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Revised Title: Identifying fracture-controlled resonance modes for structural health monitoring: insights from the Hunter Canyon Arch (Utah, USA)
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Dear Editor,

We are grateful for the time and feedback from the two reviewers and have worked to revise our paper accordingly. This reply letter is organized to address each reviewer's comment individually. Our replies to specific comments are listed in blue below each comment, with all changes to the manuscript noted in *italic*. Line numbering in our responses refer to the revised version of the manuscript. We believe that these revisions have ultimately resulted in a stronger and clearer manuscript.

We submit this revised version to be considered for publication.

Best Regards,
The Authors

Response to Reviewer #1

Reviewer #1: This paper presents a seismic response analysis of the Hunter Canyon Arch (Utah, US). Both field observations and numerical modeling are adopted in the study. An array measurement of the arch's ambient vibrations was performed with nine simultaneously recording seismic stations. A polarization analysis was performed on the recorded data, as well as a modal analysis using the cross-correlation technique. Modal frequencies, shapes, and corresponding damping ratios were estimated. Based on photogrammetry, a 3D geometrical arch model was developed, and present joint sets were characterized. This model was implemented into a finite element solver. The numerical model analysis was focused on the effects of fractures and their propagation on the modal frequencies and shapes. The numerical results were compared with the observations (modal frequencies, shapes). The authors demonstrate that the fundamental and the first higher modes are sensitive only to the overall shape and bulk modulus of the arch, while the third higher mode is sensitive to the presence of fracture and its extent. They conclude that the higher modes of the slender structures are more sensitive to the fractures (if present). Therefore, higher modes could be better suited for the monitoring of the localized damage compared to fundamental modes. The manuscript's topic is original, interesting, and suitable for the Earth Surface Dynamics journal. The presented analysis is in-depth, and the discussions are focused. The manuscript is well-written and comprehensible. In conclusion, I recommend only minor revisions.

Authors: We thank the reviewer for the time and effort dedicated to evaluating our manuscript. The constructive feedback provided valuable insights that have allowed us to strengthen and refine our work. We genuinely appreciate the reviewer's detailed suggestions, which have highlighted areas for clarification and improvement, ultimately helping us to present a more robust and rigorous study. We have carefully considered each comment and have provided detailed responses below, outlining the revisions made to address all points raised.

Specific comments and replies:

Q1.1) The addressing of the observed modes as presented in the text is not very convincing in some cases. This might be due to poor visibility of the weak modal motions in Fig. 4 (that is, the small vectors are too small):

R1.1) We agree that the normalized modal vector amplitude at some monitoring stations is in some cases very small, especially for the first three modes in correspondence with the stations closest to the arch's abutment. However, these reduced amplitudes indicate small participation of those points in the modal deformation pattern at those resonance modes, hence we believe those vectors may enhance rather than hide the visualization and interpretation of modal shapes.

Q1.2) Mode 2 (Fig. 4b) is addressed as a second-order transverse bending mode, but no node is visible in the observed shape, all points seem to be in phase. This, in my opinion, contradicts the simulation's result (Fig 5c).

R1.2) It is indeed true that no nodal point can be observed from experimental results in Figure 4b, but this is because the transverse or out-of-plane bending takes place on the Radial-Vertical (YZ) plane, thus mostly involving the pier/pillar. Due to the impossibility to measure ambient vibrations anywhere else than on the arch lintel, we could only interpret this mode of vibration based on our experience and numerical modeling results. This modal deflection pattern is very well reproduced by our numerical modeling results. However, we understand that Figure 5c may not represent this second-order bending mode correctly, hence we have decided to modify that panel to enhance the visualization of mode 2. To better clarify this point, we have attached a sketch created using our numerical modeling results for mode 1 and 2, that should highlight how these two modes are indeed a first- and second-order transverse bending modes (Fig. SM1, see Supplementary material at the end of this reply letter).

Q1.3) Mode 3 (Fig. 4c) looks very similar to Mode 1. The comparison with the simulation is also not very clear. The direct comparison of the motions at the observation points might be helpful.

R1.3) This is an example of how reduced participations in modal deformation patterns at specific locations can highlight differences between the measured resonance modes. In particular, the main differences in the experimental results between Mode 1 and Mode 3 can be derived by observing normalized modal vectors at stations H06 and H07, both in terms of magnitude and direction. Considering Mode 1, station H06 features a small amplitude, transverse-oriented modal vector that is in-phase with all the other vectors measured on the arch (i.e., from H01 to H05). For the same resonance mode, station H07 shows no significant participation, and this is well-described by the absence of a visible modal vector at that specific location. Considering Mode 3, the modal vectors for stations H06 and H07 describe a different configuration from Mode 1. Here, despite their reduced amplitude, those small amplitude vectors align toward a radial-oriented direction with respect to the arch. The direct comparison between experimental and numerical modeling results for each resonance mode, both in terms of normalized modal displacement amplitudes and angle differences between modal vector directions, is provided in Figure 6.

Q1.4) The cross-correlation technique is not very well introduced. A brief introduction would be helpful, outlining the suitability compared to other methods (frequency-domain decomposition, stochastic subspace identification).

R1.4) We added a new paragraph to the text of section 3.2 (line 144ff):

"[…] Cross-correlation modal analysis, also referred to as Natural Excitation Technique (NExT) (Farrar and James III, 1997), is a time-domain, output-only method used to estimate modal properties of vibrating structures by analyzing the cross-correlation functions between output response measurements under ambient excitation. We selected this analytical method over Frequency Domain Decomposition (FDD) and Stochastic Subspace Identification (SSI) techniques (Brincker et al., 2001; Van Overschee and De Moor, 1996) on the basis of computational efficiency. The robustness of cross-correlation modal analysis, as compared to FDD and SSI has been previously demonstrated in comparative studies (Bessette-Kirton et al., 2022; Häusler et al., 2021a), establishing it as a reliable approach for accurate modal property estimation of natural structures under ambient excitation."

Q1.5) The relative modal mass (RMM) parameter is not well introduced in the text, although it is used as a criterion for the mode selection presented in Fig 5. A brief description should be included in the text, or RMM can be omitted in the text and the figure

R1.5) We've updated the corresponding paragraph, adding more details and references to introduce the significance of the relative modal mass parameter and its implications in modal analysis (line 244ff):

"[…] This direction-dependent parameter is frequently employed in engineering studies as it measures the extent of mass participation at each resonance mode, allowing for assessment of the significance of specific eigenmodes in describing the dynamic behavior of a structure (Aenlle et al., 2021; Mayes et al., 2015)."

Q1.6) The damping was not considered in the numerical simulations, although the observed values are discussed in the text. The possible implementation of damping in the simulations would give more insight. This could be stressed in the manuscript.

R1.6) We appreciate the suggestion to include damping in our numerical simulations. This is indeed an interesting line of research that could further refine our understanding of the system's dynamic behavior, particularly by providing a more detailed representation of energy dissipation during free vibrations. However, we believe that incorporating damping, while valuable, is beyond the scope of this study. The main objective of our research is to investigate the sensitivity of resonance frequencies and mode shapes to discrete rock mass fractures, and our numerical models were specifically calibrated to match these resonance modes, and the insights we derive are based on the comparison of modal frequencies and shapes. Furthermore, while we have experimentally measured damping ratios and discussed them in the context of the observed resonance modes, incorporating these into the numerical modeling would require careful validation steps. It would necessitate an additional layer of complexity in our modeling, including time-consuming efforts to develop, calibrate, and validate models that realistically capture energy dissipation mechanisms. Given the scope of our current work, we believe it is appropriate to leave this task for future research.

Q1.7) The discussion of the damping ratios of different modes is not very clear. The damping ratio of the fundamental mode is found to be small, and it is stated in the manuscript:

<u>Line 287–288</u> *[…]. These low damping values indicate that seismic energy is trapped within the structure and unable to propagate to the surrounding rock mass (Häusler et al., 2021b).*

In contrast, in the explanation of the stronger damping of the higher modes, it is stated:

<u>Line 292–294</u>5 *[…]. We hypothesize this change could arise from increasing participation of the abutment fracture area, and corresponding fracture shear and normal compliance, in modal deflection of the arch. The hypothesis of increasing energy transmissivity at the fracture scale is also supported by our numerical modeling results.*

This is not clear. In general, seismic energy could leave the structure through the base and side of the arch which are assumed transparent. Indeed, the compliant fractures should result in lower transparency and, thus, even lower damping. However, the energy dissipation in the fracture might increase the damping. Please reformulate this part of the discussion.

R1.7) We thank the reviewer for these valuable comments regarding the discussion of damping ratios across different modes. We have tried to improve the abovementioned paragraph by adding a more in depth discussion. We interpreted the very low damping values observed for Modes 1 and 2 (≤1%) as primarily resulting from an internal friction mechanism (i.e., material damping) and the minimal contact area of the abutment and base surfaces relative to the overall size of the arch. This limited contact area restricts pathways for seismic energy to dissipate into the surrounding rock mass, effectively trapping energy within the structure (radiation damping). The significant increase in the modal damping ratio observed at Mode 3 suggests the involvement of additional energy dissipation mechanisms.

Below is the revised paragraph incorporating the new discussion (line 318ff):

"[...] Similar evidence can be derived from analysis of damping ratios. The first two resonance modes show low damping (≤ 1 %) in good agreement with results obtained at other sandstone arch sites (Geimer et al., 2020; Häusler et al., 2021a; Moore et al., 2019). These low damping values indicate that seismic energy is trapped within the structure and unable to propagate to the surrounding rock mass ***due to the minimal contact area of the abutment and base surfaces compared to the overall modal mass of the arch*** *(Häusler et al., 2021b). Starting from mode three, higher-order resonance modes exhibit an increasing trend in damping ratios (Fig. 2c). This result is not unexpected since complex deformation patterns, generally characterized by a greater number of node points, can aid in energy dissipation through relative motion and material-dependent internal friction mechanisms. However, mode three itself shows a marked increase in damping (up to 3.8 %), indicating some additional, frequency-dependent seismic energy dissipation mechanism. We hypothesize this change could arise from increasing participation of the abutment fracture area, and corresponding fracture shear and normal compliance, in modal deflection of the arch.* ***In structural engineering, it is well-documented that frictional interfaces, such as joints and connections, contribute to energy dissipation due to micro-slip and frictional losses under dynamic loading (Cimellaro, 2023; Lazan, 1968). This phenomenon, often referred to as interface or slip damping, is critical in dynamic analysis and structural design. In engineering geology, the role of fractures in modifying energy dissipation mechanisms is less extensively studied, and there is a higher degree of uncertainty due to the complex nature of rock fractures (i.e., geometry, properties, contact areas). Despite this, it is reasonable that frictional interactions at fracture interfaces, as well as surface roughness, can contribute to increased energy dissipation within rock masses (Bandis et al., 1983; Goodman, 1980; Habaraduwa Peellage et al., 2024)."***

Our hypothesis is that, although we cannot precisely characterize the fracture behavior at the scale of our study, frictional mechanisms at the fracture scale—or even at the surface roughness scale—may enhance the damping of higher-order modes. This would lead to greater energy dissipation as seismic energy is partially transmitted and dissipated through these discontinuities.

Q1.8) The angle difference presented in Fig. 6 is not defined in the text. Is the difference for the observed modes rather in horizontal or vertical direction?

R1.8) The angle difference presented in Figure 6 refers to the angular separation between the observed experimental and numerical modal vectors, regardless of orientation in horizontal or vertical directions. We retrieved the angle (θ) by evaluating the arccosine of the normalized dot product between vector pairs (i.e., experimental modal vectors were always used as reference), as explained in the following equation (line 209f):

$$\theta = \cos^{-1}\left(\frac{|\boldsymbol{u}^T\boldsymbol{v}|}{||\boldsymbol{u}||\,||\boldsymbol{v}||}\right)$$

Where $\boldsymbol{u}$ and $\boldsymbol{v}$ are the vectors being compared, $||\boldsymbol{u}||$ and $||\boldsymbol{u}||$ their magnitudes, and $|\boldsymbol{u}^T\boldsymbol{v}|$ the dot product. The text has been updated accordingly.

Q1.9) The name of the arch should be mentioned in the manuscript's title. Other arches are not studied and not much discussed in the text in the context of the study (higher modes and fractures).

R1.9) The title has been updated following both reviewers' suggestions:

"Identifying fracture-controlled resonance modes for structural health monitoring: insights from the Hunter Canyon Arch (Utah, USA)"

Q1.10) Different symbols should be used for the simulated modal frequencies (for example, in Fig. 5). You can use caret or tilde.

R1.10) We have changed the labels for modeled and measured resonance modes in Figure 5 as we agree that it could have been confusing. However, we believe it is not necessary to change the adopted symbols in other parts of the manuscript.

Response to Reviewer #2

Reviewer #2: The manuscript "Array-based ambient vibration modal analysis describes fracture-controlled mode shapes at a natural rock arch (Utah, USA)" by Grechi et al. shows how modal analysis of ambient vibrations can inform about the structural state of free-standing landforms. The approach is novel. The results exciting and promising. And this is also the flaw. The reader needs to be well versed in both fracture mechanics and stability, as well as in ambient seismic methods to be able to see the contribution this manuscript can make. The manuscript also lacks a bit in clarity of the structure and scope, and explanation of the major concepts and methods.However, with some minor revision, this manuscript will be a great contribution to our understanding of the rate of fate of rock arches.

Authors: We would like to express our sincere gratitude to the reviewer for the extensive and thorough review of our manuscript. It is rare to receive such a detailed and thoughtful evaluation, and we are genuinely appreciative of the time and effort invested. The reviewer's insights have been invaluable in helping us improve our work, allowing us to address specific issues that we now consider strengths of the revised manuscript. Below, we provide a detailed response to each of the reviewer's comments, outlining the changes made to enhance clarity and rigor.

Specific comments and replies:

Q2.1) The title reads quite complicated and not well descriptive of the main finding and scope. As far as I understand this is specific to the Hunter Canyon Arch and could maybe be also more general to natural rock arches in Utah. So, I would either make the title general to that "...at natural rock arches (Utah, USA)" or specific "at Hunter Canyon Arch (Utah, USA)". What is really the key finding here? – The title describes more of the method "Array-based ambient vibration modal analysis...." – isn't the key finding rather that there is a mode shape that is indicative of fractures, which can be determined by ambient vibration analysis?

R2.1) We thank the reviewer for the valuable suggestion. We have updated the title to better highlight our key findings:

"Identifying fracture-controlled resonance modes for structural health monitoring: insights from the Hunter Canyon Arch (Utah, USA)"

Q2.2) The abstract, if read before the manuscript, reads quite confusing. This is partially due to unnecessary complicated sentence structure, as well as jargon and concepts that are not introduced. What is the motivation? What is the scope? What is the research question? How did you address this? If you could be a bit more straight forward/explicit on that, that would enhance the readability. E.g., the motivation is the degradation of free-standing rock arches by (progressive) damage evolution, the scope is to determine the structural state including the fractures, the research question/hypothesis is that the damage is affecting the mode shapes, your approach is to do an array-based ambient vibration modal analysis.

R2.2) We have reviewed the entire abstract including a new first paragraph where we tried to clarify motivation, scope and research question of our work (please see the revised manuscript).

Q2.3) Why do natural arches have a "high cultural value" and why does it matter here – you are not suggesting any way to preserve them? Or is it rather that not only the access (physically getting there) but also the accessibility (permission to get there) is limited. What you might want to point out is that only non-destructive methods are eventually allowed.

R2.3) Yes, indeed. We have pointed out that their cultural and natural significance yields limitations to their access and accessibility. It is also true that we are not suggesting any preservation strategy, but still our results might contribute to informing interested parties and stakeholders such as National Park Services, in the definition of future preservation strategies. Line 8f:

"[...] However, methods to detect structural changes arising from fracturing are limited, particularly at sites with difficult access and high cultural value where non-invasive approaches are essential."

Q2.4) Line 9ff: The sentence reads repetitive. Please simplify and here it would be great to get your hypothesis, so it is easier to follow why you are doing what. Some of the wording seems off: results "revealed", and "assortment of 3D mode shapes". Lines14-24: While this is exiting to read as a summary, it is hard to follow if you did not read the whole manuscript before. Please narrow down and ideally be more quantitative of the key finding and its implications.

R2.4) We have updated the text to address all these suggestions (Please refer to the revised manuscript).

Q2.5) Imagine the reader skipped reading the abstract, they will not get what the motivation is. Starting with "Modal analysis" and in the following using jargon and very specific concepts without defining or introducing them makes it quite ambiguous, especially if the reader is not familiar with seismic methods including the deployment and analysis.

R2.5) We have updated the introduction following these suggestions. We understand that very specific topics and concepts are introduced, but we believe that giving definition for each of them (e.g., resonance mode, mode shape, damping, structural health monitoring, higher-order resonance modes, etc…) would impact not only the readability of this paper, but also the flow of information we provide. Besides, we have provided several literature references that will surely help any interested readers in deepening their understanding of those specific concepts.

Q2.6) The section (Section 2) reads quite unclear, and some of the methods could go into the supplements. For example, what do you mean by (line 82) "…appearance of precarious stability"? What is a (line 94) "fracture-bounded compartment"? How does the fracture sets link to your analysis or do you map them but then assume a continuum of the material and transmission of the arching stress? What is the difference between the mapped fracture sets and the fractures that degrade the stability (scale, openness, persistence…)? If the fractures (line 95f) have aperture how are your vibrations transmitted?

R2.6) We updated the text providing more information as required. Also, although surveyed and mapped fractures are open at surface, it is reasonable considering this aperture to progressively decrease at depth, and thus marking the transition from an open fracture to continuous rock mass. When dealing with vibration transmission at a fracture interface, we must consider that this transmission occurs where fracture surfaces are in contact or where rock bridges exist.

Q2.7) Figure 1 is rather confusing than providing a good overview. Only S1 and S2 fractures are mentioned later in the manuscript. The photos are very red and hard to see where the arch is and where the background rocks are. Can you provide one that just depicts the arch? Similar to figure 2a? Indicating fracture planes in red dashed lines is nearly invisible, even if not colorblind.

R2.7) We have updated Figure 1 (see revised manuscript).

Q2.8) Section 3.1. This reads a bit confusing and could be linked to the fracture sets described before. For example, take Figure 2a and make it Figure 1. What do you mean by (line109) "but still encompassing the fracture-isolated rock volume"? What do you mean with (line 113) "Winds where calm"? -remember some readers might not understand why that piece of information is important and why you don't report on other environmental conditions, e.g., T or RH. Please be clear on what this means, e.g., you mounted the samples with putty to provide good coupling, calm winds allowed for low environmental noise, and thus a high(er) signal to noise ratio of your measurements.

R2.8) We have updated Figure 2 (see revised manuscript) and the text of section 3.1 (line 112ff):

"[...] Winds were calm during the experiment, minimizing environmental disturbance and thus improving signal-to-noise ratio of the vibration data. Other environmental factors, such as air temperature and relative humidity, varied little during the short duration of the experiment."

Q2.9) Section 3.2. It is hard to follow what you did exactly. Especially, how you identified the "prominent peaks" and why those are interpreted as the "resonance modes of the arch". I see two options, one would be to share the code in the supplement, the other would be to detail the methods by which you picked the peaks.

R2.9) We have clarified that the identification of prominent peaks was performed manually (line 224), based on the consistent appearance of these peaks across spectra from different components. This interpretation was further supported by our analysis of the degree of polarization, which confirmed the organized nature of particle motion at these frequencies. We believe this approach ensures accurate identification of resonance modes without requiring additional code or automated methods.

Q2.10) In Figure 2b, please label the peaks you identified. They should match in all panels. To better compare it might be good to have the same y-axis for all directions. The x-axis differs between b panels and c and d, why? Can you combine c and d, and plot them under b, making the x axis not the mode but the frequency and just indicating the modes in all plots with a dashed/dotted line? – The dotted lines in b seem not to correspond to those in c and d.

R2.10) We have updated Figure 2 (see revised manuscript).

Q2.11) How does the amplitude of cross-correlation (Figure 3a) enable "visualization of mode shapes along the linear array"? - which is not fully linear, as H08 is offset. Please be clear on what it shows. You have not defined what "mode shapes" are by now, and what do you mean by "relative modal displacement" (line 47). Please use the figure to better explain what the modal shape and changes thereof show, as well as what the composite vector and its inclination can tell. In this section you are describing the methods, not the results. Where can I find the cross correlograms for the other frequencies?

R2.11) No results have been described or introduced in this section. Here is the definition of mode shape we provided in the text (section 3.2, line 133ff):

"[...] *Mode shapes are essential parameters in SHM studies as they represent specific deformation patterns at each resonance frequency, hence describing the relative displacement and phase relationships across different regions of the structure (Brincker and Ventura, 2015)."*

The amplitude of cross-correlation enables the visualization of mode shapes as described at line 156f:

"[...] The resulting curve for each station enables visualization of mode shapes along the linear array, describing the amplitude and phase of relative modal displacements at each recording station (Fig. 3a)."

Q2.12) You are not referring to Figure 3b in the text at all. Is it necessary? Why is the direction of the ZZ component not indicated? There seem to be also some artefacts in the figure which are distracting. Maybe consider simplifying the 3D shaded model to outlines, e.g., sometimes less is more.

R2.12) We have updated Figure 3 (see revised manuscript). We elected not change the 3D shaded model as we believe it provides the most clear and simple visualization of our results.

Q2.13) Section 3.3. This section starts with a logical jump. 3.1 and 3.2 are about the physical arch and the measurement of the ambient vibrations. This section takes a step further by using the information from 3.1 on the structure, turning it into a numerical model arch. Then you use the measured vibrational modes to simulate the dynamics in the model arch. The aspect of how you set up the model geometry, what you include and what not reads very confusing. Please clarify between model setup (mesh, smoothing, spatial data, ...), material properties and structural features and with which assumption they have been implemented in the model, please also provide the boundary conditions. You seem to have different models you tried, please make their difference explicit and why you chose those. For example, what do you mean by (lines 182) "...generate these compliant mechanical zones.", and "...a 1 m wide zone cutting...", or (line 189) "...fractures were modelled as open zones".

Q2.13) We have updated the text to clarify how boundary conditions were implemented (line 185f):

"[...] All model outer boundaries were fixed at zero displacement to simulate the natural constraints of the surrounding rock mass."

The steps we implemented to set up the model geometry are based on common practices and their explanation would fall beyond the scope of this work – even in the description of our methods.

Q2.14) The dynamic modeling part is also not easy to understand as it is mixed in with structural aspects, without saying how the one affects the other. Please be clear on how you represent the dynamics in your models and how you get "data" from your model which is then compared with the field measurements. Please state how you compared the modeling and field data. It seems also that you are not showing the comparison of the first 4 resonance modes, but 1,2,3, and 6 (Figure 6).

R2.14) We have updated the text. However, we would underline that most parts mentioned by the reviewer are already well detailed in our opinion. For example, concerning the comparison between numerical and experimental results, it's stated:

Line 204f: *"[...] For all models, we extracted relative modal displacements in the transverse, radial and vertical directions for the first four modelled resonance modes at points representative of array stations."*

To give more details on the comparison, we have added Equation 1 to explain how the angular difference between numerical and experimental 3D vectors was computed (see reply to Reviewer #1).

We have also introduced in the results section why we compared measured mode six with modelled mode four (line 250ff):

"[...] The only exception is represented by the similarity between modelled mode four and measured mode six (i.e., first-order vertical bending mode). In this case, although the modelled mode order is not the same as field data and no evidence of third and fourth measured modes was found, the continuous model predicts a first-order vertical bending mode at 6.8 Hz (i.e., 20 % lower than the measured frequency). Therefore, due to the observed similarities in modal properties, we compared modelled mode four with measured mode six to further assess consistency in the vertical bending response predicted by numerical models."

Q2.15) Here it would be good to link the figures better with the text – show and tell. For example, what does the Figure 2b really show? I can count and try to detect where the 9 peaks are, or you could label them and mark them throughout Figure 2 (see comment above).

R2.15) We have updated Figure 2 and panel b (see revised manuscript).

Q2.16) Please present the results first and then interpret (and discuss, in the Discussion). For example, to highlight what the result of the spectral analysis is you need to make clear how this was done. Then state the results, you found 9 peaks in the arch which were not present in the reference station. The interpretation of those peaks is part of the discussion (strictly speaking). This is a pattern that is found throughout the results and makes it hard to discern what the result really are and what your interpretation is. This also leads to a repetition in the Discussion.

R2.16) We understand the importance of separating results from discussion. However, in our study, the initial interpretation of the spectral peaks is fundamental to the presentation of results. Identifying these peaks as resonance modes (we have already clarified that we manually picked those peaks) is not simply interpretive but a necessary part of defining the framework for all subsequent analyses. Without this initial interpretation, the presentation of other results would lack context and meaning, as the classification of these frequencies as resonance modes is what structures our analysis. Therefore, we believe that including the interpretation of these frequencies within the results section is critical to effectively communicate our findings. Holding this interpretation for the discussion would render the results incomplete and potentially confusing to the reader.

Q2.17) It would also be good if you could link your results to your research question and hypothesis, e.g., what was tested/looked for. For example, starting in line 225, it becomes unclear what you are aiming at in this manuscript: Are you interested in structural controls of the arch or fracture-controlled modes, as the title suggests, or is that the same for you?

R2.17) We realize that using the form "structural control" might lead to ambiguities for non expert readers, as the reviewer implicitly suggests. Hence, at line 240, we changed "*structural control*" into "*fracture control*".

Q2.18) Line 228: "For this reason we **s**elected…"

Line 229: you are introducing a new terminology here: "highest relative modal mass (RMM) values", please introduce this.

Line 243: The wording "…can be better appreciated…" does not seem to fit here.

R2.18) We have addressed all these issues in the main text (see revised manuscript).

Q.219) Figure 4: Please label in black, then you can omit the outlines, but the contrast is still higher. You are not using the whole inclination-color-wheel from 90º to -90º. Please adapt so you have a better distinction in the colored arrows. Why did you change the view for panel (f) and (g)? Could you use view (f) for all of them? It seems to become overly complicated.

R2.19) We prefer to keep labels in white to increase the overall visibility. The displayed vectors do cover nearly the full range from -90º to +90º. This range is consistent with the expected orientation of vertically oriented vectors, which are naturally constrained to incidence angles within this interval. In panel (f), for example, vectors approach +90º, illustrating the extent of this range. As for the view change in panels (f) and (g), we opted for these perspectives to enhance the visualization of modal vectors based on their dominant orientation (transverse, radial, or vertical with respect to the arch), as noted in the figure caption. Using the most informative view for each resonance mode helps convey the modal vectors more effectively. We believe this approach enhances clarity rather than complicating the interpretation, as it aligns with the dominant direction of modal deformation patterns for each mode.

Q2.20) Figure 5: is (a) necessary? (b) please use different symbols for each direction. The color scheme might also not be visible for some or printed in grey scale. If you use black outlines, the difference in color becomes even harder to see for the smaller symbols. (c)

What is the min -max color bar? How do the grey arrows compare to Figure 4 and the inclination-colored arrows? Why does the view change or is the arch deforming?

R2.20) Thank you for your feedback on Figure 5. Panel (a) is essential as it provides an example of one heterogeneous model implementing fractures (see caption), showing where they have been incorporated in the simulations. This contextual information is important for understanding how the fractures influence the model results. We selected the current color scheme with careful consideration to ensure that it remains visible both for individuals with color vision deficiencies and when printed in grayscale. The color choices were tested to maintain legibility under these conditions. The grey arrows do not directly compare to the inclination-colored arrows in Figure 4. Instead, they merely represent the modal vectors at specific points on the model's mesh, providing localized information on the direction and magnitude of the deformation. Similar to Figure 4, we opted to use different views of the model in Figure 5 to enhance the visibility and clarity of the mode shapes. Each model is shown in its deformed state, with deformation magnified to illustrate the mode shapes effectively.

Q2.21) Table 1: Maybe this can go in the Supplementary Information? It is also confusing why the resonance mode no. 6 becomes 4 in the model but stays 6 in Figure 6. What is the reasoning behind changing the frequency from the field and in the different models? Why did you pick those four modes for the modeling and not the others?

R2.21) We believed that including Table 1 in the main text initially helped provide a clear comparison of measured and numerical results. However, since each resonance mode is now highlighted in panel c of Figure 2, we agree with the reviewer that Table 1 could be omitted from text. Regarding the comparison between measured mode six and modelled mode four, we have provided an explanation in this reply letter (please see R2.14). This choice was made based on the similarities in their deformation patterns and characteristics, which warranted a direct comparison despite the difference in mode order.

Q2.22) Figure 6: Can you please use different symbols for the field data and the models. Color comment see above.

R2.22) We adopted different symbols for field data and numerical models as suggested (see revised manuscript).

Q2.23) The discussion reads rather length and not to the point. Can you be more specific and quantitative? For example, lines 287f, what do you mean by this sentence and how does your measurement and the shown results support the trapping of seismic energy?

R2.23) We have added a sentence to clarify the overall meaning of the paragraph (line 320ff):

"[…] These low damping values indicate that seismic energy is trapped within the structure and unable to propagate to the surrounding rock mass due to the minimal contact area of the abutment and base surfaces compared to the overall modal mass of the arch (Häusler et al., 2021b)."

Our results suggest that, based on comparisons with literature (please see the referenced paper), low damping values likely indicate that seismic energy is trapped within the structure due to the relatively small contact area of the abutment and base surfaces in relation to the modal mass of the arch.

Q2.24) If you want to discuss the hypothesis of e.g., "…increasing energy transmissivity at the fracture scale", it would be good to state this and introduce the concept and how it relates to your study, e.g., in the introduction. Please review the hypothesis you are stating in the introduction and those in the discussion and see which of them are addressed by your study, and which are not. Please be clear about, what was observed in the field and what you modeled.

R2.24) We understand that the term "hypothesis" may have led to some confusion. In the discussion, we refer to the "hypothesis of increasing energy dissipation at the fracture scale" as an interpretive insight that emerged from our results (experimental and numerical) and subsequent comparison with literature. This was not a foundational hypothesis established in the introduction, such as the primary hypothesis regarding the control exerted by fractures on arch dynamics. Instead, this idea was formulated during the analysis phase, as we interpreted our results in light of existing studies. However, we have updated the sentence at line 335:

"[…] The increase in energy dissipation at the fracture scale is also indirectly supported by our numerical modelling results."

Q2.25) The part on the fractures and how it was implemented in the model reads very confusing, which is partly because new hypothesis and data (fracture sets) are introduced but also the basis for the whole study is unclear, e.g., what is the anticipated outcome of the model, and how does the measured resonance modes fit in? One thing that might help is to make the temporal and spatial (expected) damage/weaknesses explicit, and how that would show in the resonance frequency. This would also help to link it better to Figure 7.

R2.25) We have revised the Discussion section by moving new results introduced through our evolutionary damage modeling to the Results section, then commenting on their significance and further meaning in the Discussion section. We believe this also helps make the Discussion section more concise and focused. The damage modeling approach and results are indicated in Figure 7. No new data were introduced in this section, rather just a new approach to analyze the possible temporal development of fractures as a means of exploring modal sensitivity.

Q2.26) Figure 7: See comments to colors and labels above, as well as the observed and modelled modes 1,2,3,6/4. (c) why relative modal displacement? Why does it change? And why is mode 4 plotted with flipped axis?

R2.26) We plotted relative modal displacement at each measured and modelled array station to highlight changes in the deformation patterns across different damage stages. By focusing on relative displacement, we are able to observe how the propagation of fractures (from T0 to T6) affects the distribution of displacement at each mode. This approach allows for a clearer comparison of how specific regions of the arch respond to increasing crack depth, particularly for mode three, which shows a more significant shift compared to other modes.

The axis for modelled mode four (that is compared to measured mode six, as already explained) was flipped to enhance consistency with the orientation used in the other panels, ensuring a clearer visualization of deformation patterns across modes. This mode has in fact a dominant vertical bending direction. Therefore, a vertical-radial (ZZ-RR) plane was selected to better represent modal deformation. This adjustment helps to compare mode shapes across all stages more effectively, especially in identifying shifts associated with crack damage.

We have adjusted colors and labels of Figure 7 for better clarity and alignment with your previous comments (see revised manuscript)

Q2.27) Please come back to the bigger picture and the research question here. The writing is lengthy and ambiguous and vague. Please be precise on which hypotheses did you test and what are their outcome? What are the take home messages? Tell the reader why these frequencies, why these modes are indicative of fractures, bulk properties, local changes? How does it relate to spatial scales, and how much would the mode shape change due to ongoing fracturing (temporal), thus how sensitive is this approach? What can we take away from this specific arch and apply it to other structures or arches?

R2.27) We have susbstantially revised the conclusion section (see revised manuscript) to strengthen its connection to the overarching research question and main findings. We believe the writing is now clearer and more focused on the study's primary outcomes and take-home messages. We hope the updated version will address any concerns regarding clarity and focus. Here are some specific answers to the reviewer's comments:

1. The relationship between certain modes and fracture-controlled effects is explained in terms of local compliance, as fractures introduce structural heterogeneity that impacts higher-order modes. This provides insight into why specific modes are more sensitive to localized fractures (spatial scales).

2. Concerning the temporal sensitivity of our approach, answering questions about long-term sensitivity would require extended monitoring to observe potential structural changes over time due to progressive fracturing. This is beyond the scope of our current study but represents a valuable direction for future research, however, was a point conceptually addressed at least by our damage evolution modelling

3. Lastly, we believe our conclusions outline key aspects of this approach that are broadly applicable to other structures or arches. We have, however, expanded this section to ensure clarity on what can be applied to similar geological formations.

Supplementary material

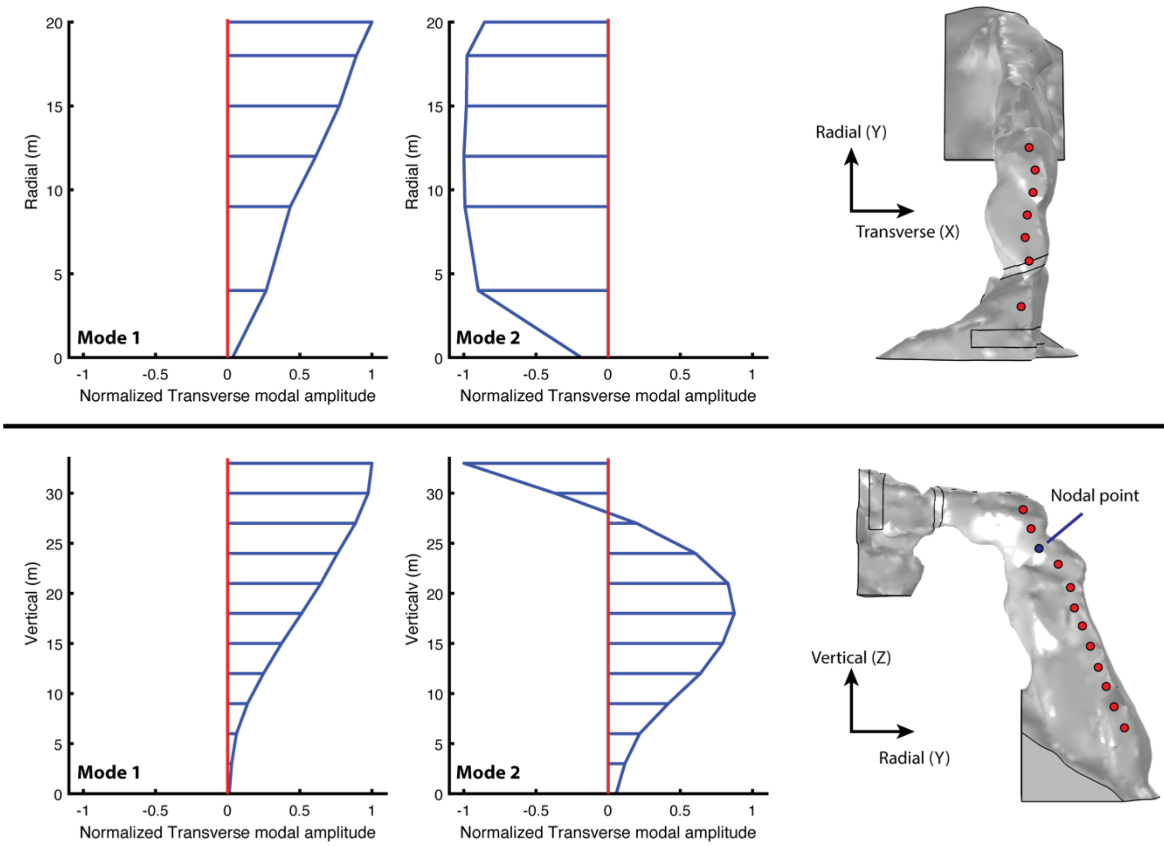

Fig. SM1: Normalized transverse modal displacement amplitude for mode 1 and 2 derived from numerical modeling. Transverse modal displacements are measured in correspondence with the red circles shown in the sketches: the upper and lower rows represent modal displacements in the XY plane and YZ, respectively.