EGUsphere, referee comment RC2
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# Comment on egusphere-2022-389

Anonymous Referee #2

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Referee comment on "Constraints on fracture distribution in the Los Humeros geothermal field from beamforming of ambient seismic noise" by Heather Kennedy et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-389-RC2, 2022

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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 well-suited. 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.

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.

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).

Line 90-91: "Spectral whitening
and one-bit normalisation were applied in the time domain
". Spectral whitening is not a 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?

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.

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.

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.

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?

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.

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

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

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?

Minor comments:

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

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

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