Response to Reviewer for:

“Constraints on Fracture Distribution in the Los Humeros Geothermal Field From Beamforming of Ambient Seismic Noise” Kennedy et al.

Many thanks to the reviewer, for their detailed and constructive comments. We have implemented most of the suggestions, and for those that we did not implement we explain why below. Reviewer’s comments are in black plain text; our responses are in italics with increased indentation (specifications to specific lines and figures refer to the revised manuscript). In the revised manuscript we have marked all changes in red text.

Best wishes,
Heather Kennedy

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.

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 42-47).

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 151-154).

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 163 and 182).

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 192).

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 225 and 236).

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 226).

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 240-242).

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 52).

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 64).

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 175 and 186-187).

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