Responses to esurf-2021-36

General Comments

Dear Editors
Dear Reviewers,

thank you very much for your review and the positive feedback. Please find our point-by-point replies to your comments in this document.

Reviewer 1

I see all my previous comments addressed or adequate explanations why they were not addressed. The quality, clarity and structure of the text improved and I see only a few minor formatting/structuring things that should or could be improved. I am thus very happy with the manuscript at its current stage and the elaborated reply letter by the authors. I believe the clarifications have significantly increased the readability and potential impact of the material.

Section 3.1 reads a bit like an introduction. Would it be possible to either move the introductory parts to the peak picking and polarisation approach to the introduction or rewrite the section so that it is more focused on the investigated landforms instead of too broad?

- ➔ We added a few sentences to introduce the peak-picking approach already in the introduction.
- ➔ However, we would prefer to keep the rather extensive introduction and explanation of damping in Section 3.1, as we think that most readers are not familiar with structural damping and the techniques used to determine this parameter.

Figure 3 would benefit substantially from adding the names of the landforms on top of a, d an g, so that one realises that each landform is represented column-wise.

- ➔ Implemented.

Figure 4, would it be possible/meaningful to also add the vector arrows (as in fig. 5 d-g) to the XYZ plots? This would solve the small consistency issue.

- ➔ Yes, we now use the same style as used for Figure 5.

Regarding the overarching discussion of the "Short Communication" format, I understand these points. Indeed, the clarifications have resolved the apparent ambiguity of introducing a new versus applying an established technique. As mentioned in the reply letter, the updated version did indeed do an excellent job in resolving this initial misunderstanding. As for the correct title convention, I would leave this decision to the editor.

- ➔ Regarding the length of the article and the introduction of some more basic physical concepts, we still see this manuscript as a full article. However, we are absolutely open to the editor's decision and would also agree with a short communication.

REVIEWER 2

I acknowledge the great amount of work the authors have accomplished to reorganize the paper and answer many of reviewer's remarks. The paper has substantially gained in clarity by separating Methods, Results and Discussion. Anyway, I still have some few points that I demand to be fully addressed before final publication.

1) Rainbow Bridge : for the fundamental mode at 1.1Hz, damping of 0.9/0.6 % EFDD/SSI-COV against 2.4% by Geimer et al (2020) by half-power fitting technique. In principle, this mode is not mixture of two close modes. Any idea of the reason behind this over-estimation ?

- ➔ The half-power bandwidth technique is (like EFDD) suffering from spectral leakage. In case of different spectral resolution, also different (apparent) damping values are expected.
- ➔ We added the following explanation to the manuscript:
- ➔ "Rainbow Bridge had the lowest fundamental frequency in the study by Geimer et al. (2020), who used the same settings to compute the power spectra for all arches, including arches with higher resonant frequencies such as Squint Arch. Therefore, it is likely that these parameter settings were not ideal to resolve the low resonant frequencies of Rainbow Bridge with sufficient resolution. Therefore, we interpret the discrepancy between the half-power bandwidth and EFDD and SSI-COV as a result of strong spectral leakage for the half-power bandwidth technique."

2) Corona Arch : interesting finding of the hidden mode around 5.3 Hz. On the contrary, nothing is explicitly said about the technique to identify it : marked peak in the 2nd Singular Value ? Please clarify, may be in the 3.2 section when presenting the EFDD ?

- ➔ Exactly, the peak on the second singular value is picked and the mode shape of the 'hidden' mode is determined by the second singular vector.
- ➔ We added sentence for explanation.

3) Squint Arch : do not agree with "EFDD to retrieve the full normal mode shapes" as the authors could only recognize the first two normal modes (data quality of the nodal geophone array). Please qualify your statement.

- ➔ We mean "full-length" across the arch span, in contrast to a single station measurement.
- ➔ We modified to "full-length"

4) Musselman Arch: from Fig 5b (EFDD technique) it seems that the 4th mode at 6.58 Hz is peaked in the 2nd singular value (SV). Did the authors extract the modal shape from the 2nd eigenvector ? Please clarify.

- ➔ No, all resonant modes are picked on the first singular value and all mode shapes are based on the first singular vector.
- ➔ We specified this in the revised version.
- ➔ See next comment for more details.

Additionally, the 3rd mode at 5.62 Hz is also present in the 2nd SV. The authors stated that there are clearly not "hidden" modes in this case. May the authors comment on this ? especially in relation with the hidden mode recognized in Corona Arch (from a peak in the 2nd singular value)

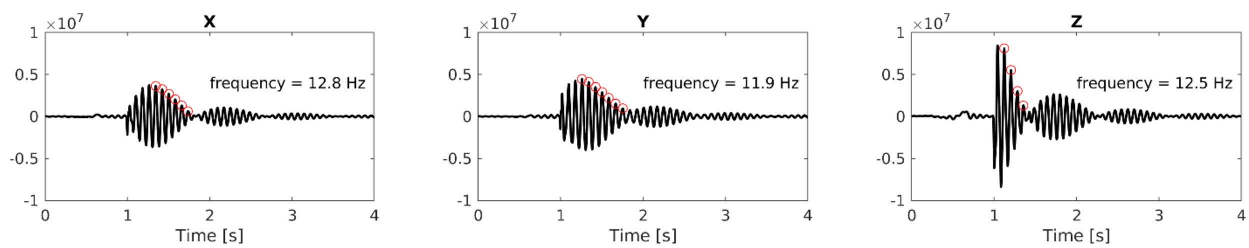
- ➔ This is a very good observation, thank you for pointing this out. We agree that this deserves some explanation.
- ➔ In case of Musselman Arch, this signature on the second singular value is related to an anomalous sensor component, which increases the noise floor across the entire spectrum. Note that the second singular value always peaks at the noise floor. We cannot state with certainty whether the anomaly was caused by bad sensor coupling or by a technical issue.
- ➔ We could simply remove the erroneous sensor from the analysis. However, we think that discussing this issue is more interesting and added a paragraph to Section 5.1 and a new Figure with the singular value plot with and without the erroneous sensor. This clearly shows that the issue is resolved by removing that sensor.
- ➔ In contrast to Musselman Arch, the higher singular values at Corona Arch (but also at Rainbow Bridge and Squint Arch) are peaking above the noise floor, indicating the presence of a real mode.

5) Finally, I apologize to insist on this point: I had asked further details of the "active" experiment. The authors referred to "Geimer et al. (2020) (who) applied this technique to a set of small-sized natural rock arches by stomping on the ground next to the structure and applying a band-pass filter around the resonant frequency". I did not find more details on the paper of Geimer et al (2020) (acknowledge the authors that facilitated it). Then, the authors may find here the place to develop.

First, eq (3) is not derived from eq (2), unless "small" damping (how much small ? 1%, 10%...). Second, y(t) is the amplitude of what exactly ? From the text (Line 192), it seems simply the recordings (just after the stomp) filtered around the resonant frequency of the mode. Several questions then arise : which sensor(s) ? All of them ? Average ? How many time windows used ? How far away from the arch ? What about source deconvolution (as from Figure 2b it is written IRF) ? Can the stomp be enough to excite fundamental modes at rather low frequencies (< 15 Hz) ?... It would be much simpler to explicitly show the example of Squint arch (logarithmic decay of active "stomp" data). The authors state that the active experiments can be considered "good estimates of damping" (Lines 190-195). This must still be demonstrated.

- ➔ Equation 3 is an approximation of Equation 2 for small damping ratios. We replaced the equal sign in Equation 3 by an approximation sign.
- ➔ There is no limit of what is small, as this depends on the uncertainty tolerance of the analysis. When evaluating Equations 2 and 3 it is evident that the error is smaller than 2% for damping values smaller than 20%, which covers most practical structures (Chopra, 2015).
- ➔ For damping ratios smaller than 3%, as it is observed on all rock arches in this study, the error is as small as 0.04%.
- ➔ y(t) is the amplitude of the measured quantity as a function of time and can be deformation, velocity or acceleration. As defined in Section 2, we are recording velocities. Specification added.

- ➔ It is not the scope of this paper to demonstrate that performing active experiments is a good technique for measuring damping of a structure. Measuring the decaying amplitude of a vibrating structure is the most direct measurement of damping (the decay of the amplitude is the damping). The problem with active experiment is that it is not always possible (or allowed) to excite the structure artificially, which is the reason why ambient vibration techniques are used (e.g., Magalhães et al., 2010). We added a sentence to Section 3.1.
- ➔ This study is about the demonstration that the EFDD and SSI-COV modal analysis techniques can be used to determine the modal properties of rock arches. This study is not about determining damping by active experiments and we do not want to overload our publication by discussing techniques that are not required for our study.
- ➔ Geimer et al. (2020) used the active experiment to verify their results obtained by the half-power bandwidth. Therefore, in the revised manuscript, we avoid direct comparison to the active experiment and only mention the comparison to the half-power bandwidth method (which is the same value). This shows that our study is not relying on this active experiment and that our manuscript would contain the same results and conclusions if that active experiment was never performed.
- ➔ As a reply to the reviewer, we show the raw (i.e. unfiltered) data of one stomp below. The dominant frequency is here simply determined by the inverse of the median of the natural periods determined by picking the maxima on the trace (red circles). The dominant frequency is ~12.5 Hz, corresponding to f2 in our study. Note that Geimer et al. (2020) interpreted f1 and f2 as one single fundamental mode.



References

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