Earth Surface Dynamics Discussions

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Comment on esurf-2021-36

Anonymous Referee #2

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-RC3, 2021

This short communication of Hausler et al. presents the comparison of two classical methodologies currently used for operational modal analysis of engineering structures applied. It is an original and interesting idea to apply both methods to geomorphological features such as rock arches. The objective is clear, the paper can be fluently read and the results are interesting. Nevertheless I have several questions and remarks that I would like to be adressed/discussed by the authors before final publication.

General remarks

Need to exactly define what is EFDD and SSI-Cov methods. I understand is meant to be a short paper, but this are not current techniques in Geomorphology, so some hints will help readers a lot. Especially, how the damping is estimated by each method ? and what are the main processing steps in both.

Need to specify and discuss that only two of the four sites have array data. So, for example, what is the advantage to use advanced techniques in single station measurements ?

Specific Remarks

Abstract.

Line 14-16 "Therefore, we investigate two algorithms well-established in the field of civil engineering through application to a set of natural arches previously characterized using conventional seismological techniques."

-I would not call "algorithms" but instead "methods" for EFDD and Co-SSI.
-Please specify what do you mean by "conventional seismological techniques" -> may be the polarization analysis (Lines 17-18) ?

Line 19: the authors state that the proposed advanced techniques have "the capability to resolve closely spaced modes and provide stable damping estimates" and provide "more detailed characterization of dynamic parameters". After reading the whole paper, I'm not convinced that the results presented validate both statements (unless dynamic parameters, the authors mean exclusively modal shapes and frequencies)

Introduction

Line 71. EFDD is really "well-suited" for distinguish closely spaced modes ? Can the authors underline what enhanced EFDD is compared to FDD ?

Line 80-85. Sort of repetition of the main capabilities of each proposed advanced technique. Please delete.

Line 84. The authors suddenly include "rock slope instabilities", but they were not studied in the present work. Please clarify or delete.

Methods

Line 85. The methods section begins with the site presentations and instrumentation. Please adapt the section's title. The authors should clearly specify why each site has been chosen for the present study. Different instrumentations have been applied (See my General Remark).

Line 114. Please specify "data residuals" of what ? : Velocities, cross-correlation traces, at which sensor, components, etc.

Line 117. "Modes (i.e. Poles)" Need a MUCH longer explanation.

Line 120-125. This paragraph fits better in the Introduction part : the fact that the two methods have been previously compared in other context.

Line 130. This paragraph include technical details of the SSI that are not clearly followed by the reader. Please clarify

Results

Line 137-140. The authors state that damping values for the fundamental mode are quite different from the three techniques. By the way, the authors should previously define the "half-power bandwidth method" used by Geimer (2020), with respect to the "mode bell" fitting of EFDD.

Line 135. For this example of Rainbow Bridge, it may help the reader to recall that here a single station analysis is being used, and that is the reason a single Modal vector is compared.

Line 153. Corona Arch. It seems here two closed modes are found between 5.0 and 5.4, but EFDD and SSI_Cov indicate exactly the same frequency ! So the advanced methodologies were not indicated to separate close modes ? Please rephrase the paragraph to explain this behavior.

Line 158. The concept of "modal incidence" is not clear at all. Please redefine. In fact, could the authors propose other terminology (for the single station measurements) because it is quite confusing. I would not see an "incidence" angle for a mode. If I understand correctly, the authors would like to compare vector orientations in 3D, it is not better simply "azimuth, dip and rake" ?

Line 160. here again damping values are different. Reasons ? Can the authors advance any uncertainty for each estimation ?

Line 164. Squint Arch. Mode splitting is proposed here for the two close modes at 11.5-12.5 Hz cause by anisotropy. I do not have access to the work of Geimer et al (2020) but it looks that "homogeneous numerical model" does not reproduce a mode-splitting phenomenon.

First I guess "homogeneous" should be replaced by "isotropic". In fact, it may be the case that heterogeneous (though isotropic) models may present these two modes with quite close frequencies, but completely different modal shapes. In fact, it seems to be the case from the EFDD results of the later experiment with the 6 node stations : the first one seems to be a longitudinal mode, while the second one seems to be bending in the transvere direction. Is that also confirmed by the single station analysis (azimuth/incidence) ? This should be discussed in the paper. In fact why looking to anisotropic models (rather complex) when may be a numerical modal analysis could support this two "close" modes ? It may be useful for the readers to get the Figures from Geiger et al (2020) co-author included in the present paper.

I can not see why the full modal analysis with many sensors is not much exploited. For example, there is also the strange phenomenon of mode f3 (near 20 Hz) that completely dissapears in the second campaign. It would be really helpful to compare the recordings from the broadband seismometer (1st campagin) and the node exact (or closely) located node for the 2nd campaign. This would be quite useful for new planned operational modal analysis campaigns with node-type equipment.

Line 187. Puzzled about this interesting active experiment. More information needed.

Last thing, about Squint Arch. What would be the damping value estimated from the EFDD or SSI of the nodal campaign ? The peaks in the SVD looks quite different from the ones of Figure 2c). It will be useful to compare the two campaigns in light of different instruments, number of sensors, both for frequency and damping characterization. Which is the impact ?

Lines 210-215. It is rather dissapointing that the experiment with the higher number of sensors (2x16) is not much further discussed (with respect to the other 3 cases, only one paragraph !). For example, two lines were measured: synchronously ? with a reference station ? how much time duration ? Were these the same instruments that the ones in Squint Arch ? Why a twisting mode (torsional mode) is not being identified ? What about the dimensions (especially width, thickness) of the arch ?

Discussion and Conclusion

Line 227. I'm not fully convinced about the statement that both methods are "well-suited" to determine all dynamic parameters. Please rephrase, the objective of the paper was to look to differences in parameter determination. Anyway, the differences are important and the instruments for data acquisition seem to have much stronger impact than the methodology. Comment on that ?

Lines 235-240. Damping estimation (even with EFDD and SSI-Cov) is always difficult and I'm not convinced that the advanced techniques are more "robust" than the half-power bandwith picking. Is there no "spectral smoothing" in both advanced techniques ? In the EFDD a "mode bell" is fitted to an SVD singular value, then back transformed in time, and measured by logarithmic decay; and in SSI-Cov, as far as I understand, there is also a parameter fitting in the least-square sense. No smoothing and/or regularization at all ?

In conclusion, I advise the authors to revise this paragraph, specially the statement concluding that more advanced techniques would give more robust estimates of damping. Robustness may only be assessed if a detailed uncertainty analysis is carried out: different time windows, spectral estimation, etc.

Line 239. "determined by the active impulse measurement at Squint Arch." I'm really puzzled about this experience. There is not much information in the manuscript. If an active impulse was used (hammer?) , I could imagine relatively high frequencies involved. How damping at high frequencies can/may be compared with damping at the whole structure scale (freq < 15 Hz) ? I think the authors should give much more information of the active experience in the present paper.

Line 245-255. On the other hand, I agree with the authors about the capabilities of sensor arrays to better characterize modal shapes of different rock arches or geological structures compared to a single station approach.

Line 254. homogeneous "isotropic" models.