Review by Ronan LeBras

This is a well-written manuscrit about the exceptional Hunga, Tonga, event of January 2022, recorded by an exceptional network of seismometers, infrasound sensors, and barometers, that happened to be in place in Alaska at the time of the event. I have written a few comments in the text. These are to be found in the attached annotated PDF of the submitted manuscript. In addition to the comments in the text, it would be good for the authors to address the following point:

One result of the analysis comparing seismic and infrasound recording is that while the vertical seismic velocity is shown to match the 90-degree-shifted infrasound recording, the radial and tangential seismic components do not match the infrasound recording. I have several remarks around this major results demonstrated in the manuscript:

(1) It would be good to make it very clear that the infrasound trace shown in the insets in Figure 8 is different in the top figure, when it is compared with the vertical seismic velocity trace. To emphasize the point best that they are indeed different, it would be better to show two insets when comparing it to the vertical velocity trace. The first would show the actual pressure trace and the vertical velocity component, and the other would be the 90-degree (Hilbert transformed) pressure trace and the same vertical vertical velocity component.

(2) Beyond showing the inconsistency of the observations with the theory of Sorrells (1971) and Ben-Menachem and Singh (1981), it would be good to evoke possible tracks to further explain these discrepancies. Are there limitations to the applications of the theory? Is the Lamb wave of the Hunga, Tonga event so different from the majority of previous observations that it warrants an entirely new theory?

On other points of detail:

I have checked that all referenced papers are cited in the text.

The figures are generally of good quality. I do believe that my above remark regarding Figure 8 would improve the visualization aspects of the manuscript.

Additional comments from an annotated manuscript file

Section 1.1, “Hunga volcano in the Kingdom of Tonga”
*Although most readers will know which volcano and eruption you are referring to, it would be good to mention the various names of the volcano and associated 2022 eruption that can be found in the literature. The Global Volcanism program of the Smithsonian Institute, an influential institution for volcanism uses the name Hunga Tonga- Hunga Ha’apai. The two parts of the composed name refer to two distinct islands previous to the eruption. See https://volcano.si.edu/volcano.cfm?vn=243040

Section 2, “which may be estimated as a function of temperature via the expression c = 331.3 m/s + 0.6 m/s/◦ C × T emp where T emp is the local air temperature in degrees Celsius (°C). We downloaded mean hourly surface temperature data for the Alaska region from the ERA5 reanalysis product f”
*Please provide a reference for this equation or show how it was derived

Review by Toshiro Tanimoto

This study analyzes pressure and seismic data from Alaska and derives the shallow velocity structure at each station through a combined analysis of these datasets. The analysis has been conducted carefully. The results contain innovative aspects, particularly in the higher-frequency range, making this paper suitable for publication. I have some suggestions for revisions and clarifications, but with reasonable responses to these requests, the paper should be published.

Questions and Comments

1. **Pages 6-7:** The PSD calculation (Welch's method) is clearly explained. However, I would like additional details on the coherence calculation, as the selection of time windows for stacking can influence the estimates. A brief description would suffice.

1. **Figures 3 and 4:** The coherence results for the high-frequency range (>1.0 Hz) appear somewhat low in Figure 3a (approximately 0.4-0.5), whereas coherence for the same frequency range appears high in Figure 4a. From the context of this paper, I assume coherence should be high (>0.8). A brief explanation of the seemingly low coherence in Figure 3a would be helpful.

1. **Page 11, Lines 277-278:** It seems that Figure 3a in line 277 and Figure 3b in line 278 should actually be Figure 4a and Figure 4b, respectively. Please verify and correct if necessary.

1. **Page 17, Lines 354-360:** The signals from the Hunga-Tonga eruption are Lamb waves, which behave like surface waves (for pressure) on the atmospheric side. Modal analysis indicates that vertical and horizontal components are phase-shifted by 90 degrees. Therefore, the low correlation observed is likely due to the time-shifting of pressure data. The correlation should be high before the time shift for horizontal data. You might consider modifying this paragraph accordingly, as this result does not seem to be surprising.
2. **Page 18, Section 4.1:** The analysis based on horizontal seismic data is compelling. However, in the low-frequency range (0.01-0.05 Hz), tilt effects could be problematic. For seismic noise data, these effects are significant and often insurmountable. The situation may be more manageable for Lamb wave data. For frequencies above 1 Hz, tilt should not be a problem, and you may be able to obtain clean results from horizontal data. That seems quite exciting to me.
3. **Page 30, Section 4.3:** The discussion on data from the Chelyabinsk bolide is particularly interesting. But the signals from a bolide start as shock waves which quickly turn into regular sound waves but strictly speaking we do not know the speeds accurately. I agree we should study these data from bolides but we might need to trod carefully.
4. The structural results should be published in tabular form (for each station), perhaps as a supplementary file. This would be valuable for other researchers (including myself) and would likely increase citations to your paper.

Reviewer Comments 1

For author and editor

This is a well-written manuscript about the exceptional Hunga, Tonga, event of January 2022, recorded by an exceptional network of seismometers, infrasound sensors, and barometers, that happened to be in place in Alaska at the time of the event. I have written a few comments in the text. These are to be found in the attached annotated PDF of the submitted manuscript.

Corrections from annotated manuscript:

- *Line 101: Name of Volcano*
  We changed to the suggested longer title of the volcanic complex in the abstract and in the introduction. We then adopt an acronym of "HTHH" for the remainder of the text. We agree that this adds more specificity and hopefully avoids confusion, and believe that reverting to the acronym for the rest of the text enhances readability.
- *Line 195: Derivation or reference for estimate of sound speed as a function of temperature*
  Thanks for catching this oversight. We have added a short description of the expression along with a reference.
- *Line 217 (and others): Correct notation for Lame parameter*
  Thank you for this catch. We have corrected our notation.
- *Line 392: Fix spelling typo*
  Thank you for this catch. We have fixed this typo.

In addition to the comments in the text, it would be good for the authors to address the following point:

One result of the analysis comparing seismic and infrasound recording is that while the vertical seismic velocity is shown to match the 90-degree-shifted infrasound recording, the radial and tangential seismic components do not match the infrasound recording. I have several remarks around this major results demonstrated in the manuscript:

(1) It would be good to make it very clear that the infrasound trace shown in the insets in Figure 8 is different in the top figure, when it is compared with the vertical seismic velocity trace. To emphasize the point best that they are indeed different, it would be better to show two insets when comparing it to the vertical velocity trace. The first would show the actual pressure trace and the vertical velocity component, and the other would be the 90-degree (Hilbert transformed) pressure trace and the same vertical vertical velocity component.

Thank you for this suggestion, we really like the idea of emphasizing the phase delay like this! We found that the additional inset was crowding the waveforms a bit, so rather than use insets, we expanded the figure from 4 panels to 8. The new panels include one for the non-phase delayed vertical seismic, one for the phase-delayed vertical seismic as suggested, as well as panels for the zoomed-in radial and tangential components. Because the figure is now considerably larger, we separated the waveform plots for M26K and TOLK into dedicated figures (now figures 8 and 11).

(2) Beyond showing the inconsistency of the observations with the theory of Sorrells (1971) and Ben-Menachem and Singh (1981), it would be good to evoke possible tracks to further explain these discrepancies. Are there limitations to the applications of the theory? Is the Lamb wave of the Hunga, Tonga event so different from the majority of previous observations that it warrants an entirely new theory?

These are great questions! We think that the theory is sound and that the inconsistencies are likely a result of observational conditions. Specifically, we suspect that the horizontal channels at the stations showing departure from theory are contaminated by tilt noise. We have expanded our discussion under the "Observational departures from theory" section to address this, and added a figure (Figure 9) that provides evidence for the tilt theory. Figure 9 shows map views of vertical-infrasound and radial-infrasound correlations, as well as an example of a station that exhibits poor radial correlation for a surface sensor and good correlation for a colocated borehole sensor. The implication here is that the seemingly random spatial distribution of stations with poor pressure-radial correlations, along with an example showing poor correlation for a surface sensor vs a borehole sensor, indicates contamination by tilt noise. We find this evidence compelling but not definitive, but think further work towards supporting the tilt hypothesis is out of the scope of the paper. We also added text to the conclusion section summarizing that we believe tilt is the most likely explanation rather than an issue with the half space theory.

On other points of detail:

I have checked that all referenced papers are cited in the text.

Thank you!

The figures are generally of good quality. I do believe that my above remark regarding Figure 8 would improve the visualization aspects of the manuscript.

We agree that this suggestion has improved the visualization considerably.

Reviewer Comments 2

For author and editor

Review of "Alaska Upper Crustal Velocities Revealed by Air-to-Ground Coupled Waves from the 2022 Hunga, Tonga Eruption" by K. A. Macpherson, D. Fee, S. Awender, B. Chow, J. Colwell, S. Delamere, and M. Haney.

This study analyzes pressure and seismic data from Alaska and derives the shallow velocity structure at each station through a combined analysis of these datasets. The analysis has been conducted carefully. The results contain innovative aspects, particularly in the higher-frequency range, making this paper suitable for publication. I have some suggestions for revisions and clarifications, but with reasonable responses to these requests, the paper should be published.

Questions and Comments

1. Pages 6-7: The PSD calculation (Welch's method) is clearly explained. However, I would like additional details on the coherence calculation, as the selection of time windows for stacking can influence the estimates. A brief description would suffice.
   Thank you for catching this, as we inadvertently skipped it. We have modified the paragraph that describes the PSD and coherence calculations to include details on coherence calculations (we use the same parameters as for our PSD calculations)
2. Figures 3 and 4: The coherence results for the high-frequency range (>1.0 Hz) appear somewhat low in Figure 3a (approximately 0.4-0.5), whereas coherence for the same frequency range appears high in Figure 4a. From the context of this paper, I assume coherence should be high (>0.8). A brief explanation of the seemingly low coherence in Figure 3a would be helpful.
   Good point and we did not explain this adequately. The low high frequency coherence in Fig 3a is a result of the large amount of scatter across the network, as some stations have very good high frequency coherence and others do not. We have added an explanation of this to the paragraph beginning on line 170. The high-frequency coherence in Fig 4a looks more pronounced as we "gray-out" low coherence values.

3. Page 11, Lines 277-278: It seems that Figure 3a in line 277 and Figure 3b in line 278 should actually be Figure 4a and Figure 4b, respectively. Please verify and correct if necessary.
   Thank you for catching this! This was a significant typo and has been corrected.

4. Page 17, Lines 354-360: The signals from the Hunga-Tonga eruption are Lamb waves, which behave like surface waves (for pressure) on the atmospheric side. Modal analysis indicates that vertical and horizontal components are phase-shifted by 90 degrees. Therefore, the low correlation observed is likely due to the time-shifting of pressure data. The correlation should be high before the time shift for horizontal data. You might consider modifying this paragraph accordingly, as this result does not seem to be surprising.
   We were not clear in our description of what was being cross-correlated. We phase delayed the pressure before cross-correlating with the vertical seismic, but we did not phase delay the pressure before cross-correlating with the radial component. This is why the low vertical-radial correlation value is surprising. We have now stated this explicitly in the paragraph, and in the figure captions. Thank you for catching our ambiguous wording!

5. Page 18, Section 4.1: The analysis based on horizontal seismic data is compelling. However, in the low-frequency range (0.01-0.05 Hz), tilt effects could be problematic. For seismic noise data, these effects are significant and often insurmountable. The situation may be more manageable for Lamb wave data. For frequencies above 1 Hz, tilt should not be a problem, and you may be able to obtain clean results from horizontal data. That seems quite exciting to me.
   We think so too! We added a discussion of tilt contamination to this paragraph, and pointed-out that a higher-frequency analysis using horizontal data may be possible. We then reiterate that using the vertical only with simplified equations is viable and gives robust results.

6. Page 30, Section 4.3: The discussion on data from the Chelyabinsk bolide is particularly interesting. But the signals from a bolide start as shock waves which quickly turn into regular sound waves but strictly speaking we do not know the speeds accurately. I agree we should study these data from bolides but we might need to trod carefully.
   Agreed! We were a bit light on some details in the bolide sections. We have added a note about the complexity of infrasound generation from bolides with an additional citation. We have also referenced work that shows that IMS array detections computed acoustic trace velocities. Finally, we now mention that the over 6,000 km distance from the source at these stations make the plane wave assumption reasonable.

7. The structural results should be published in tabular form (for each station), perhaps as a supplementary file. This would be valuable for other researchers (including myself) and would likely increase citations to your paper.
   This is a great idea! We are adding a CSV file with the max peak-to-peak pressure, coupling ratios, rigidity estimate, and velocity estimate for each of the three bands at each station.

   Toshiro Tanimoto

Editor Comments

Beyond the two reviews sent by the experts (including one annotated PDF, which I'll share via a Discussion note due to its large size), I would like to ask you to check a geological map of Alaska, and whether the hard-rock / sedimentary basin sites show respectively higher / lower velocities in the uppermost crust. Such a comparison would be a more convincing result than that with the CRUST model that is rather coarse.

Thank you for this suggestion. However, we believe that there is some confusion as we do not compare our results to CRUST1.0. We do use CRUST for density estimates so that we do not use the density estimates from the model we are comparing to. For our 2 km and 5 km velocity estimates, we compare the a tomographic model for Alaska from Berg *et al.*, 2020. We believe this to be an ideal model for comparison because it has good spatial (0.2x0.1 degree) resolution and very good depth resolution (250 meter thick layers above a depth of 5 km).

For our shallow estimates, we compare the USGS proxy Vs30 map, which has 30 arc-second resolution.

We have added the spatial resolution to our text description of the Berg and USGS velocity models in the “Existing velocity models for comparison” section.

We do agree that there should be a relationship between our shallow Vs estimates and outcropping geological units from the Alaska geological map. We have included a box plot showing our shallow Vs estimates as a function of simplified rock type (we reduced 24 categories, many of which just had a few entries, to 3; sedimentary, crystalline, and till) in the supplementary material, and see the expected relationship of crystalline rocks having the highest median value, followed by sedimentary and the till.