RC1:

Dear authors

This is a review for the manuscript 'Constraints on Fracture Distribution in the Los Humeros Geothermal Field From Beamforming of Ambient Seismic Noise'

I believe the manuscript is well presented and written and is worthy of publication as is. The only point I feel should be made is perhaps that the assumption 'seismic velocity is faster along orientation of a fault ...' merits more investigation, however I agree that it is beyond the current scope of the manuscript and should be explore in separate work.

I have included the pdf with a small number of minor comments, which are not critical but could perhaps be of use.

We thank the reviewer for your minor comments, all comments have been acted upon. Any specifications of lines refer to the tracked changes manuscript.

Best regards
Reviewer

Minor Comments

Line 45: first time acronym used in the text, specify meaning Los Humeros Volcanic Complex?
Brittle-ductile?

We thank the reviewer for pointing out this mistake and have added these acronyms in this first appearance (Lines 50-51).

Line 80: Instrument type?
Line 81: Instrument type?

We apologise for any confusion, the 45 3C stations are referring to the 25 broadband (22 Trillium C-120s and 3 Trillium C-20 PH) and the 20 short-period (Mark L-4C-3D) stations. We have made this clearer and included the instrument type for the SP stations (Line 144).

Line 111: Unable to find the supplementary materials
Line 122-123: Again mention of supplementay materials, perhaps the authors are referring to Appendices?

We agree this should be the appendices and not the supplementary materials, all mentions of supplementary materials have been changed to appendices or the appendix (Lines 178-179 and 191).

Figure 3a : difficult to observe the white circle encapsulating true h-wavenumber

We thank the reviewer for bringing this to our attention and have changed the figure fittingly (figure 4).

Figure 4: Is quite difficult to observe the bootstrap resampling curve, perhaps change the color or the line thickness.

Thank you for pointing this out, the figure has been changed accordingly (figure 5).

Figures 6 and 7: Text resolution in figures 6 and 7 could be improved

We thank the reviewer for pointing this out, the figure's text resolution has been improved accordingly (figures 7 and 8).

Line 229-230: Has there been any modeling done on this topic? I would be curious about certain cases in which faults zones are highly brecciated (slow velocity) for example.

Thank you for this observation. However, upon further investigation, there is a lack of modelling that has been done on this topic and the extent of velocity investigation in fault zones speaks of the low-velocity areas (low shear velocity) in the damage zone caused by faulting, around the faults themselves (Thakur et al., 2020).

References:

Thakur, P., Huang, Y. and Kaneko, Y., 2020. Effects of low-velocity fault damage zones on long-term earthquake behaviors on mature strike-slip faults. Journal of Geophysical Research: Solid Earth, 125(8), p.e2020JB019587.

RC2:

Comments to the Author(s)

The manuscript presents a 3C-beamforming analysis of the ambient noise recorded at Los Humeros Geothermal Field in Mexico. The 3C beamforming allows the separation of the different polarized waves contained in the ambient noise. The velocity, measured as a function of the azimuth and frequency, is then estimated by picking the maximum of the beamforming diagram for each polarization state. The observed azimuthal variations are interpreted to be due to azimuthal anisotropy and the frequency variations due to depth variations. Both retrograde Rayleigh wave and Love wave anisotropy are estimated. The direction of fast velocity anisotropy does not seem to correspond to the main orientations of fractures in the studied region. Several geological features are discussed to account for this observation.

The studied target, the scientific question, and the methodology are exciting and wellsuited. I think the methodology is mostly well executed, but there is some missing information in the paper to fully assess the results' validity. I understand that this is a first PhD student paper, and I fully acknowledge the amount of work that has been done to write this paper. However, the presentation's overall quality and the paper's organization and structure should be reworked to convey the results better and fit the expected quality of a scientific journal such as Solid Earth. There are redundancies in the text and some unclear or approximate terminology that I will precise below. I also feel that the extensive geological description in the Discussion Section should be placed in the introduction and only referred back to it in the Discussion. The equations in the main text are all faulty, with many typos. Finally, I think some figures might not be necessary while others are missing.

Thank you for your insight and for pointing these discrepancies out, all errors in equations have been fixed alongside any redundancies in the text and unclear terminology. Furthermore, the in-depth geology from the discussion section has been moved to the introduction and is referred to (Lines 77 to 136); all mention of specific lines and figures for changes made refer to the track changes file.

In general, because Riahi et al. (2013) is your main source of inspiration and because this paper is very well structured and written, I would suggest that you follow its structure even more closely. Especially, some figures such as the equivalent of their Figure 2 and 6 should be shown in your paper to better highlight the network response and the distribution of seismic energy as a function of the type of waves, frequency and azimuth.

We thank the reviewer for this observation, an equivalent of figure 2 from Riahi et al. (2013) has been added to the paper (figure 2). We appreciate the suggestion of adding an equivalent of figure 6 from Riahi et al (2013), however, we feel that the polar plot showing the variation in anisotropy for both Love and retrograde Rayleigh waves is a suitable equivalent (figure 6 in this paper).
We appreciate the reviewer's comment on structuring this paper to be more like Riahi et al. (2013), however, as our paper heavily focuses on application to a geological setting as opposed to

predominantly focusing on methodology development such as in Riahi et al. (2013) we do not think following a similar structure would be suited in this case.

Detailed comments:

Line 42-43: The last sentence of the paragraph is wrong. Anisotropy and surface wave (not only Rayleigh wave) dispersion are two different things. The dispersion comes from vertically heterogeneous media, while the anisotropy of surface waves can have several origins and natures. Azimuthal anisotropy can come from vertical fractures oriented in a specific direction but can also be caused by foliations and mineral and preferred orientations of crystals. This is different from radial anisotropy, which depicts the difference in wave speed between vertically polarized shear-waves (Rayleigh waves) and horizontally polarized shear waves (Love waves).

We apologise to the reviewer for making an incorrect statement and have implemented the relevant changes referring to radial and azimuthal anisotropy accordingly, whilst differentiating between anisotropy and surface wave dispersion (Line 44-48).

Line 90-91: "Spectral whitening
and one-bit normalisation were applied in the time domain
". Spectral whitening is not time-domain processing. One-bit normalization is strongly non-linear processing affecting the amplitudes of the signal heavily and sometimes the phase if the whitening is not done properly. How does this pre-processing of the noise affect the estimated polarization of the surface waves, the beamforming results, and overall the anisotropy estimation?

This pre-processing was used as the beamforming methodology does not require the absolute amplitudes of the noise, thus, the pre-processing methods were used to normalize the frequency spectrum whilst retaining the phase information (Nakata et al., 2019); which is of the main importance for the beamforming analysis (Lines 154-156).

Line 99: When written this way, "retro-, prograde Rayleigh and Love waves," I understand that both Rayleigh AND Love waves are retro- and prograde. It is confusing and should be written differently. Maybe writing "retro- and prograde Rayleigh waves as well as Love waves". There are other places in the text where similar wording is used and should be checked.

We apologise to the reviewer for causing any confusion and have reworded all areas where this confusion occurs (Lines 165-166 and 186).

Line 127: "The direction of propagation is anti-clockwise from east, making an azimuth of 90 degrees equal to North." This is not the standard definition of an azimuth, this is the definition of a trigonometric angle. You should use the formal definition of azimuth, mainly because all anisotropy estimations in the Smith and Dahlen equation must be taken clockwise from North. With the correct definition of azimuth, you should obtain different

orientations for the anisotropy, possibly solving the discrepancy between your measurement and the fracture orientations.

This definition of azimuth although not standard was used due to the beamforming methodology requiring this direction of propagation, thus, the anisotropy estimations using standard azimuth definitions along with any further calculations were altered prior to the analysis accordingly to account for this change in the propagation direction (Line 196).

Line 160 and 171: Eq. 1 and 2, check the equations. Some terms are missing, and the 3\theta in Eq. 2 should be 2\theta.

We thank the reviewer for pointing out this negligence and have added the missing terms and fixed the typo (Lines 229 and 240).

Line 161: When fitting the histograms, what method do you use? If it's a least-square fitting scheme, what are the effects of the numerous outliers on your fitting procedure? Would a least absolute deviation (as in Riahi et al.) be more robust?

We apologise for not stating what method was used. The more robust least absolute deviation was used for the histogram fitting procedure, as done in Riahi et al. (2013) (Line 230).

Line 175: Explain more in detail why you use the 0.05-0.5 Hz frequency band. I guess that comes from the spatial aliasing limits of your array, but this is discussed nowhere

Thank you for noticing our missing explanation. The lower limit was picked due to limits of the spatial aliasing of the array and the upper limit was chosen to focus on smaller frequencies, thus deeper depths. This clarification has been added appropriately (Line 244-246).

Figure 3: At what time is taken the snapshot of the wavefield in panel a)?

The wavefield is computed in the frequency domain for one frequency so there is no time dependency (figure 4).

Figure 4: Try a least absolute deviation fitting as well, to assess the effect of the outlier measurements on the anisotropy parameter values.

Thank you again for pointing this out. As answered above, least absolute deviation fitting was done for the histograms (figure 5).

Figure 5: Do you use a quality criterion (such as the amplitude of the beamforming) to keep or reject a velocity measurement? There are many velocity measurements above 3.5 km that are probably just noise. Maybe cleaning these measurements by rejecting the less reliable ones would make the anisotropy more appearant and the fit more robust. What is the cause of the apparent line around 2.8 km/s? Is there a measurement bias inducing this oversampling at this specific velocity?

*We thank the reviewer for your comments. The quality criterion that was used was all the maxima above the threshold 0.5*A_{max} but with no minimum amplitude. We do reject strong (unphysical) outliers with a velocity above 10 km/s, as we use a maximum velocity of 10 km/s when fitting our curve. We agree that the line around 2.8 km/s stands out, and while it might refer to an actual dominant Rayleigh wave velocity, the linear feature might be enhanced by the choice of bin size. From our understanding of the role of the array design, it seems possible that an oversampling effect is created for certain velocities; however, this would require a more detailed investigation (figure 6).*

Minor comments:

Line 48: "extremely". Please refrain from using subjective terms in papers.

We have omitted using subjective terms throughout the paper, thank you for the suggestion (Line 54).

Line 60: "trap-door". Explain this term

We have included an explanation for a trap-door caldera, thank you for bringing this to our attention (Line 65).

Line 111: Replace "Supplementary Materials" by "Appendix" where suited.

All referrals to supplementary materials have been changed to appendix, thank you for bringing this up (Lines 178-179 and 191).

Additional References:

Beamforming and Polarization Analysis. (2019). In N. Nakata, L. Gualtieri, & A. Fichtner (Eds.), Seismic Ambient Noise (pp. 30-68). Cambridge: Cambridge University Press. doi:10.1017/9781108264808.004