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.

RC2: 'Comment on hess-2021-359', Anonymous Referee #2, 10 Nov 2021

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.

We will revise the methodology in line with this and comments by RC1.

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.

We will add the specific assumptions behind the theory to the methodology.

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.

We agree and will revise the title accordingly.

Detailed Remarks

p.2 line 45: please define ETs

ET will be defined.

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

This statement is true for the Fast Fourier Transform (FFT), but it is different for harmonic least-squares (HALS) which is what we used. For further information please refer to the recent work by Schweizer et al. (2021). We will add a clarifying statement to the manuscript.

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.

We will revise this mistake.

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.

This comment aligns with the uncertainty analysis suggested by RC1. We refer to the response given earlier.

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

In this context, "p" is not a subscript. We acknowledge that this is hard to distinguish and will change all "ETp" to "ET_pot".

p.6 line 138 typo: dilation à dilatation
This will be corrected.

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.
This will be corrected.

p.8 section 2.2.3 some discussion on the boundary layer depth associated with the parameter aw/rw should be done in regard with the pre-strain model depth here after.
We are confused as the reviewer refers to section 2.2.3 but also a "boundary layer depth" and pre-strain model (which is section 2.2.4). We are unsure what this means.

p.8 section 2.2.4 more discussion on the boundary layer depth is missing. For instance, at which depth/diffusivity the amplitude AM2 is maximum?
The depth/diffusivity combination at which A_M2 is maximal can be seen in Figure 2a. We will add a statement including a reference to the figure.

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?
XXX

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.
We will add a statement to clarify this.

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.
We decided to separate the influences of ET and AT in the methodology as they require disentangling before combining the obtained results. Section 2.3 discusses the influence of AT and moving this after 2.2.2 would merge this with the section discussing ET influences. This change would confuse the reader's understanding of the separate mechanisms that are shown in Figure 1.

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

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?
We will use a more sophisticated approach to obtain uncertainties and correlations between the various parameters.

p.17 line 349 please define MASL (m above sea level)
We will revise as suggested.

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.
We wish to point out that the reviewer is likely referring to the Fast Fourier Transform. In this work we use harmonic least-squares (HALS) which has been tested by Schweizer et al. (2021). We will clarify this including a citation to avoid confusion.

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.
We agree and will revise this.
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.
This dataset was previously analysed by Rau et al. (2020), who justified the use of their model choice. We will clarify this by adding a reference.

p.24 section 4.2 I do not really understand this long discussion. It should be simplified in order to highlight the major points.
We will simplify this section as appropriate.

p.24 line 497 typo: stain à strain
This mistake will be corrected.

p.25 line 533 typo stain à strain
This mistake will be corrected.

p.28 lines 606-609 these statements have already been claimed before, please remove this repetition.
We will review this and revise as appropriate.

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.
We have not investigated the influence of ocean tide loading on the M2 and S2 components. We will do this for the locations given and assess how this would influence our results. We have responded to the request for uncertainty as part of RC1.