Response to Reviewer File for “The Influence of Shallow Subsurface Properties on Particle Motion in Acoustic-Seismic Coupling”

We thank the editor and reviewers for their helpful comments that improved this manuscript. Below you will find a copy of each review along with our responses to individual points. Responses will be in blue below each comment.

Note that the inclusion of a U.S. Geological Survey coauthor required a separate internal review. That review is also included at the bottom of this document and any changes to the manuscript have been noted.

Best,
Logan Scamfer and coauthors

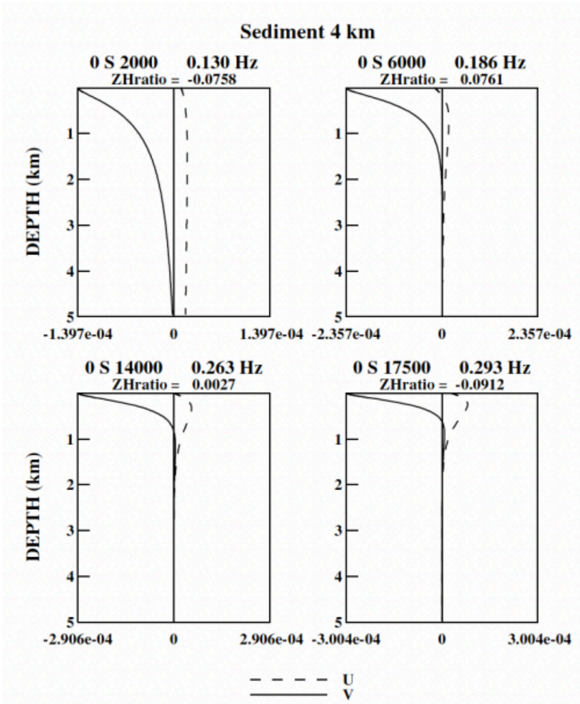
Reviewer A

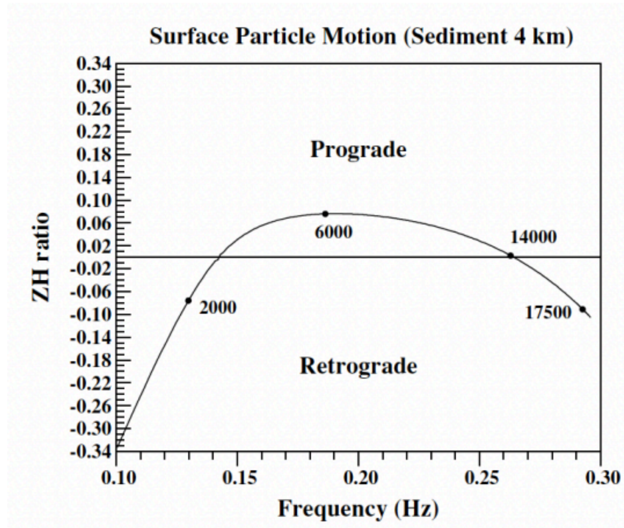
Review of “The Influence of Shallow Subsurface Properties on Particle Motion in Acoustic-Seismic Coupling” by L. Scramfer, D. Fee, J. Bishop, M. Haney, and K. Macpherson.

This study utilizes data from the 2012 Camp Minden explosion, recorded by the EarthScope Transportable Array, to analyze surface particle motion in air-to-ground coupled waves. The paper presents a method for measuring particle motion, demonstrating a clear correlation with shallow seismic velocity: the prograde particle motions are observed in the near-surface, low-velocity region while the retrograde particle motions are observed in high-velocity regions. I find this finding extremely interesting and think that this paper should definitely be published in SEISMICA.

I do have one request for the authors to add some explanation on the nature of this particle motion difference. I think this paper is overall written very well but since I am requesting to add some information, I am putting this paper in the category of major revision. But a relatively short description with a figure will suffice to satisfy my request.

Details of my question
Let me elaborate on the core of my request. While the paper currently emphasizes the correlation between particle motion and shallow seismic velocity, the transition between prograde and retrograde motion can also occur at a single, fixed location as a function of frequency. To be more precise: at high-velocity hard-rock sites, prograde motion likely never occurs at any frequency. But, at soft-sediment sites, it is much more probable that different frequency ranges will exhibit distinct prograde and retrograde behaviors.
As an illustration, consider the case of Rayleigh waves published in Tanimoto and Rivera (2005). The figures below (reproduced from Fig. 4 and Fig. 5 of that study) demonstrate this effect in a relatively thick (4 km) sedimentary layer.





The figure on the left illustrates the vertical (U, dashed line) and horizontal (V, solid line) eigenfunctions within the upper 5 km of the Earth. The nature of the particle motion is determined by the ratio U/V: a negative ratio indicates retrograde motion, while a positive ratio indicates prograde motion. Results are presented for four specific frequencies: 0.13 Hz, 0.186 Hz, 0.263 Hz, and 0.293 Hz.

These plots demonstrate that the transition in particle motion is primarily driven by changes in the surface value of the vertical eigenfunction (U). Specifically, the surface value is positive at 0.13 Hz, becomes negative at 0.186 Hz and 0.263 Hz, and returns to positive at 0.293 Hz.

The figure on the right provides further detail, showing that prograde motion occurs between approximately 0.14 Hz and 0.26 Hz, while retrograde motion is observed below 0.14 Hz and above 0.26 Hz. The fundamental mechanism is the sign of the vertical eigenfunction at the surface, which undergoes two zero-crossings at about 0.13 Hz and 0.30 Hz. Given these observations, my question is whether a similar frequency-dependent feature can be identified in the air-coupled modes analyzed in this paper.

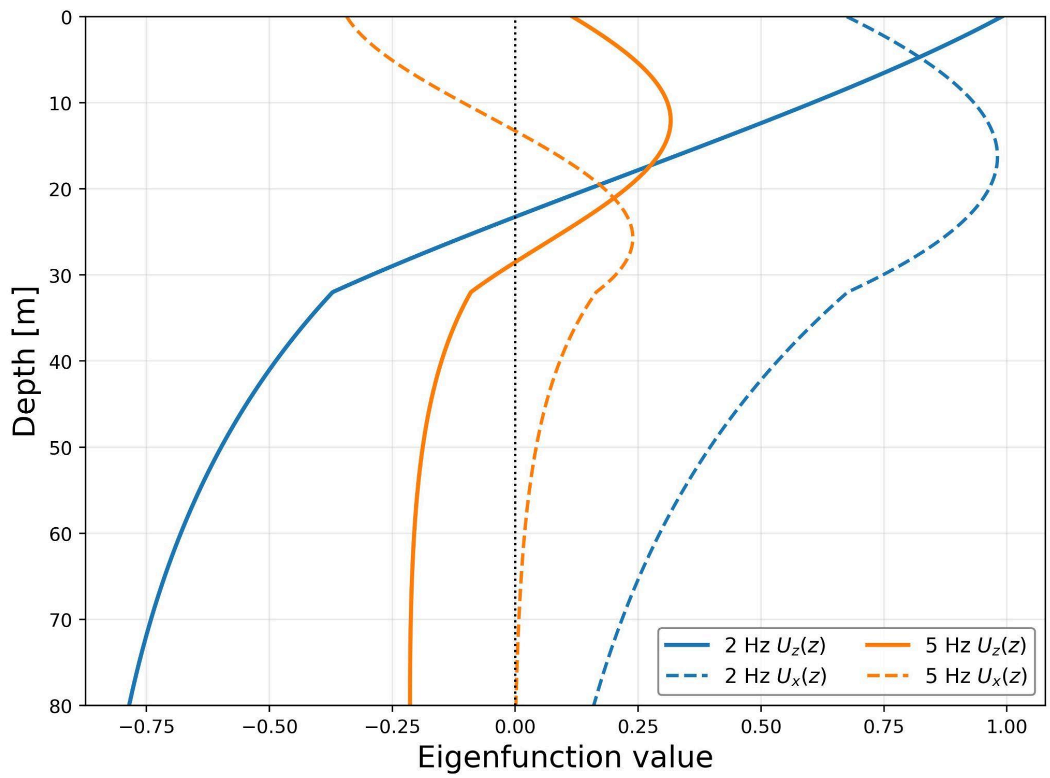
We thank the reviewer for raising this point. We agree that frequency-dependent eigenfunction behavior is important to examine when interpreting particle motion and its transitions between prograde and retrograde motion. In the air-to-ground coupling case we examine here, we observe similar behavior to those of Rayleigh waves described in Tanimoto and Rivera (2005), however there are notable differences that we present below.
To further highlight the importance of this analysis, we have added a few paragraphs to the text (Methods section 3.3 and Discussion section 5.2) and a figure to the supplemental information

to provide a more comprehensive examination of particle motions using eigenfunctions for the readers, and highlight their relevance.

Based on these observations, I have one request and a question for the authors.

1. Take the case in Figure 9 (Figure S6). It clearly shows that there is a major change in the compliance parameter that occurs at about 3 Hz. Please make plots for the vertical eigenfunctions at two selected frequencies below and above 0.3 Hz. Discuss changes in them in a paragraph or two.

In the attached figure below we compute the real part of the vertical (solid line) and horizontal (dashed line) eigenfunctions at 2 Hz (blue), and 5 Hz (orange) from the surface to a depth of 80 m. These eigenfunctions are for the best-fitting subsurface model for Z43A, shown in Figure 8 in the main text, whose waveforms and compliance are shown in Figure 9. Note we use U_x for the horizontal eigenfunction and U_z for the vertical eigenfunction.



The methodology to compute the eigenfunctions at depth is briefly described below:

Following Methods Section 3.3 of the manuscript, we showed how to solve for the surface eigenfunction vector $\vec{f}(z_0)$ for a frequency ω. To solve for the first two components, horizontal (U_x) and vertical (U_z), of the eigenfunction vector at depth, we employ the propagator matrix $\boldsymbol{P}$.

For a location at depth z in layer 1, the eigenfunction vector is obtained by

$$\vec{f}(z) = \boldsymbol{P}_1(z - z_0)\, \vec{f}(z_0), \tag{1}$$

where $\boldsymbol{P}_1$ is the propagator matrix describing the elastic properties of layer 1.

If the depth z lies below the first layer, the propagation is performed through the layer and then into the halfspace:

$$\vec{f}(z) = \boldsymbol{P}_{\mathrm{HS}}(z - h)\boldsymbol{P}_1(h_1)\vec{f}(z_0), \tag{2}$$

where h is the thickness of the first layer and $\boldsymbol{P}_{\mathrm{HS}}$ is the propagator matrix describing the underlying halfspace.

The horizontal and vertical displacement eigenfunctions are the first two components of the vector.

Earlier, the reviewer noted that in Tanimoto and Rivera (2005) *"the transition in particle motion is primarily driven by changes in the surface value of the vertical eigenfunction (U)"*. In our example it can be observed that this is not always the case, as the horizontal eigenfunction at the surface is actually positive. At 2 Hz, both the vertical and horizontal eigenfunctions are positive at the surface, resulting in a positive (U/V) ratio and therefore prograde motion. At 5 Hz, the horizontal eigenfunction becomes negative resulting in a negative (U/V) ratio and therefore retrograde motion. This indicates a prograde - > retrograde transition occurring at ~3 Hz.

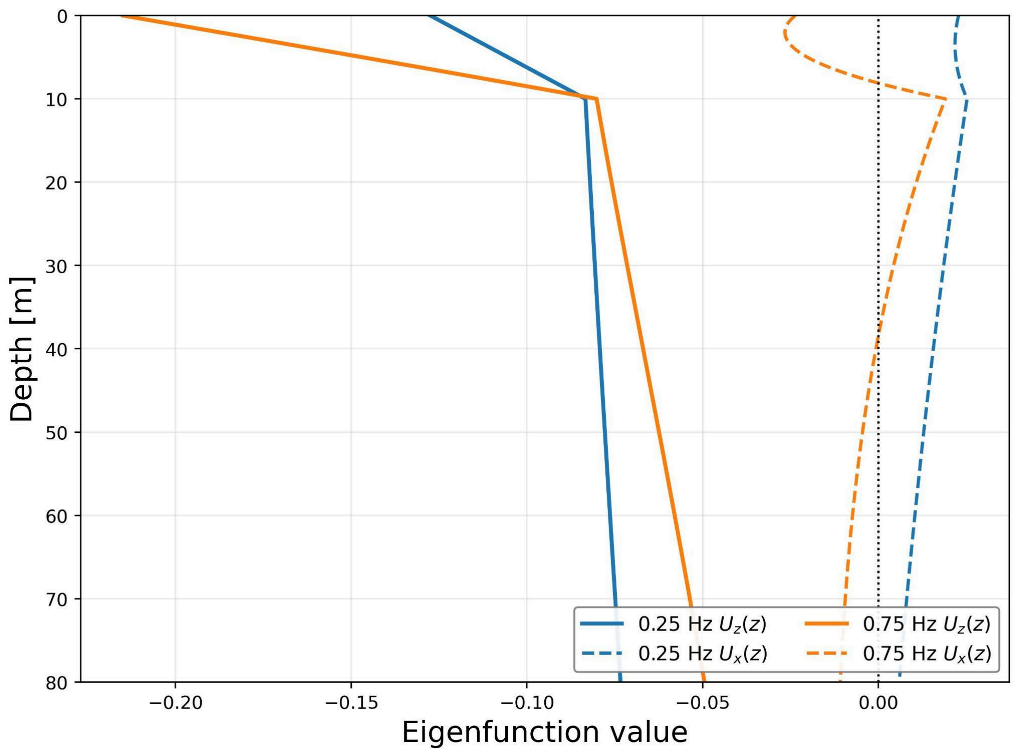
We pose that the different character of the eigenfunctions here versus Tanimoto and Rivera (2005) could be due to the differing boundary conditions at the free surface. Here only the horizontal stress is zero and the vertical stress is controlled by the pressure of the acoustic wave, whereas the Rayleigh wave problem has no horizontal or vertical stresses. This difference affects the vertical stress eigenfunction which in turn affects the other associated eigenfunctions.

To illustrate that the controlling factor for the sense of retrograde or prograde for air-to-ground coupled waves is the horizontal eigenfunction, we also compute the horizontal and vertical eigenfunctions for a modified version of the 244A model (so that the 244A model includes a retrograde -> prograde transition). We select a modified version of the 244A model to show that the controlling factors for air-to-ground coupled particle motion do not differ between leaky and guided waves.

The modified 244A model parameters are as follows:

- c_hat = 348.3
- Layer: Vp/Vs = 3, Vs = 418.1 m/s, rho = 1716.8 kg/m^3, h = 10 m
- Halfspace: Vp/Vs = sqrt(3), Vs = 3556.1 m/s, rho = 1906.5 kg/m^3

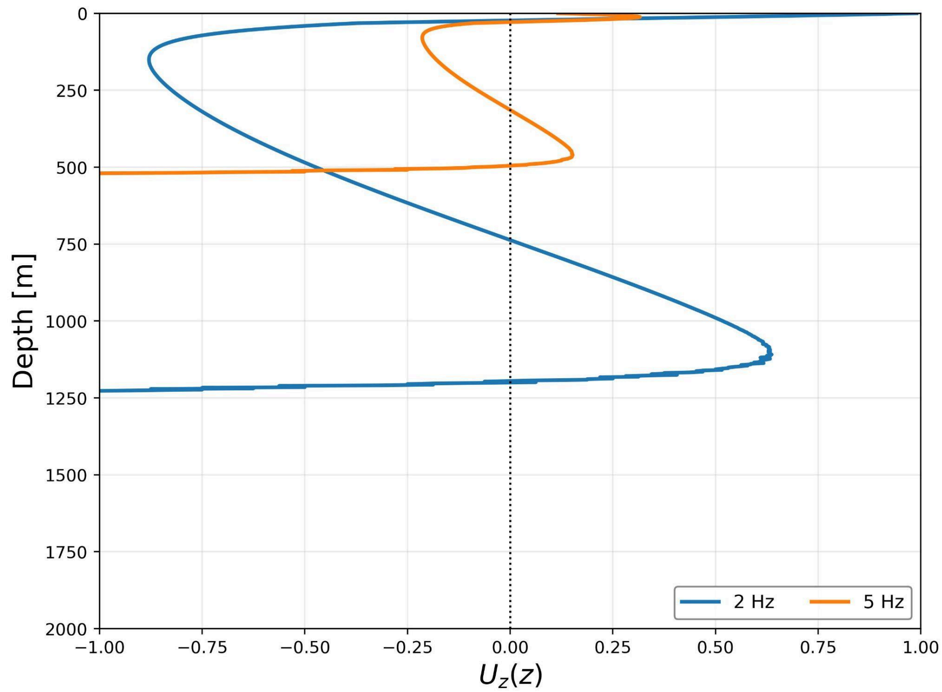
Below we show the real parts of the horizontal and vertical eigenfunctions for 0.25 Hz and 0.75 Hz:



This result shows that both guided (244A modified) and leaky (Z43A) air-to-ground coupled waves have the same control on particle motion: the sign of the horizontal eigenfunction at the surface, as opposed to the vertical eigenfunction as seen by Tanimoto and Rivera (2005) for Rayleigh waves.

> In the caption for Figure S6, it is mentioned that the measured compliance parameter is for leaky waves at higher frequencies, based on phase velocities. Show the eigenfunctions at both in the air and the solid Earth. If they are for a leaky mode, the eigenfunctions should probably be oscillatory instead of exponential decaying.

Below we show the real part of the vertical eigenfunctions at 2 and 5 Hz to a depth of 2000 m using the best-fitting subsurface model for Z43A (Figure 8, 9).



Here we observe that the eigenfunctions do oscillate and then grow exponentially. This behavior is opposed to the characteristics of guided waves (like Rayleigh waves) where the eigenfunctions exponentially decay to 0 with depth. Also, as mentioned in the paper revision, the example from TA.Z43A examined in Fig. 8 & 9 has properties discussed in Xu and Lognonne (2024) when they look at the case of apparent velocity less than the compressional velocity of the halfspace but greater than the shear velocity of the halfspace. They point out that the waves leak SV energy into the halfspace.

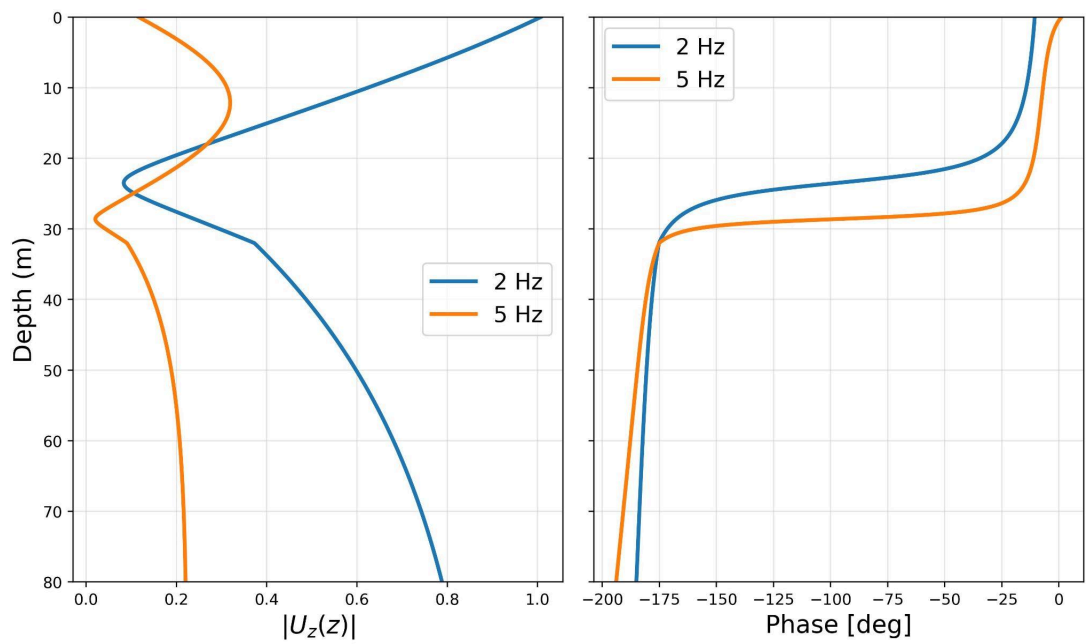
Regarding showing the eigenfunctions in the air, we claim here that we can figure out what they look like without formally computing them.
Note that we are using the pressure-loading technique of Xu and Lognonne (2024). In that technique, the air is a forcing function used to excite the solid Earth. This is in contrast to the normal mode technique, in which the eigenfunctions are solved for the entire air-Earth system. These distinctions have been recently discussed in Tanimoto (2026; JGR), which has just been published and is now cited in our paper. An example of a coupled fluid-solid normal mode code using finite elements has been described by Haney and Tsai (2017; Geophysics). For the simple case of an air halfspace over a solid Earth halfspace, and no gravitational effects (g=0), the normal mode technique yields a guided wave called the Stoneley wave. Ignoring gravitational effects is suitable for high frequencies well above the Brunt-Vaisala frequency, such as the signals we analyze in our paper. In this paper by Strick and Ginzbarg, the authors analyzed the phase velocity of the Stoneley wave:
Strick, E., and A. S. Ginzbarg, 1956, Stoneley-wave velocities for a fluid-solid interface: Bulletin of the Seismological Society of America, 46, 281–292

Strick and Ginzbarg (1956) did not analyze the eigenfunctions of the Stoneley wave, but since it is a guided wave the eigenfunctions decay exponentially in both directions (decay upwards in the air and downwards in the solid). It turns out that for the case of the Earth having higher speeds than the air (Vair<Vs<Vp) and much higher density than the air, the Stoneley wave speed is just slightly less than the speed of sound in the air (Figure 1 in Strick and Ginzbarg). This means that the eigenfunction in the air decays very slowly with altitude; however, it eventually does exponentially decay to 0 at an altitude of infinity. In the limit of the air density going to zero, the Stoneley phase speed becomes equal to the speed of sound in the air and the eigenfunction in the air is constant, with no vertical exponential decay. Brekhovskikh, in his 1980 book Waves in Layered Media, discusses properties of Stoneley waves in section 7.4.

From this discussion, it's clear that in order to plot the eigenfunction in the air depends on knowing the density of the air. The pressure-loading technique does not specify the density of the air, in contrast to the normal mode technique, and so it's not possible to plot the eigenfunction in the air using the available information used for pressure-loading technique alone. If one assumes a typical value for air density, such as 1.29 kg/m^3, then the eigenfunction is a slowly decaying exponential with altitude and the prescribed phase speed in the air is slightly less than the true speed of sound in the air. The slowly decaying exponential eigenfunction in the air obeys the continuity of vertical displacement and normal stress at the air-Earth interface.

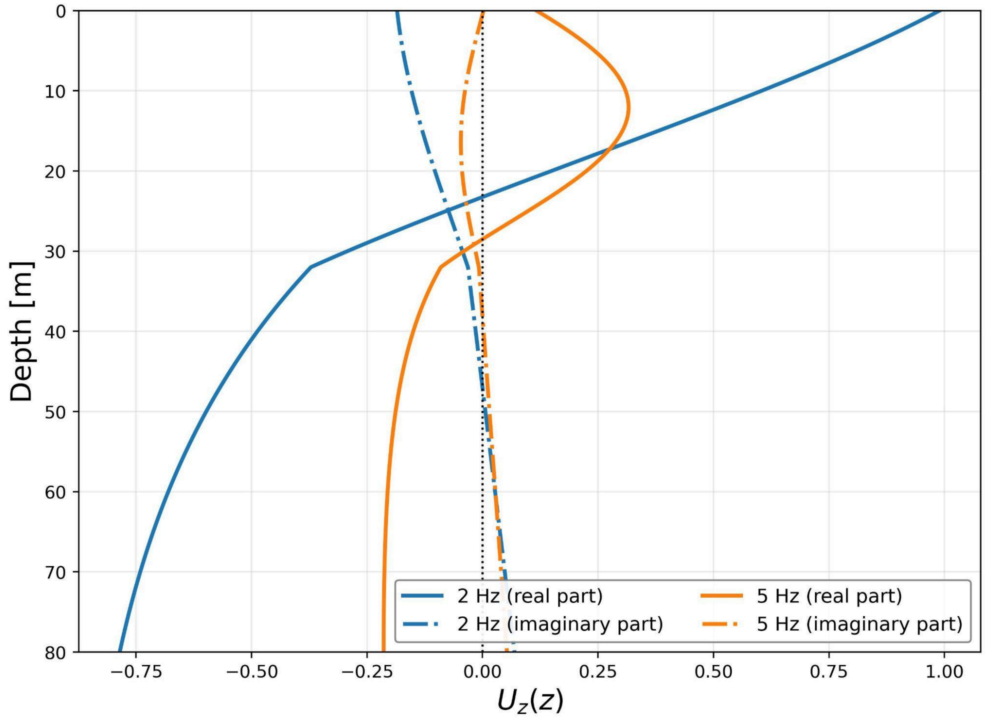
> If you are using complex quantities in the frequency domain, please make a plot for absolute values of vertical eigenfunctions and a separate plot of phase plots that should indicate changes in phase (or 180 degrees change in phase).

In the figure below we have plotted the magnitude and phase of the vertical eigenfunction at both 2 Hz and 5 Hz:



Again, this example uses the subsurface model from Figure 8. This figure clearly shows a 180 degree change in phase that occurs at the depth where the sign of the vertical eigenfunction

changes. This phase change is only visible for leaky waves whereas the fully guided wave for the modified 244A model has a constant phase and the eigenfunction is real.
For additional context, we also show the real and imaginary parts of the of the vertical eigenfunction at these two frequencies below:



2. Why is phase velocity about 220 m/s at higher frequencies in Figure S6? What are the seismic velocities used for computation?

The phase velocity of 220 m/s is at higher frequencies in Fig. S6 because the subsurface model used (the best-fitting inversion parameters from Figure S4), has a low-velocity layer overlying a higher velocity layer. In this context, we have increasing shear wave velocities with depth, and therefore the Rayleigh wave phase velocity will also increase when moving from higher to lower frequencies.

The exact model is as follows:

- S-wave velocity layer: 235.7 m/s
- S-wave velocity halfspace: 348.3 m/s
- Density layer: 1738.6 kg/m3
- Density halfspace: 2678.4 kg/m3
- Layer thickness: 33.5 m
- Quality factor: 16.0

With reasonable response on these points, I will recommend publication of this paper.

Toshiro Tanimoto

Reviewer C

Scamfer et al have produced a well written manuscript detailing a study of explosively generated air-to-ground coupled seismic waves. They have developed a methodology that can help to automatically characterise coupled wave polarisation characteristics, such that the technique can be used for extensive datasets where manual identification of polarisation features might prove too time-consuming. Using this extensive dataset the authors have shown that polarisation features can, in a broad sense, be correlated to expected shallow subsurface elastic properties. The authors then focus on arrivals at three stations, and undertake a Bayesian inversion for the best fitting one-layer plus halfspace subsurface model. The methods are novel, and are an extension of previous studies and will, in my opinion, be of interest to the readership of Seismica.

I have a few queries, most of them very minor / trivial. The point that I feel might be worth exploring a little more is the sensitivity of the inversions to the assumed apparent velocity of the incident airwave. Some of the inverted parameters, especially for the case in which propagating surface waves are generated, may be quite sensitive to this choice. If not, it would be useful to understand this.

Once the authors have considered my points, and revised / rebutted as appropriate I'm confident it will be a useful contribution to the air-to-ground coupling literature.

L189-195. Later in the paper (L457 – 471) the authors provide a well reasoned paragraph discussing the assumption of radial/transverse partitioning of energy (based on the receiver-source backazimuth). For this first time reader it was not obvious from the methodology described in L189 to 195 that the authors would address this assumption. It may be useful for this section to be slightly expanded so that it is explicit that the methodology assumes elliptically polarised motion in the radial-vertical plane and that this assumption will be discussed later in the paper.

We agree that introducing this assumption earlier in the paper is important. We have added two sentences to the second paragraph of section 3.2 that introduce this assumption and inform the readers that we will discuss it later.

L201. Readers are perhaps unlikely to be familiar with the NIP and the work of Meza-Fajardo et al., (2015). It might be helpful to expand the sentence regarding normalisation to provide a little more understanding of how the cross-spectrum is being normalised, i.e., 'We then normalise the cross-spectrum by dividing through by the product of wave amplitudes $(A_Z(\omega)A_R(\omega))$, so that ...'

The cross-spectrum $S_{\hat{Z}R}$ normalization is shown in Equation 9, where the cosine of the argument of the cross-spectrum is taken. This is the only normalization. To make this clearer we have added a reference to Equation 9 explicitly: "normalize the cross-spectrum (Equation 9)".

L201/202 'so that 1 indicates retrograde and -1 indicates prograde particle motion'. Is this strictly correct, or is it more accurate to say that 'so that positive values indicate retrograde particle motion and negative values indicate prograde particle motion'? Consider Fig 4 of the manuscript as an example: in the 6 to 7Hz passband the motion is described as prograde (at L360) whereas in the lower passband (3 to 4Hz) the motion is described as retrograde (at L359) – I'd agree with this. However, the NIP values (Figure 4d) are not (exactly) 1 and -1. From the NIP definition I'd also agree with this – the values of -1 and 1 will only be achieved if there is a pi/2 phase shift between the radial and vertical components which Fig 3e and f indicate isn't the case.

We agree on this point that prograde/retrograde does not require exactly -1 or 1. Overall a positive NIP does imply retrograde and a negative NIP does imply prograde, however if the NIP is near 0 the particle motion is essentially rectilinear and not elliptical.

We agree this should be specified to improve clarity. We have revised this sentence to "... so the values range from -1 to 1, where positive values indicate retrograde and negative values indicate prograde particle motion, which defines the NIP:"

Two paragraphs later we also add onto the last sentence: "... -0.5 for prograde, and to exceed 0.5 for retrograde, because an NIP near zero implies rectilinear motion." to ensure the readers understand that values close to zero cannot be interpreted as prograde/retrograde.

L208 to 211 – Consider moving this paragraph to the end of the section. You are describing an averaging process over a time-frequency region that is not yet defined – it is detailed (1.5-10Hz) in the first sentence of the next paragraph. Also, for this reader, it was slightly confusing to read about the 'spatial distribution of AGCW particle motion' when the rest of the section details the variation in time-frequency space. Consider updating first sentence to something like: 'When examining the geographical distribution of AGCW particle motion across the sensor network,'.

We agree, and have moved this paragraph to the end of section 3.2.

We have also updated the first sentence in the paragraph referenced to not confuse the reader between time-frequency space and geographic space. The new sentence now starts: "To examine the geographic distribution of AGCW particle motion across TA stations, …."

L249 – here you state 'at the top of the halfspace' without introducing the reader to your model (of n layers above a halfspace). Suggest adding a single short sentence describing the model before you discuss 'the halfspace'.

This is a good point, and we have added a sentence to the first paragraph of section 3.3 to present our model as a layer-over-halfspace before referencing the halfspace.

L266 this is no doubt pedantic, but the Jardetsky and Press (1952) air-coupled Rayleigh waves are not exactly monochromatic – there is some (albeit very slight) frequency dispersion associated with their 'Branch III' solution (see their Fig. 1). Perhaps consider changing 'monochromatic' to 'quasi-monochromatic' or 'almost monochromatic' (but happy to be rebutted with regards this point).

We agree that quasi-monochromatic would be a more accurate description of these waves. We have changed monochromatic to quasi-monochromatic

L281. How sensitive are the results of your Bayesian inversions to the choice of one apparent acoustic velocity (348.3m/s)? – presumably there is an appreciable uncertainty on the airwave apparent velocity (closer to 10m/s than 1m/s, perhaps)? For example in L415 the authors note that the posterior probability distribution for the halfspace shear wavespeed appears to be very tightly constrained at Station Z43A, much more so than other parameters (Fig. 8d). Why is this parameter so well constrained by your inversion? Is it this halfspace shear wavespeed that controls the presence (or frequency) of the quasi-monochromatic decaying wave in the vertical displacement? If so, how sensitive are these values to the choice of apparent velocity? I note that in extreme cases, where the apparent velocity changes significantly, the generation of surface wave modes depends upon having a low enough apparent velocity (see, for example Fig. 4 of Lin and Langston, 2007, GRL 'Infrasound from thunder').

This is a good point to raise. The apparent acoustic velocity indeed carries some uncertainty and could plausibly vary by ~10 m/s as mentioned. Because air-to-ground coupling depends strongly on the relationship between the incidence angle of the acoustic wave and the subsurface velocity structure, the inversions are sensitive to this choice, particularly at stations where surface wave activation occurs (e.g., Z43A).

At Z43A the halfspace shear-wave velocity is (likely) tightly constrained because of your explanation: It controls the presence and frequency of the quasi-monochromatic wave in the vertical displacement. The excitation of these waves (and the frequency it occurs at) depends on the surface wave dispersion characteristics of the subsurface which heavily depend on the halfspace shear-wave velocity, but also the overlying layer velocity and thickness. Because of this, the best-fitting halfspace shear-wave velocity (and likely other parameters) may shift if a different apparent velocity were assumed.

Upon reviewing our discussion section, we feel that this limitation and its effect on the inversion result was not sufficiently addressed. We have therefore added a paragraph to the end of discussion section 5.2 describing the uncertainty in the apparent acoustic velocity and its influence on the inversion.

Figure 6 (inversion at TA.244A). Earlier in the paper (L212) you make the case for choosing a lower frequency bound (1.5Hz) using the argument of depth sensitivity (based on the kernels of Tanimoto and Wang). For the Bayesian inversions the lower bound is extended to 1Hz (such that the depth sensitivity of the lowest frequency is peaked at about 50m). The Bayesian inversion for TA.244A (Figure 6) results in a structure with a relatively tightly constrained upper layer thickness of ~ 67+/-7m. This question likely highlights my ignorance, but given that only the very lowest frequencies in the passband used for the inversion are sensitivity to depths corresponding to the layer thickness, what is providing the tight constraint on the layer thickness (as any higher frequencies are presumably much more sensitive to significantly shallower structure)?

This is a good clarifying question. First, we realize that the difference in the inversion frequency band vs NIP analysis was not specified in the methods. We have added a sentence to the end of paragraph 3 in section 3.4 to specify this. The frequency band was extended to 1 Hz for the inversions because coherent coupling was present at all stations down to ~1 Hz, and we are not as interested in Vs30 (~1.5Hz) for the inversions.

To answer the question: The depth sensitivity relation from Tanimoto and Wang (2019) (h=0.15c/f) is a good approximator of the depth of maximum sensitivity, but not the maximum depth that the waves are sensitive to. Air-to-ground coupled waves at a single frequency have variable sensitivity to different depths. See Figure 7 in Tanimoto and Wang (2019), showing depth sensitivity kernels for a wind source (c =1 m/s) at 0.01 Hz. According to the depth relation the sensitivity is 15 m, however the sensitivity kernels are actually sensitive to greater depths, up to almost 60 m.

In the context of our inversion, the 1 Hz component is still sensitive to the halfspace, however the majority of the sensitivity of the band is in layer 1, which could be why the parameters are so well constrained for layer 1.

To provide better clarity to the reader, we first will specify that the depth relation is not a limit, but rather an approximation of the depth of maximum sensitivity. We have added a sentence in the second to last paragraph of section 3.2

L462 – I'm unsure that there is evidence for cross-wind generated azimuthal deviations being range-dependent. If you require a reference for the choice of 5 degrees, section 5.3 of Nippress and Green (2023) ["Global empirical models for infrasonic celerity and backazimuth", GJI] summarises a large number of ground truth observations and their 95% confidence bounds on the backazimuth deviation population are almost exactly +/- 5 degrees.

We appreciate the reference for the choice of 5 degrees, and have added it. The range-dependent azimuthal deviations we imply here are based on the results in Vergoz et al., 2022 (see section 5.1 in the discussion) who found significant back-azimuth drift (>> 5 degrees) at multiple IMS stations over very long propagation distances from the Hunga, Tonga eruption. These propagation distances often exceeded the circumference of the Earth.

Typographical Considerations

[As Seismica is copy-edited by volunteers, they ask reviewers to identify any typographical issues. I note some here in case of interest.]

Eqns 1 and 2: t is not defined, whereas all other symbols are. Suggest defining t for completeness.

We have now defined t following equation 2.

L.51 – style conventions usually require a comma after 'e.g.', so that it is 'e.g.,' - consider adding here.

We have added a comma here and after e.g. for all instances in which e.g. is used.

L.179 – suggest ‘chose’ instead of ‘choose’. The rest of the sentence is in the past tense.

We have changed ‘choose’ to ‘chose’.

Eqn 7. - should there be a parenthesis around (\phi_z(t,\omega)+\pi/2)? Currently the (\pi/2) term is real not imaginary.

Thank you for catching this, yes there should be a parenthesis. We have added them.

L 206 – remove ‘the’ from ‘by the multiplying’

We have fixed this.

L207 – consider replacing ‘aforementioned magnitude-squared coherence’ with ‘magnitude squared-coherence (Eqn. 4)’ to assist the reader.

We have removed “aforementioned” and included the equation reference.

L235 \hat{c} and \omega have previously been defined (L56 and L63). Do they need to be re-defined here? If so, perhaps be consistent and re-define \mu (which is defined in L62).

We choose to keep the redefinition to help remind the readers, however we have added the redefinition of \mu for consistency.

Eqn 14. Here the authors have a negative sign in the numerator. This is different to Equation 5 of Xu and Lognonne (2024) where there is no negative sign in the numerator. Please check (I may have misunderstood, or not accounted for a definition difference?).

The convention in Xu and Lognonné (2024) is positive downwards (similar to Sorrells, (1971)), whereas we use negative downwards. See Figure 3e in Xu and Lognonné (2024) where a positive pressure signal produces an initial increase in vertical velocity.

We have added a sentence to section 3.3 to explicitly note this.

L333 incorrect parentheses around reference. Should be ‘Wills et al., (2022)’ not ‘(Wills et al., 2022)’

We have fixed the parenthesis for this reference.

Figure 6 caption: perhaps ‘g)’ instead of ‘g’ for consistency with other panel labels?

Thank you for catching this, we have made this change.

L401: ‘shear-wave velocity of 568m/s’: should this be 568 or 468 (as per Figure 7h)?

Yes, this should be 468. We have fixed this.

L410: suggest Figures (plural) in ‘(Figures 2,8)’ - currently singular.

We have changed this to “(Figures 2, 8)”.

L497: require end bracket for ‘(Figure 3.’

Fixed.

L611 and 612. Replace the two instances of ‘TO BE ADDED’ with the required references.

We have added the missing references to the two GitHub repositories which are now publicly available.

L620: is a publisher required for Aki and Richards book reference?

Good catch, we have added the publisher “MIT Press” to the reference.

USGS Internal Review by Alex Iezzi

Summary:
The study “The Influence of Shallow Subsurface Properties on Particle Motion in Acoustic-Seismic Coupling” by Scamfer et al explores air-to-ground coupling of acoustic waves. They employ multiple novel methods to quantify the seismic particle motion, model the coupled waves, and perform a Bayesian inversion to infer properties of the shallow subsurface. Of particular importance, they find that a larger portion of coupled waves have prograde motion, which is significant because some techniques for backazimuth estimation assume retrograde particle motion.

The paper is very well-written, methods are thoroughly explained, and figures are clear and effective. I have only a few minor comments for the authors to consider.

Overall Comments:
1. I wonder if infrasound propagation has a secondary effect on the retrograde/prograde pattern that may not be as cleanly explained by the large-scale subsurface properties of the V S30 model? It seems like there might be a shift near the first stratospheric bounce distance (it’s a shoulder season so not sure if the winds shifted towards the east yet), and maybe the incidence angle from arrivals from different waveguides may impact the coupling? I know propagation modeling is likely beyond the scope of the study, but it might be worth mentioning super quick in the discussion section. I pulled this quote from the discussion section of the Bishop et al. (2023) TCM paper that might be relevant: “These propagation paths will intersect the ground surface at different incidence angles (Matoza et al., 2011), which affects the apparent speed of the propagating acoustic wave and the coupling characteristics (Anthony, Ringler, et al., 2022; Bishop et al., 2022).”
This is an important point, and we agree that the incidence angle has a direct effect on the frequency at which the prograde/retrograde transition occurs, and therefore what sense of particle motion may dominate at a given station. We have added a paragraph to the discussion Section 5.1 (after paragraph 2) to note that the differences in propagation (tropospheric or stratospheric) could contribute to the dominant sense of particle motion in addition to the subsurface. We also note that Wills et al., 2022 performed propagation modeling for the Camp

Minden Explosion and from their results we do not see any strong correlation with stratospheric arrivals and changing dominant particle motion.

2. I appreciate that you chose to highlight different stations with varying degrees of success for your methods. It is super important in research to highlight where limitations exist as opposed to just highlighting the examples in which your method works perfectly.
Thank you.

3. Figures 6,7, and 8: This is totally a personal preference so not a necessary comment to address, but it's usually good practice to avoid red-green combo unless necessary since it's the most common form of colorblindness. Just something to consider to try to be more inclusive when making figures.
Thanks for pointing this out, we agree on improving the inclusivity of our manuscript. We have modified Figures 6,7,8, S3, and S4 to have colorblind-friendly colors.

Specific Comments:

Line 83: A globe is a model of the Earth so it might make a bit more sense to say “Earth” instead of “the globe”.
We have changed the text to “around the world” instead of “across the globe”.

Lines 140-141: It's slightly awkward to have a single sentence paragraph, so you may consider combining it with the next paragraph.
Agreed. We have combined this with the next paragraph.

Line 152: Suggest adding the make/model of broadband seismometers since it is included for the infrasound sensors
We have specified the make/model of the three broadband seismometers used by the TA.

Lines 175-176: You may consider adding the celerity values here for completeness
We have added the celerity range typical of stratospheric and tropospheric infrasound propagation to the text.

Lines 375-378 (referring to Figure 5 e,f): Would it be worth also mentioning the large difference in the axes (an order of magnitude) for these two subplots in addition to calling out the amplitude distribution between vertical and radial? The other two examples are much closer in displacement values between the two frequency bands.
Yes, we have now mentioned that the scales of these two panels are significantly different.

Like 404: You can remove “the” in “As seen in the Figure 7b-c…”
We have made this change.

Line 497: Missing a “)” after “(Figure 3”
Fixed.