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Comment on hess-2021-359

Anonymous Referee #1

Referee comment on "In situ estimation of subsurface hydro-geomechanical properties using the groundwater response to semi-diurnal Earth and atmospheric tides" by Gabriel C. Rau et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-359-RC1, 2021

This work provides a methodology to evaluate geomechanical properties of subsurface materials based on Earth and atmospheric tides (EAT). The methodology is based on the observation of head and uses models that are taken from available literature studies. The approach is demonstrated with a few numerical examples related to real case studies.

I found the paper and the proposed methodology interesting and I support publication of this work. I think that the paper is not very easy to read and I suggest some restructuring and streamlining of the presentation, ultimately leading to a number of revisions concerning the presentation of the methods and results. Detailed comments are reported below.

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.

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 undertand. This is not clear from the figure caption.

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. 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.

I found the discussion of the results quite interesting, particularly the fact that they seem to disclose non trivial 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.
- 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?
- 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.

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.

Other minor points:
Data Figure 3 are scarcely readable, please improve readability of the Figure.

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.

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.