post and pre-strain as far as I understand. This is not clear from the figure caption.

We agree and will overhaul the discussion. We will change the terminology to reflect the physics of the system: Pre and post strain referred to the observed phenomena how a well water level response occurs in relation to the strain. In reality, the phase shift is caused by vertical water leakage through a semi-permeable zone. This shifts the harmonic so that the signal appears to occur earlier. We will rename "pre-strain response" to "leaky response", whereas "post-strain response" will become "confined response". Further, we propose to move the discussion how harmonic response results are compared to existing literature to Section 2 (Theoretical background). This provides better theoretical context before the results are presented.

General comments on the results and discussion:

In general, there is little appreciation in the paper for the uncertainty associated with the estimated quantities. I suggest reporting more details about parameter estimation via curve fitting, e.g., the value assumed by residuals, RMSE or other similar error indicators, and the confidence bounds associated with the key estimated properties.

We will do our best to report the results of uncertainties and correlations of the parameter estimation in our revised manuscript. Numerical uncertainties originate from the harmonic least-squares (HALS) routine. We will also try and propagate uncertainties through the non-linear solving routines.

Following up on this comment I have a particular concern: the method prescribes the selection of pre- or post-strain model according to a phase shift evaluation, where the two models are assumed to be both possible if the shift is between -1deg and 0deg. However, I wonder if this overlap is not too restricted as, in principle, the estimation of the phase shift from observed data may be affected by a larger interval of uncertainty.

We agree that this is an issue. However, uncertainty in phase estimates predominantly affect the estimated hydraulic conductivity. This parameter is not subsequently used when further properties are estimated. Therefore, it should not be an issue. We are going to note this in our revised manuscript.

I found the discussion of the results quite interesting, particularly the fact that they seem to disclose nontrivial response of subsurface materials to EAT. These could be due to multiple factors, as extensively discussed in the paper. I have three comments on this point, which in my view should be considered in a revision:
I wonder if the observed results may be the results of some particular assumption embedded in the parameter estimation procedure. In particular, can the authors demonstrate that the proposed methodology leads to consistent results when applied to synthetic data (i.e. data numerically generated with known parameters and artificially perturbed)? This would demonstrate that the estimation method is robust in terms of parameter identification, at least when the data are consistent with the assumptions.
We are unsure what "numerically generated" refers to in this context. If the reviewer refers to the parameter estimation, we applied two different checks in our work: (1) we tested the accuracy and reliability of the signal processing using synthetically created harmonics and Gaussian noise (Schweizer et al., 2021); (2) we tested the non-linear solver for delivering a unique solution. We will note these points in the revised manuscript. If the reviewer means a numerical model of the physical subsurface processes, then this would be out of scope for the current manuscript.

Regarding the discussion of the negative Poisson ratio (section 4.4), and given the fact that the measurement sampling volume is unknown, is it possible that this result is due to boundary effects?
Yes, this is possible. However, we cannot conclusively answer this as there is a lack of knowledge in the literature. We will include this as a possible cause in our revised discussion.

The authors state that the results could be used to infer poroelastic properties to be used in civil and mining construction. However, I have the impression that some of the estimated parameters may be driven by the very specific conditions associated with EAT, and may not be portable to different conditions and loading. For instance, I wonder if some of the observed parameters may be associated with different time scales associated with material responses.
We appreciate this comment. There is a lack of general understanding of the portability of EAT derived results, including their frequency dependency. We will revise our discussion to clarify this further.

In section 4.2 the authors provide a sort of sensitivity analysis. (e.g., eq. 36-37). This is hard to follow because the discussion is only qualitative. I suggest either dropping it or expanding it. However, the paper is already dense and long and I wonder if the author really need to include this point.
We agree with the reviewer that expanding upon this would further lengthen the work. Rather, this should be addressed in a future contribution. Also, we believe it is useful that we mentioned our checks mentioned earlier. We will add a statement to the revised manuscript that this needs further work.

Other minor points:
Data Figure 3 are scarcely readable, please improve readability of the Figure.
Figure 3 will be improved according to the suggestion.

Line 405: if bounds are imposed it seems quite logical that no none of the parameters exceeded the fitting bounds. I suggest rephrasing and, as mentioned above, provide more quantitative details about the estimates.
The intent was to illustrate that the results were not biased by fitting problems. We will revise this and add more quantitative information about the estimates.

At line 648 the authors state that the model offers the advantage to rely on information on grain compressibility, available in the literature. However, at line 629 they state the opposite, i.e. that grain compressibility data are generally lacking. Please reconcile the two statements.
What we meant was that grain compressibility can be considered, if available. However, to the best of our knowledge, the only available data in the literature is from quartz. Consequently, our application was limited to using this. We will reconcile these statements to better reflect this.