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Comment on hess-2021-359

Anonymous Referee #2

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.,
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Summary

This paper presents a methodology to estimate hydro-geo-mechanical properties of semi-confined sub-surface using the groundwater response of a borehole to Earth and atmospheric tides. The methodology is based on previously developed methods that were independently used either to investigate a tidal dilatation response or a surface load response. The originality of the present work is to combine both approaches and to successfully retrieve the hydro-geomechanical parameters of the sub-surface only by measuring the water-level pressure heads and using hydraulic and linear poro-elasticity theories. I suggest this paper to be published after some revisions mentioned here after are performed.

General comments

The paper is very long and organization of the sections is quite confusing. The methodology is described only on page 19, even after the description of the 4 studied sites. In order to better understand the sections on the pre- and post-strain response of the water level response and the discussions on the compressibility or incompressibility of grains, the methodology should be introduced right after the introduction.

In the different stages of the methodology, the hypotheses behind the employed theories should appear more distinguishly: drained or undrained conditions, consolidated or unconsolidated, lateral flows or only vertical flow etc.

Finally, the title should be more precise since the groundwater response was studied only

at semi-diurnal periods (for instance add “semi-diurnal” before Earth and atmospheric tides) and precise M2 and S2 tides in the abstract. The surface load model used in the pre-strain water level response and the model used in the post-strain response are frequency-dependent, elastic parameters too. We may expect different results when analyzing diurnal or longer-period tides for instance.

Detailed remarks

p.2 line 45: please define ETs

p.2 line 46: “separating tidal components” it depends mostly on the spectral resolution, hence on the length of the time-records used.

p.3 line 71: You applied a moving average spanning across a time period of 3 days; such a process is equivalent to a low-pass filtering not high-pass filtering. It filters out higher frequency signals. Please replace “longer frequency” by “higher”, since I do not know what means a longer frequency.

p.3 section 2.1: how well are identified the M2 and S2 tidal components in the data using HALS? How large are the uncertainties on the amplitudes and phases? Particularly on the phase, how precise it is, since it will affect the phase-shift value used to determine the use of a pre-strain or post-strain model.

p.5 equation (2): the superscript “p” in the following equations designs “pore” but here is this “p” for potential? Please clarify.

p.6 line 138 typo: dilation à dilatation

p.7 section 2.2.3 the figure 3 is referenced here before Figure 2. Please reorganize figures in order they are numerated in the order of citation.

p.8 section 2.2.3 some discussion on the boundary layer depth associated with the parameter a_w/r_w should be done in regard with the pre-strain model depth here after.

p.8 section 2.2.4 more discussion on the boundary layer depth is missing. For instance, at which depth/diffusivity the amplitude A_{M2} is maximum?

Interpretation of Fig. 2 is missing. For instance, with respect to the plots shown in Fig. 6.11 in the book by Wang (2000), at what depth the diffusive pore-pressure effects are confined? What is the limit in terms of thickness for using this theory as a good approximation? What about the phase of eq. (14), if you plot it wrt z/d, in which depth/diffusivity range does the sign change?

p.8 line 196 is this 10 m the value obtained for d when the pore-pressure is equal to surface load? How much larger the pore-pressure can be wrt surface load (when loading efficiency is larger than 1)? Please explain better the adequacy (the valid depths ranges) when combining ET and AT.

p.10 section 2.3 please introduce here BE = barometric efficiency

This section related to damping could be put into or right after section 2.2.2.

p.12 equation (26) in the denominator, the \theta should be rather a \gamma.

p.14 equations 32-35 are solved using an iterative LS scheme. Why not using a Bayesian inference in particularly to check the correlations between the various parameters?

p.17 line 349 please define MASL (m above sea level)

p.17 last line: "the ~28 days" as the minimum requirement for what? In order to separate M2 and S2 in terms of frequency resolution we would need 57 days. Please precise.

p.18 line 378 Detrending using SciPy function detrend is done by fitting a linear function, not by moving average, please correct this sentence; the moving average enables to low pass filter the data.

p.21 section 3.3 The choice of the post-strain model for the Death Valley site should be discussed since the phase shift of -1 degree is at the limit between pre and post-strain models.

p.24 section 4.2 I do not really understand this long discussion. It should be simplified in

order to highlight the major points.

p.24 line 497 typo: stain à strain

p.25 line 533 typo stain à strain

p.28 lines 606-609 these statements have already been claimed before, please remove this repetition.

p.26 section 4.4, discussion on the negative Poisson ratio. What about the influence of ocean loading? Have you quantified its impact on the amplitudes and phases of M2 and S2 for the 4 sites considered in this study? Uncertainties on the M2 and S2 phases should be discussed too since it may influence the values of the Poisson ratios obtained at the end. Correlation between the parameters should be checked too.