Dear Dr. Heresi, Dr. D'Amico, and anonymous reviewer B,

Thank you for taking the time to review our article, we appreciate your feedback, assessment, and suggestions for improvement. We have addressed each of your comments in the article and shortly describe our response below in red text. Again, sincere thanks for a timely and constructive review.

On behalf of the authors,

Jeff Moore

Editor (Pablo Heresi):

The paper is very interesting and well-written. Based on the comments from the reviewers, I kindly ask you to revise and resubmit for a new round of reviews.

One of the reviewers suggests that the manuscript title, noting that insolation "drive(s) … resonance frequency drift", might be modified to allow for other potential causes of this effect. This is based on the fact that correlation does not imply causality, and therefore, we cannot be totally sure that insolation cycles are driving the patterns of resonance frequency. I tend to agree with the reviewer in a general sense, but I don't really want to force you to change the title. So, I suggest you find another verb for the title if you feel like it could be enhanced, but I leave this decision up to you.

Thank you for this feedback, it's certainly a fine line to find the right phrasing that conveys the point we seek to establish in the paper while allowing for uncertainty that remains based on the methods of analysis. We adjusted the title.

Reviewer A:

The paper deals with the monitoring of resonance frequencies of a large sandstone tower in Utah. The monitoring last two years and data were collected by means of a seismometer set to collect data the authors continuously. Temperature data were also recorded in-situ.

The authors propose that resonance frequency drifts at the investigated site are caused by material property changes rather than an opening and closing rear crack. In this regard, the paper (and the readers) will benefit from a simple chart/diagram that illustrates the method and the various steps of the modelling phases.

Here we would appeal that in this paper we use an established climate model, and thus we refer readers to the original references to understand the model and its different components. We did not develop the model for this study, and in fact only use a small portion of its capabilities to simulate insolation patterns, which is described in the text. For brevity, we aim to keep the description of the modeling steps to a minimum, however, we have added some further text to help clarify the approach.

Overall, the comprehensive and insightful results significantly contribute to the field. In general, the paper showcases a deep understanding and thorough analysis of the subject matter. The clarity of the writing, combined with the robustness of the research, provides valuable perspectives and I think it enhances our journal's reputation for high-quality content.

Thank you for these comments!

Finally, I think that the paper is robust and in a very good shape and it can be accepted for publication.

I waive anonymity.

With kind regards,
Sebastiano D'Amico – University of Malta

Recommendation: Accept Submission

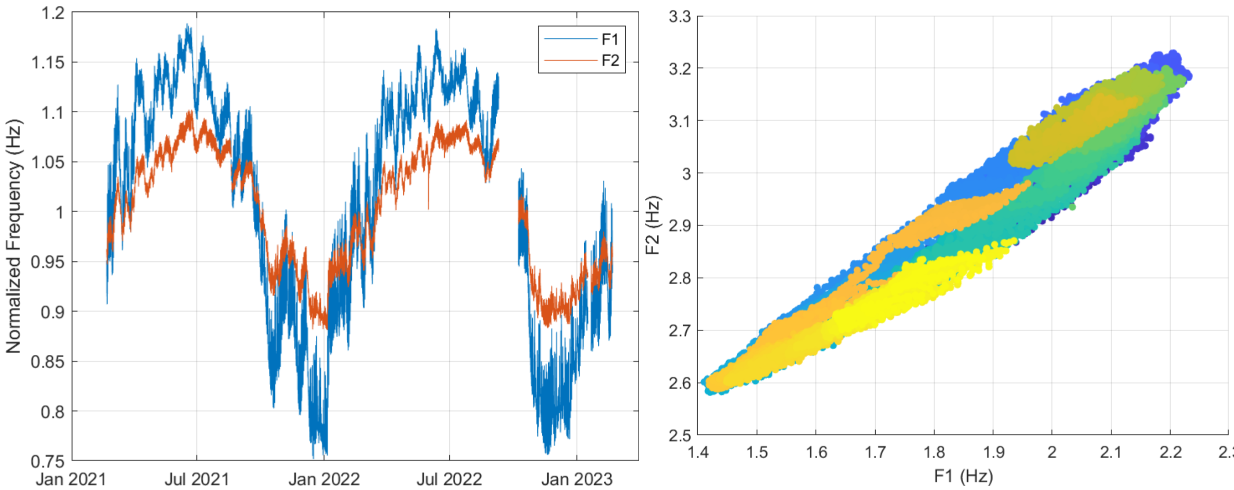
Reviewer B:

The authors analyze continuous seismic data from a seismometer deployed atop a 36-m-high sandstone tower and identify seasonal and daily variations in two resonance modes that have previously been associated with bending modes of the sandstone tower. The seasonal effects cause ~2-fold increases in the fundamental frequency between winter and summer months. They compare the changes in fundamental frequency of the resonance with air temperatures and solar insolation, finding that air temperature lags frequency changes by about 35 days, while variations in insolation are approximately in phase with the changes in the fundamental frequencies over the two year period of monitoring. They model insolation effects to remove cloud-coverage and other stochastic effects and find correlation between insolation and frequency changes for the resonance of the sandstone tower. This work is very interesting and appears to point to insolation as a novel environmental factor that can affect the daily and annual structural resonance effects.

Comments

- The first and second resonance frequencies show seasonal changes of factors of about 1.86 and 1.45. The authors comment that the normalized frequencies "exhibit nearly identical drifts" though these seem to be sufficiently different to imply differences in response of the tower over the year. If seasonal effects cause changes in stiffness of the tower, which translate to resonance frequency changes, and these effects were equally experienced by both bending modes, should this ratio be identical? Can the authors comment on this detail? Also, please clarify what is meant by "normalized" on line 153.

Good point, we were not clear. Indeed, the annual change in F2 is about 78% that of F1 as correctly identified. What we meant in that statement was that the annual *pattern* of drifts was the same, which is especially clear when the frequency time series are normalized (by their mean). See plots below:



This observation implies that the overall results of the analysis, e.g. see Figure 5, will be the same if we were analyzing F1 or F2 or both, so we aim to simplify the presentation and only discuss F1. However, that was not clear and needs clarified, which we have adjusted in the text. Moreover, the observation that F2 varies seasonally by an amount less than F1 is also an interesting observation that we now mention and discuss briefly.

- The daily trends in environmental variables and resonance frequencies (Figure 6) show the best correlation between resonance frequency and rock temperature, and rock temperature appears to best correlate with air temperature. The feature of these timeseries that is not captured by insolation is the long tail after the peak in temperature/insolation that also shows up in the resonance frequency timeseries. Can the authors explain how insolation is the driving factor despite insolation not including this long tail feature? Figure 6 shows that rock temperature timeseries best correlation with the frequency changes, suggesting that they drive these changes. The question I think you are trying to address is whether insolation or air temperature drives rock temperature, right? That air temperature timeseries also show this long-tail feature seems to be an important part of this discussion, and the authors should address this question more clearly.

Thank you for pointing this out, something we did not specify clearly in our previous text. The main thing we wish to point out is that the daily insolation cycles are controlling the *timing* of daily frequency drifts, both the onset of the daily rise and evening frequency drop. Indeed, the overnight tail of frequency decay closely mirrors that of rock temperature, as pointed out, reflecting diffusive processes not accounted for by insolation alone. This has been clarified in the text.

- Whether daily and seasonal changes in resonance frequency are driven by the same mechanism is unclear, and the discussion of this effect would benefit from some additional analyses. Air temperatures are demonstrated to not cause the frequency changes because the resonance effects precede temperature changes, and there are no known mechanisms that could explain this relationship. Annual insolation variations correlate with the changes in resonance frequency. Could there be other mechanisms causing these effects? For example, other studies have identified presence of fluids and water table level as important contributors to temporally varying seismic velocity. Have laboratory studies indicated that temperature variations of the scale reported here (~20-25 C) can change shear moduli sufficiently to explain changes in resonance frequency?

Here some of the clarification that arose from earlier points may also help clarify this aspect of the article. But in addition, we have added references to previous studies that show the magnitude of the daily and annual frequency wander we measured is within the measured range of previously reported values for rock slope instabilities and other (arch) sites. These past studies additionally highlight that water-infiltration affects are mostly small for rock masses, while ice formation can have a large affect but with notably inverse correlation that we do not observe at our site. We also discuss in the text how some of the differences between the daily and annual signals might be related to different diffusion skin depths, in that the daily signal only really involves the outer skin while we argue the tower heats farther through for annual temperature changes.

Minor comments:

- Abstract, line 15: include "resonance" between "daily" and "frequency"

Done.

- Please include brief description of the seismic instrumentation and previous results--did previous work identified that this resonance was associated with the tower by comparing the resonances from sites on and away from the tower. How was the station coupled with the tower to ensure resonances reflect structural effects?

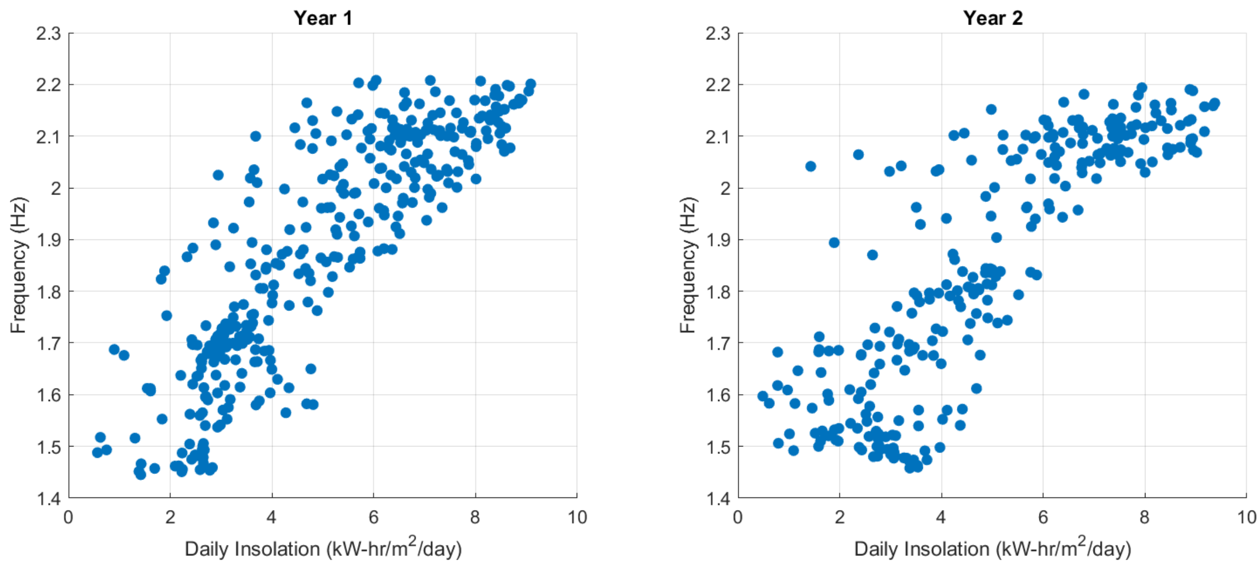
Requested information now included in the text. Yes, we did do site-to-reference measurements in an early deployment to confirm the localized measurement of the resonance modes on the tower only. The station was glued to the rock surface with an outdoor silicone adhesive, as was its protective cover, this is an approach we've successfully used at several other bare rock sites in similar environments.

- Line 93: Change to "instrument-response-corrected"

Done.

- Figure 2: The correlation between resonance frequency and insolation appears to be weaker for 2021 than for 2022. Have the authors considered this difference and can this observation be addressed in the manuscript.

Interesting observation. I guess to my eye I don't see a strong difference, but I can imagine that one cause of an apparent difference might some missing data in 2022 when seismometer battery issues sometimes prevented a full day of measurement and thus there are fewer daily average resonance frequency data points. Below is a quick plot of the data split by year. There are some things visible in year 2: a) bit fewer data in the mid-frequency region (for the reason above), and some larger outliers in the higher frequency region (summer) where insolation drops rapidly but frequency does not, times most likely associated with summer storms and periods of cloud cover. Overall, I tend to see from these a similar linear trend with some substantial periods of noisy data caused by changing cloud cover, keeping in mind that insolation shown in these plots is measured at a difference location than our tower site (as described in the text). Note that the linear fit coefficients for the two years' curves are very close (essentially identical slopes and intercepts).



Recommendation: Resubmit for Review