Response to Review iteration 2 for HESS Manuscript 'hess-2021-359':

We thank the editor and the two reviewers for their constructive comments and suggestions. We have addressed all points and provide detailed answers below. We hope that these revisions allow our manuscript to be published in HESS.

Report #1

Suggestions for revision or reasons for rejection

The authors have provided a point-by-point response to my comments. In some cases the comments have resolved the comments, for example I appreciate the new terminology for leaky response which I find much easier to follow. However, I do have a few remarks mainly concerning the quality of the text and the presentation of the work. I have to add that I got confused in the review because the version with marked changes does not seem to correspond to the last manuscript version (or at least it is not fully consistent). This should be double-checked. Below a list of points that need to be addressed, for which I am referring to the file manuscript-version-3.

We tried our best to make a track changes document in the LaTeX environment, which was not easy given all the revisions that were done. We apologise for any confusion this may have caused.

It is clear now that the methodology provides a deterministic analysis leading to the calculation of flow and poro-elastic parameters from observed data and the issue of parametric uncertainty is actually not considered in the paper. In light of this comment, I find misleading the title of section 4.1 which includes the expression "parameter estimation". To me this sounds like a statistical estimation method, but this is not what is meant here. I suggest changing the title of this section. Also the text includes a discussion about uncertainties which remains unresolved and I am not sure that it makes sense to include it, as it might be misleading (as it was for me).

We have renamed the title for section 4.1 to "Influences on the quantification of properties".

In terms of discussion about uncertainties, we believe that the reviewer is referring to this sentence:

"Schweizer et al. (2021) further noted that HALS outperforms the discrete Fourier transform, but also that devising an objective error estimation for HALS is difficult, as it depends on the nature of the residuals (difference between measurement and model), and this requires further investigation."

We wish to leave this standing, as it points to the need for further research. We believe that clearly pointing out the remaining knowledge gaps and future research needs, and directions are important aspects of a good Discussion section.

Line 490: "Further research is required to test the applicability of analytical solutions based on simplified assumptions applied to real-world conditions." The authors have added this sentence to reply to one of my comments. However, I thought the point of the paper was precisely to prove the applicability of analytical solutions to real world test cases. So I find the statement confusing and not properly addressing the point. This response should be revised again in my view.

We have reformulated this as follows:

"Further research is required to independently validate results derived from passive methods that are based on simplified conceptual understanding and their analytical solutions and to test the influence of different and more complex real-world conditions, such as geological heterogeneity at different scales."

We hope that this clarifies what we mean.

Conclusions: The authors state that "The new method enables site-specific heterogeneity to be evaluated …", but I do not see how. The method gives a unique estimate of the parameters (as it is now clarified), there is no way to determine any spatial variability, and the support scale of the measurement is unknown. The parameters probably correspond to some "effective" properties, but I am puzzled when I try to imagine how these measurements can be used to characterize heterogeneous fields of aquifer properties.

We have revised the paragraph as follows:

"Our approach allows estimation of the complete hydro-geomechanical parameter space in a passive way, i.e. from monitoring records of groundwater pressure head, measured atmospheric pressure and calculated ET. The primary advantage is that all parameters are determined for the same in-situ conditions and that the estimated values therefore should be internally consistent. The new method provides hydro-geomechanical properties of the larger rock mass. This is a clear advantage to methods that require taking samples to the laboratory where replicating field conditions such as in-situ confining pressure and representative scale can be problematic. When combined with laboratory estimates on intact rock, it enables evaluation of scale-specific heterogeneity. Further, our method enables more monitoring bores to be tested for hydro-geomechanical properties at a lower cost compared to conventional aquifer pump testing. There is thus the possibility of better characterizing the heterogeneity of aquifer properties. However, our method also raises the need for further research in key areas where significant uncertainties remain, for example the possibility for non-linearity of the poroelastic response to surface loading and Earth tide forces. Addressing the identified uncertainties could contribute towards improving subsurface monitoring and characterisation in both consolidated and unconsolidated systems."

We hope that this clarifies what we mean.

Since it is now clear that the uncertainty in parameter estimates is not addressed, this point should be mentioned in the discussion or the conclusions. To mention a very basic point, it is unclear how any measurement error will propagate to computed parameters.
This statement contradicts the earlier comment by the reviewer where they requested omission of uncertainty considerations in the discussion. We believe that we have addressed this in our earlier response.

Other minor points:
At line 120: I guess "programs" should be replaced with codes/tools/software or similar.
Done.

Formatting of equations (6)-(12) should be improved. Remove the "and" between equations (unnecessary).
We believe that equations should be incorporated into the flow of the text for improved readability. Instead of removing the connecting words, we have revised these to improve the meaning and flow of the text.

If possible provide a physical definition for all quantities introduced, or explain the role of each equation (as done neatly in other sections). Same applies to eqs (32)-(35).
We have carefully added physical definitions wherever possible. Note that parameters, for example those related to Kelvin functions, are purely numerical.

Generally I suggest revising all Figure captions, as they contain some typos or inconsistent formatting.
We have double checked and corrected all figure captions.

Report #2

Summary

I thank the authors for performing major revisions in the structure and text of the manuscript that make it clearer and more pleasant to read. I also thank them for responding to all my remarks with clear answers. This paper is now undoubtedly worth publishing in HESS after the authors perform some minor corrections listed hereafter.
Thank you, we appreciate the review effort.

Detailed remarks

p. 5 line 99 equation (1): the definition of the tidal potential V is missing and should be connected to the incomplete sentence "where M2 is the tidal frequency"
We have corrected this.

p. 6 line 107: when computing theoretical tides, for instance with predict.for function within ETERNA software, a model is used for the Earth. This model can be elastic or anelastic. In the later case a phase is

introduced in the tidal constituents. In your tidal prediction using PyGTide, global anelasticity of the Earth should be considered to have correct M2 and S2 theoretical phases. Please provide some additional information on the Earth's model used for the tidal predictions.
We have added the following to the manuscript:

"*This is based on ETERNA and uses the Wahr-Dehant-Zschau model (Wahr, 1981; Dehant and Zschau, 1989) which assumes an elliptical, rotating, inelastic and oceanless Earth.*"

p. 10 line 202: "(e.g., -0.5°C in Figure 3b)" Please remove the "C" since this is degree not degree Celsius.
We have corrected this.

p. 10 lines 202-203: "Figure 2c,d show the solution space when considering the strain response as well as separation of hydraulic properties" This sentence seems unconnected to previous ones and to Fig. 3. Please clarify this remark by adding for instance "at leaky conditions".
We have corrected this.

p. 19 Figure 4: it is strange to see pressure in m instead of Pa. Maybe add in the legend something like "barometric pressures (in equivalent water heights in m)"
This allows direct comparison of the magnitude of influences. We have revised the caption as follows:

"*Time-series of groundwater levels from bores GW075409.1.2 and Thirlmere 2 located at Thirlmere Lakes (NSW, Australia), barometric pressures (in m equivalent water heights for easier comparison to the groundwater pressure heads), corresponding theoretical Earth tides (in nano-strain, nstr) calculated using PyGTide.*"

References: please correct references for repeated url or doi.
We corrected these. Note that it was not possible to highlight this in the track changes. However, we believe that typesetting to standard format will take care of this issue.