Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/esurf-2021-36-RC1, 2021

Comment on esurf-2021-36

Anonymous Referee #1

Referee comment on "An Update on Techniques to Assess Normal Mode Behavior of Rock Arches by Ambient Vibrations" by Mauro Häusler et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-36-RC1, 2021

Häusler et al. present a study in which they apply two vibration analysis techniques to rock arches which are assumed to provide more detailed and robust results than previously employed methods. The study covers a relevant, timely and sufficiently novel topic, thus providing originality and an appropriate scope regarding the audience of the journal. The manuscript is in general of adequate quality; the language is scientifically correct and appropriate. Tables and figures are of proper quality with the exception of some too short captions (see below). It is good to see that the data is being made available. With some revision, it will make a valuable addition to the journal.

I see a few general concerns that I think need to be addressed, though. First, the study does not provide any benchmark data, but only relies on comparisons of the results of the two new techniques (EFDD and SSI-COV) – either amongst each other or with respect to results by Geimer et al. (2020). Thus, it is not possible to judge the overall quality/correctness of the presented modal information beyond that relative level of comparison. How can we know that the finer resolved results by EFDD and SSI-COV are real, due to the rock structure, and not just artefacts of either the data collection or the utilised methods? Perhaps this standing question can be solved by citing and discussing existing literature examples that provide the theoretical justification in this respect.

Second, the data are not interpreted in a geoscientific way, hence the implications of the identified frequency modes for the landform. Are the discovered values in agreement with what one would expect for these landform geometries, rock types, stress distributions and environmental settings? I suggest the authors spend a few sentences on establishing the context of their analysis and the journal's main scope: fostering understanding of Earth surface dynamics. This is especially relevant when considering the pitch given in the introduction.

Third, I see some ambiguities and arbitrarities in the presentation of the methods. It is good to see that the authors mention the multitude of model parameters but then, we simply get a reference to a table in the appendix in which the used parameter values are listed. The problem here is that these parameter values need to be introduced and justified. This should include a discussion of expected ranges, for example based on what other researchers have found or used. This may also include a description of the process that lead to the decision on the ultimate parameter values used. Currently, we have to take the parameter combination at face value, which is a fair bit from transparency and reproducibility.

Fourth, there is a mix of methods, results and discussion in each of the respective sections, which should be resolved. I give detailed comments to this issue further below. And I may emphasise that this is not a crucial flaw but one that should simply be resolved to give the manuscript a clear and organised structure.

l.1, The title is very (if not too) long and it also reads very (if not too) technical, with a lot of quite specific jargon, especially when considering the main scope and readership of the journal. I recommend to shorten the title, remove the detailed technical/methodological terms and in exchange to add more emphasis on the environmental context (e.g. "monitoring rock arch material strength evolution", but this is just a non-ideal example that I give to reveal what I might expect as a reader to see, feel free to adjust as you please).

l. 9-10, you could also consider motivating the study by a geotechnical pitch, instead of or in addition to the hazard one. Especially since you do not discuss the hazard perspective in the interpretation section, at all.

l. 27-29, you need to better motivate this sentence/abstract. There is a break in logic, here. I suggest you first motivate by the needs to monitor the stability of these landforms. Then you briefly mention the classically used techniques and their shortcomings. Then, this gives you the pitch to identify the research gap and thus motivate the seismic approach as complementary solution.

l. 36, "resonant frequencies arise primarily due to changes in rock mass stiffness". This is true but there are also other important factors that control the frequency. See for example Bottelin et al. (2013) or the full story told by Lévy et al. (2010). These other important aspects should be mentioned, as well.

l. 41, "more invasive monitoring techniques", mention these other techniques, perhaps in relation to my comment above (l. 28-29).

l. 81, can you explain why this refers to overlapping modes? Is this specific to the method?

l. 84, “picked manually”, here some information must be given on the criteria used to define these manual picks.

l. 92, please define “output-only technique”, this seems quite generic to me.

l. 99, You need to tell why each of the parameter values was chosen. Currently this is just arbitrary. It seems that the paremeter combinations determine to a significant extent the output of the technique, so this is a crucial part that deserves clear description and rigour. Have you tested different combinations and optimised them manually/iteratively? Have you used values published by other authors? Did you set the parameters just arbitrary?

l. 103-105, this would actually be much better suited for the introduction when you motivate the two techniques and want to convice the reader of their appropriateness. I suggest to move this to the introduction. It certainly does not match here in the results section.

l. 107 “modelled with a low number of modes”, “the maximum number”, please mention what a low number is and what the concrete maximal numbers were, and more important give a justification for these numbers.

l. 111, “user-defined accuracy criteria”, what are these criteria? Please describe and justify them.

l. 116, actually I would like to see the “raw” data, in this case the spectrograms or spectra, if just to be convinced that from these raw products one cannot already see the same frequency modes as in the advanced analysis.

l. 126, Fig. 2, it might help to colour code the singular value lines to indicate which is the first, second, third SV (a legend would be needed in that case, too). Also, this figure should contain the PSDs of the data sets to compare the new outcomes against them. In panels c, f and i, it remains elusive to me when a pole is defined stable and when not. Is there any criterion that was used? For example in c around 2.7 Hz there are many apparently stable values that still are plotted as blue crosses.

l. 133-151, there are many occurences of interpretations in this part. Please separate presentation of results and their intepretation throughout the text. Here is just an example.

l. 141, what means “good agreement”? Please quantify or leave it.

l. 159-163, This approach has not been mentioned in the methods. Please move it to the methods section and also give more context and information on SDOF.

l. 163, “half-power bandwidth technique”, here as well this may be better mentioned in the methods section (or introduction if it is more appropriate there) but in any way, some short explanation of the term and its implication needs to be added, especially in a non-seismologist journal.

l. 176-185, this section is also full of repeated interpretations of results. Please separate these materials into the appropriate sections, “Results” and “Discussion”.

l. 189/Fig. 3, the cation is too short and gives too little context about the presented material of this figure.

l. 206-208, this is repetitive and redundant. Consider removing.

l. 208, what means “well suited”? Can you quantify this? Besides, how do you know the methods a re well suited if there is no independent benchmark data to compare against? Data like rock mechanical model predictions of expected frequencies and their degree of overlap? Ovreall, this will be tricky to show. See my general comment on this issue.

l. 209-211, How can we be sure these are not just artefacts but indeed emerging due to a “better” approach?

l. 216-220, these descriptions are not really an outcome/implication of this study but rather a generic property of the method that should be better mentioned in the introduction (or methods section).