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Dr. Yen Joe Tan
Handling Editor
Seismica

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Response to "Seismoacoustic measurements of the OSIRIS-REx re-entry with an off-grid Raspberry PiShake"

Dear Dr. Yen Joe Tan

Many thanks for facilitating the reviews of this paper. The comments made by reviewers are helpful, and we are pleased to address them.

The changes are outlined in this document, with

Deletions in orange
Additions or modified quotes from the text in green
Comments in this file only (not in the manuscript) in blue.

Changes are also tracked in the attached re-submitted manuscript.

With best wishes,

Dr Benjamin Fernando (on behalf of the authors)

Reviewer A:

Summary Comments: This Report documents the installation of a low-cost seismic and acoustic sensor and the subsequent recording of the OSIRIS-Rex capsule re-entry. The authors describe the motivation, installation, and some notable features of the seismic and infrasound data. They note some interesting observations of these waves, in particular the shape and how it is consistent with previous similar events. They also derive some properties of the acoustic-seismic coupling and ground, and highlight their deployment and how it may be emulated elsewhere. Overall I think the manuscript is well-written, the figures are of good quality and help convey the results, and the manuscript is appropriate for a Seismic Report. I have some comments for the authors to better put their work in context of previous work and seismoacoustic installations and correct some terminology.

We thank the reviewer for their helpful comments, all of which have been addressed to improve the manuscript.

Major/General Comments:

1. Lines 126-131 discuss how the infrasound shape is very characteristic of a shockwave arrival, but I disagree. The infrasound signal doesn't look like an "N-wave" to me. It looks more sinusoidal or bipolar. An N-wave has a specific N-like shape with a near-vertical pressure rise and very sharp decrease in pressure, and often causes a double "boom". This signal is impulsive but not N-wave or shock-like compared to N-waves in the literature. Sonic (and hypersonic) aircraft often do not produce N-wave like infrasound signals at long distances. An early and good reference on this is:

Pierce, A.D., and Maglieri, D.J. ~1972. ''Effects of atmospheric irregularities on sonic-boom propagation,'' J. Acoust. Soc. Am. 51, 702–720.

This point is well-made, and we have added clarification that this signal is better described as showing 'rounded N-wave' behaviour, in line with the nomenclature from the reference above which we have also added. The sentence now reads:

"This shape is characteristic of a shockwave which has been distorted by propagation through a turbulent atmosphere (Pierce & Maglieri, 1972). It is very similar to previously recorded signals from hypersonic re-entries (e.g ReVelle et al, 2005)."

2. The downward first motion of the seismic is interpreted as "physical shaking of the instrument" but my impression is that it is more likely a downward motion of the ground by the pressure wave. In Novoselev et al (2020) they clearly show the this for a colocated surface seismometer (node) and infrasound sensor following theory from Ben-Menahem and Singh (1981). I

suggest closely looking at this and similar papers, and also to calculate the exact phase difference between the seismic and acoustic.

Novoselov, A., Fuchs, F., & Bokelmann, G. (2020). Acoustic-to-seismic ground coupling: Coupling efficiency and inferring near-surface properties. Geophysical Journal International, 223(1), 144–160. https://doi.org/10.1093/gji/ggaa304

We agree that the initial wording was clumsy and have rephrased this, as well as adding in a reference to *Ben-Menahem & Singh (1981)* which describes this phenomenon in more detail:

"This likely represents the shaking of the surface induced by the overpressure (Ben-Menahem & Singh, 1981)."

3. Regarding the seismic coda, I suggest looking at a recent paper by Willls et al (2022) (and Novoselov et al above) where they see a similar long coda and attribute it to a Rayleigh-wave like signal (actually a Stonely wave I think) in the soft sediments.

Wills, G., Nippress, A., Green, D. N., & Spence, P. J. (2022). Site-specific variations in air-to-ground coupled seismic arrivals from the 2012 October 16 explosion at Camp Minden, Louisiana, United States. Geophysical Journal International, 231(1), 243–255. https://doi.org/10.1093/gji/ggac184

We have added in these references, and explicitly mentioned the influence that the shallow sub-surface might have. This is now a separate paragraph which reads:

"Similar features, identified as Airy waves, are seen by Edwards et al (2007); whilst Novoselov et al (2020) identify Stoneley waves in seismic coda generated by seismoacoustic coupling. These propagate in the thin, low-velocity surface layers where the shear velocity approaches the acoustic wavespeed in air (Wills et al, 2022). This is comparable to the geological setting here, with the PiShake sensor resting on a low-velocity alluvial layer."

4. For your coupling-ratios and ground compliance, I suggest looking at additional references on this topic to provide a broader context and discussion of how it is highly dependent on many factors, such as arrival angle, frequency, etc. Otherwise yes I agree your estimates are consistent with previous studies, so I think good to put into context for the reader:

Matoza, R. S., & Fee, D. (2014). Infrasonic component of volcano-seismic eruption tremor. Geophysical Research Letters, 41(6), 1964–1970. https://doi.org/10.1002/2014GL059301

Bishop, J. W., Fee, D., Modrak, R., Tape, C., & Kim, K. (2022). Spectral Element Modeling of Acoustic to Seismic Coupling Over Topography. Journal of Geophysical Research: Solid Earth, 127(1), e2021JB023142. https://doi.org/10.1029/2021JB023142

We have added in a note to this effect, this section now reads:

"We note that measurements of seismoacoustic coupling strength are in general very sensitive, in particular to the frequency bands considered, surface topography, and wavefront shape/incident angle (Matoza and Fee, 2014; Bishop et al 2022)."

5. Lines 196-7 note how you measured "non-linear acoustic wavetrain" features of similar quality to other studies. First, I do not think you are actually seeing nonlinear waves here, but rather the linear expression of a nonlinear source recorded at a relatively far distant. I think you have a nice recording here but I don't think you have established "comparable quality" or at least should be cautious here without a more thorough comparison. Colocating or comparing directly the recordings would provide more direct evidence for this, and rule out any inconsistencies such as the nonlinearity of the instrument response, etc.

We have edited this section to be more considered and nuanced, specifying which features of the wavetrain we record which are comparable to those seen in other studies, without suggesting that a single-station has comparable quality to broader-band instruments or arrays. This section now reads:

"Whilst naturally limited in sensitivities to long periods (<1 Hz), this work also demonstrates the ability of the PiShake instrument to capture many of the notable features in the wavetrain, from the initial rounded N-wave to the coda likely associated with Stoneley waves propagating in the low-velocity subsurface. Whilst our single station does not offer the same seismic insight as arrays or co-located broader spectrum instruments would, these features of the wavetrain are recorded comparably to past studies."

6. I would also encourage a more careful comparison with other remote, real-time deployments at the end of the manuscript. Multiple geophysical station types exist that transmit data over radios, cell networks, and satellite (e.g. BGAN) in real-time using low power and do not require a generator. An example:

Busby, R. W., & Aderhold, K. (2020). The Alaska Transportable Array: As Built. Seismological Research Letters, 91(6), 3017–3027. https://doi.org/10.1785/0220200154

We have added this as an example to the end of the manuscript, “Such potential has already been demonstrated with conventional seismometers (Busby & Aderhold, 2020) but not to our knowledge with PiShake-type arrays.”

Minor or specific comments:

7. Line 21: How do you know you were able to record “all the salient features”? I would suggest rephrasing to record some “notable features” or something. There may be signals out of band or present on more sensitive/lower noise instruments that you are unaware.

We have rephrased this in line with the changes made to point 5 above; the sentence now reads:

“...the ability of the PiShake instrument to capture many of the notable features in the wavetrain, from the initial rounded N-wave to the coda likely associated with Stoneley waves propagating in the low-velocity sub-surface.”

8. 40: Add “and acoustic” after “seismic”? I think the acoustic data arguably add more information.

This has been added, the sentence now reads:

“In each case, seismic and acoustic measurements enabled information about the capsule’s hypersonic dynamics and propagation of the sonic boom shockwave to be collected”.

9. 57: Can you quantify what you mean by “far” here? Also suggest adding something about existing acoustic networks in the region (or lack thereof).

We have clarified that the nearest permanent seismometer is located more than 50 km away. The sentence now reads:

“The nearest permanent seismic station was 50 km away (NN.Q11A at Duckwater, Nevada), precluding the use of an existing seismic network to provide local data. Similarly, no permanent infrasound stations were located nearby.”

10. 58: Where did the “restrictions” come from…the funding source? Live-streaming is not typical for geophysical data (perhaps real-time but I think you mean something different here).

We have changed this to “constraints” to be more clear. By live-streaming in this context, we mean that the data were only available over the internet (no on-site local readout) and that this was done with effectively no lag time (>1s). This was specifically for outreach/educational purposes.

11. 82: Suggest not speculating that the hills may have influenced the coda here, and wait until you discuss it in results/discussion.

We have removed the following part of the sentence:

" may also have influenced the seismoacoustic coda recorded".

12. 127: Remove "extremely"

We have removed this, the sentence now reads:

"with a rapid overpressure (0.7 Pa) pulse and sharp peak followed by an underpressure trough (0.6 Pa), lasting approximately 0.5 s total. "

13. 132-4: The suggestion that the elevated infrasonic power levels are from a "rumble" seems fairly speculative. I would think it could just as likely be wind noise, and perhaps an evaluation of noise levels over time might be helpful here.

We have added in the caveat that this may simply be due to wind noise, and a reference to the portion of Fig 3 that we are looking at. The sentence now reads:

"The background infrasound noise level appears to be slightly enhanced at low frequencies (<10 Hz) following the arrival as compared to before, though not enormously so (Fig. 3, ~5 s before and after the infrasound arrival). This feature may be the signature of a low-frequency sub-audible infrasonic rumble, or alternatively may simply be associated with elevated wind noise"

Figures:

14. Add a) and b) labels to each panel. Add lat/lon and more details to map. Note that blue text indicates path altitude.

We have added labels and lat/long as suggested. The figure caption now reads:

"Schematic views of the pre-landing projected ORX EDL path in blue, showing top-down (upper panel) and side-on (lower panel) views. Capsule heights above sea level are indicated along the trajectory. The total length of the path flown after atmospheric interface is approximately 1500 km."

15. Remove "highly" before "dispersive". It looks like fairly standard dispersion to me.

This has been done, the sentence now reads:

"A short, dispersive chirp-like signal is apparent between approximately 1 and 7~Hz in the 3-4~seconds following the initial seismic arrival"

Reviewer B:

1. Introduction omits any historic use or precedence for using acoustic sensors to record or measure these events, despite it being the focus of this paper. Acoustic deployments are outlined in Silber et al., 2023, and should be summarized here as well to motivate the use of both seismic and acoustic sensors.

As per response to point 8 from Reviewer 1 discussed above, we have edited the manuscript to be clearer that both seismic and acoustic data are useful, both in past recordings and this one:

The entry, descent, and landing (EDL) of artificial spacecraft can serve as an analogue for these natural meteoroid events, enabling calibration of seismoacoustic measurements using an object of known trajectory, mass, and dimensions (Silber & Bowman, 2023).

In each case, seismic and acoustic measurements enabled information about the capsule's hypersonic dynamics and the propagation of the sonic boom shockwave to be collected.

Papers cited include Yamomoto et al (2011), ReVelle et al (2010), and ReVelle & Edwards (2010); both of which include and discuss acoustic measurements as well.

2. It would additionally be beneficial to discuss why a co-located sensor was chosen, and some of the background work focused on seismo-acoustic coupling, which is a major conclusion of the paper and not well introduced. Some additional references for the authors to consider:

Novoselov, A., Fuchs, F., & Bokelmann, G. (2020). Acoustic-to-seismic ground coupling: coupling efficiency and inferring near-surface properties. Geophysical Journal International, 223(1), 144-160.

Wills, G., Nippress, A., Green, D. N., & Spence, P. J. (2022). Site-specific variations in air-to-ground coupled seismic arrivals from the 2012 October 16 explosion at Camp Minden, Louisiana, United States. Geophysical Journal International, 231(1), 243-255.

Stevanović, J., Teanby, N. A., Wookey, J., Selby, N., Daubar, I. J., Vaubaillon, J., & Garcia, R. (2017). Bolide airbursts as a seismic source for the 2018 Mars InSight mission. Space Science Reviews, 211, 525-545.

Langston, C. A. (2004). Seismic ground motions from a bolide shock wave. Journal of Geophysical Research: Solid Earth, 109(B12).

Albert, D. G., Taherzadeh, S., Attenborough, K., Boulanger, P., & Decato, S. N. (2013). Ground vibrations produced by surface and near-surface explosions. Applied Acoustics, 74(11), 1279-1296.

Chen, T., Larmat, C., Blom, P., & Zeiler, C. (2023). Seismoacoustic Analysis of the Large Surface Explosion Coupling Experiment Using a Large-N Seismic Array. Bulletin of the Seismological Society of America, 113(4), 1692-1701.

Further to the response to point 1 above, we have added the following sentence to highlight why both seismic and acoustic measurements are particularly useful when made together:

Exact co-location of acoustic and seismic measurements enables estimation of coupling parameters across the surface interface, helping to constrain how incident acoustic signals produce their seismic counterparts. This is particularly useful when detecting natural meteoroids given that the worldwide seismic network is much denser than its acoustic equivalent.

3. Given the sonic-boom like results from the infrasound data, the introduction could additionally be supported with an overview of some of these observations, expected signals, etc.

Using the references suggested above, we have added this into the methodology: location, which now reads:

At a location this distance from and altitude below the EDL track, we anticipated detection both of the direct sonic boom (on the acoustic sensor), and the induced deformation of the ground (on the seismometer). It was also expected from published literature analysing conventional explosive sources that further features might be detected in the seismic coda, for example coupled surface waves (Novoselov et al, 2020; Langston 2004). Previous work indicates that these observations are site-specific and hence not a given, with a dependence on both local ground properties and current atmospheric conditions (Wills et al, 2022; Chen et al, 2023).

4. Was any wind noise reduction deployed for the infrasound sensor?

There was no physical wind noise reduction deployed for the sensor on-site, however we have added in additional suggested wind noise reduction measures to the manuscript. For full details, see response to point 9 below.

5. Line 114 – ‘elevated significantly above the background noise’ – I would like to see either a longer snippet of data prior to the arrival or a spectrogram to

confirm that this is indeed an arrival above background noise to support this claim.

We have addressed this in our response to Reviewer A's point 13 above - in short, we cannot be sure that this is a 'rumble' and it may just be background noise. Both possibilities are now expanded upon, and we include a reference to the specific feature being looked at in Fig. 3:

"This feature may be the signature of a low-frequency sub-audible infrasonic rumble, or alternatively may simply be associated with elevated wind noise"

6. Similar comment for line 132 – I do not feel that enough background noise data is shown to justify these claims.

This point is addressed in response to point 5 above with the addition of further background noise plots.

7. Similarly, the Seismo-acoustic noise section is interesting, but is not supported by figures or examples. Either remove this section entirely or support with figures and observations to be included in the manuscript.

We have added in specific references to features in Fig. 3 to highlight the phenomena that we are discussing. This section now reads:

Strong resonances in both instruments are observed at 30 Hz, with weaker resonances in the seismic data at 11 Hz and 19 Hz. These appear as horizontal lines in Fig. 3.

…These are also thought to be electromagnetic glitches associated with rapid changes in the generator's load. These appear as vertical spikes in Fig. 3.

Corresponding comments are also made in the caption to Fig. 3.

8. Air-to-ground coupling section beginning line 157 is missing references – where do equations originate from, where are the alluvium values originating from?

The equations follow Kenda et al (2022), and are now referenced:

Following Kenda et al (2020), the compliance Kv is then…

Furthermore, the seismic parameters are taken from published literature from the Great Basin Desert. We have clarified this and added in a sentence to reference it:

These are commensurate with detailed surveys from the wider region (Allander & Berger, 2009).

9. Deployment suggestions – why no comments about 1) lack of seismic sensor anchoring into the ground and 2) lack of infrasound wind noise reduction. Both of these would contribute significantly to data quality and I am surprised they are not mentioned.

We have added two suggestions to this end:

- The addition of a wind cover to the instrument would likely substantially reduce the noise levels of both the acoustic and seismic data.
- Better anchoring of the instrument into the ground would be expected to especially benefit the seismic data.

10. The stated goal of this study is to co-located seismoacoustic sensors with an optical tracking station and examine correlation. No data from the optical station is included and therefore the project objective is not met within this paper. While I feel the observations themselves warrant a publication, section 1.3 must be revised to more accurately state the outcome of the manuscript.

We are interested longer-term in looking at whether light curves can ever be correlated with infrasound readings directly for meteors (this has been suggested, though somewhat dubiously, by Siraj & Loeb (2023), "Localising the first interstellar meteor with seismometer data"). However, the light-curve data from the OSIRIS-REx EDLis from another team and has not yet been released and therefore we do not feel we can comment or mention the results yet, hence the removal above. Acknowledging this, we have revised the stated paper aims such that they now read:

This project aimed to co-locate a seismoacoustic station with an optical tracking station closer to the point of peak heating, in order to study the re-entry process at the point where the maximum amount of kinetic energy is being dissipated into the atmosphere. In order to evaluate whether any correlation between the two sets of measurements existed.

<u>Further changes not suggested by reviewers:</u>

We have included an estimation of the carbon cost of this work, and added this into a new section (Sec. 6). We realise that this is not common in seismology, but believe it is an important step toward research sustainability.