<u>Response to reviews</u>

We are grateful to both reviewers for their insightful comments. We have made appropriate changes to the text, which are highlighted in blue (reviewer A) and red (reviewer B), in response to the comments. Below are more detailed responses to both reviewers.

<u>Reviewer A</u>

Thank you for your comments, particularly your insights on the development of the instantaneous frequency method. Below we respond to your points in the order they are raised. All changes to the text are highlighted in blue.

1. *First, with regard to the use of instantaneous frequency matching approach as used by Matheney and Nowack, there were also alternative approaches at the time, including the frequency-shift method in the frequency domain by Quan and Harris (1997).*

   We have added an acknowledgement of Quan and Harris (1997)'s approach (line 228-230) and have added a clarification that instantaneous frequency is not equivalent to spectral frequency (line 245-6).

2. *Here, I just give a brief digression on causal attenuation operators related to the use of either equations (20) or (21) as discussed in the paper (please skip if this is already well known).*

   We have revised our discussion of the choice of causal attenuation operator to include some of the additional details raised in your discussion on attenuation operators (lines 255-259). We have added an example in the supplementary material (supplementary figure 3) showing that the implementation we use does give a causal pulse, reproducing a similar figure from Shearer (2019)'s textbook. A practical solution for researchers is to add unit tests to their implementations to prove the code produces causal signal. Whilst our code is not released as a software package, we will certainly be adding this in our implementations.

3. *There is a typo in Equation (22),*

   Fixed. We also add some additional description of how the synthetics are constructed to avoid confusion between nu and the synthetic shear-wave source polarisation (line 296-7).

4. *Next, Figure 1 needs to be larger with bigger labels and also maybe labels on the individual curves in each sub-plots in terms of P, S1 and S2.*

   We have revised the label sizes as suggested and increased the figure size within Seismica guidelines. The caption has been edited to make it clear we compute these examples.

5. Maybe to be more specific, one could specify the fast direction as Phi sub(f) to distinguish clearly from Phi sub(r) used for reference frame rotation.

   We now use ϕ_f throughout the text for fast polarisation direction.

6. *I think that Figure 5 is a nice plot. However, more annotation of the individual curves would be useful. For 5a, is beta the same as nu, the source polarization (if taken from the Gabor wavelet in Eqn (22))? What's the dash-dot line in Fig 5c at -80 degrees? Maybe several more comments on the trace covariance matrix and the eigenvalue lambda-2 might be useful to some readers with regard shear-wave splitting analysis for completeness, in addition to the references given.*

   Figure 5 has been revised and simplified. Beta is the source polarisation of the synthetic shear waves, but we have elected to remove this bar to clean up the figure. Legends have been added to each panel. We have added some comments expanding on eigenvalue minimisation for shear-wave splitting (lines 329-333), where lambda-2 is used as a measure of shear-wave particle ellipticity which, in the absence of anisotropy, should be linear. Naturally, any potential significant source of additional phase shift between S1 and S2 poses an issue for shear-wave splitting measurements.

7. *For the source polarization, can the pulses be reshaped? For example, for observed data could one use say the P-wave pulse to correct for this on the S-waves?*

   This is an interesting idea. We had not considered reshaping the waveforms. In the data examples shown here, we have good constraints of source polarisation as SKS phases are known to be broadly radially polarised. Ideally, we want to remove the requirement for source polarisation stacking and are seeking future avenues to peel back this restriction.

8. *Figure 9 is I think very important in that corrections for dt* are needed even for delta-t plots. Maybe label the figure as Stacked Lambda-2 Surfaces or something to make it clearer.*

   We have added a title to Figure 9 to emphasise that it shows stacked lambda-2 surfaces.

9. *For the observed data, possibly adding one example of the data, which could be useful to the reader to see.*

   We have added a figure (Figure 11) showing a data example for one SKS phase rotated to the fast polarisation direction and the measured instantaneous frequency (for the analysis window used). Some small changes have been made in lines 506-7, 531, 534 to refer to the figure.

10. *Figure 10 is an important summary map. Can you make the lines at the station triangle bigger, or label them directly for delta-t and dt*? Also, label your axes as*

Latitude (deg), Longitude (deg), the color bar as Vs (km/sec). and add a km scale on the map.

Done.

11. *At the bottom of Page 24, lines 556-559, the authors note "This relies on the assumption that the squirt flow model (Chapman, 2003), is valid under upper mantle conditions and that the melt is hosted in very low aspect ratio inclusions which can be treated as an aligned fracture set in an isotropic host rock" Maybe several more comments on this might be useful, although I assume that this has been assumed in previous papers.*

    Clarification has been added at lines 556-559. We are extending the Chapman (2003) squirt flow model to upper mantle conditions, where it has not been tested observationally or experimentally. This should not affect the model validity, provided model parameters (such as the mineral-scale squirt flow frequency) can be chosen appropriately. Squirt flow as a mechanism for melt in the mantle has been discussed for a long time (e.g., Makvo and Nur, 1975) although typically at the grain-scale. Chapman (2003)'s multi-scale model allows us to consider the contribution of squirt flow to velocity and attenuation anisotropy, with the main assumption here being that we can conceptually treat low aspect ratio melt inclusions as fluid-filled fractures.

<u>Reviewer B</u>

Thank you for your comments. All changes in the text that relate to them have been highlighted in red.

<u>Comment #1. The title, abstract, and non-technical summary do not reflect the contribution of the work.</u>

The title has been revised to "A new technique for measuring SKS attenuation anisotropy gives new insights into melt near the Main Ethiopia Rift."

Lines 21 – 22: The result at station FURI is not new and had previously been explained by aligned melt (Ayele et al., 2004) from shear wave velocity anisotropy analysis.

We agree that we are building on previous SKS shear-wave splitting at FURI, indeed the fact that FURI has been previously studied with the anisotropy interpreted in terms of melt by Ayele et al., (2004) made it a compelling test case to attempt measurements of attenuation anisotropy. We have made this more explicit in the abstract and non-technical summary (lines 29-32; line 46-7). The new aspect here is measuring attenuation anisotropy, which has not been done before for SKS (line 25).

Lines 22 – 24: From Supplementary Figure 6, the predicted attenuation anisotropy is near-zero across the entire range of the fracture dips and is within the noise level of the measured *attenuation anisotropy (0.25 s in lines 500 and 556). Therefore, the fracture dip seems to be constrained chiefly by the shear wave velocity anisotropy.*

Supplementary Figure 6, as shown in the manuscript, was incorrect and has been revised. Sorry for that. Showing the noise level is a good idea, we now shade the region of +/- 0.25s. This figure shows two important things. Firstly, that for delay times < ca. 1.75s there are 2 or even 3 dips which can produce the same splitting delay time. Secondly, the sign of dt* (if resolvable above noise) gives a good additional constraint on what dips are feasible. For FURI this is very important as the observed dt* < 0 requires shallowly dipping fractures.

Also, the result of the average fracture strike contradicts the average fast polarization orientation from the shear wave splitting results. The result suggests that the melts are aligned both parallel and perpendicular to the rift. If this is not the case, more discussion on these results is required.

This arises from the requirement to have shallowly dipping fractures. Typically, using just SKS shear-wave splitting, one would interpret near-vertical fractures where fracture strike and shear wave fast polarisation directions align. An important consequence of the attenuation anisotropy measurement is that this forces us to adopt a shallow fracture model. In this case, the fast polarisation directions (computed by solving the Christoffel equation) are perpendicular to the fracture strike. In this sense, the fracture strikes and fast polarisation directions are not contradictory. This result does complicate previous SKS work in the region which interprets splitting in terms of vertically aligned melt (Ayele et al., 2004, Kendall et al., 2005). This is an intriguing result that certainly, in our opinion, motivates further work in the region. We suggest that directly beneath melt we have more shallowly dipping melt, which represents melt rising at the rift and then being channelled away laterally. Alternatively, there could be a different mechanism which is generating the attenuation anisotropy observed which is currently not known. We have expanded the discussion on this (lines 600-4; lines 614-621).

Lines 25 – 26: The results do not show how attenuation anisotropy can distinguish between anisotropy due to crystal or melt alignment. The study is entirely focused on attenuation anisotropy due to fluid alignment. Instead, the measurement of a negative attenuation anisotropy is purported to distinguish between the two dominant mechanisms of aligned-fluid anisotropy: crack scattering and squirt flow (lines 408 – 411).

We do calculate the expected attenuation anisotropy using a single crystal Olivine tensor for a large range of plausible, and perhaps implausibly low, mantle Q's (Supplemental Figure 1). This shows that attenuation anisotropy induced by the effect of simply having a strongly aligned crystal fabric is negligible compared to what is predicted by the squirt flow of aligned melt inclusions. We do not expect more complicated crystal scale mechanisms to operate at seismic frequencies. Therefore, we discount crystal alignment mechanisms (lines 200-204), with observations of attenuation anisotropy requiring a melt alignment mechanism. To make this clearer, predicted dt* for Q=50 has been added to Figure 1 (c) and (f). We have re-worded lines 22-3 to reflect that a comparison is made. We have also added a discussion of this in lines (545-551) and made this more explicit in the abstract (line 22-3, line 34).

(2) The instantaneous frequency matching method requires source polarization stacking to accurately determine the attenuation anisotropy parameters. The technique requires different source polarizations for the same source-receiver path, i.e., sampling the same region of attenuation anisotropy (lines 365 – 367). Ideally, this implies repeating teleseismic earthquakes with different source polarizations. The synthetic example in section 4 implements this requirement. However, the example at the MER does not. The earthquakes have a wide distribution of epicentral distance and back azimuth (Fig. 10) and thus would most likely sample different areas of attenuation anisotropy. Therefore, some discussion is required here on the uncertainty that may be introduced by violating the assumption.

This point is correct if applied to teleseismic or local S phases, and indeed presents a significant challenge to measuring attenuation anisotropy in the near-surface where its potential to improve fracture characterisation could prove very powerful. This is a planned body of future work (funders permitting). In this case, the saving grace is that SKS transits the outer core as a compressional wave and therefore it is radially polarised when it re-emerges from the core (e.g., Hall et al., 2004). Therefore, we can broadly assume that SKS backazimuth equals (or at least approximates) its source polarisation. Obviously, the source polarisation distribution is not perfect, but the coverage we do have for FURI should be sufficient.

A synthetic test that varies the shear wave splitting parameters applied to the synthetics (line 373) may be helpful. I am sure that the splitting result for the MER data is diverse even though only the station average is presented (Fig. 12).

We have repeated the tests as suggested where the shear-wave splitting parameters are drawn from normal distributions centred on the input parameters with standard deviations which mimic the uncertainty in the measured splitting at FURI. I.e., $\phi_f \quad N(30,5); \delta t \quad N(1.5,0.15)$. The scatter in delay time has some effect in the positive $\Delta t^{¿}$ case, increasing the uncertainties, but we can still recover the correct input parameters. This revised test has been added as supplementary figures 5 and 6, with some discussion on lines 456-468.

(3) The paper is rather lengthy. For more impact, I suggest focusing more on the contribution of the work, including further exploration of the method limitation and discussion of the contradictory results at the MER.

This is true, and to try to keep the length down we have tried to keep further expansion of discussion brief. In our opinion, the most significant contribution of the work is the method for measuring attenuation anisotropy and this paper is written with the intent of describing the method with a case study highlighting where attenuation anisotropy can be applied. In the MER case, FURI is only one datapoint so we are reticent to overinterpret at this stage. Instead this case study serves as a good motivator to revisit SKS across the region, although there are few permanent stations and if we want to estimate uncertainties in our measurements (using bootstrapping) then we require a sufficiently large sample size, say more than 50, of SKS phases which is difficult to achieve at the largely temporary deployments across the MER.

(4) Avoid using just one subsection, such as 2.1 or 3.4.1. I suggest merging the subsections to their parent sections.

These subheadings have been removed.

(5) Figure 5 is missing panel labels.

Done

(6) Line 373: delay time is 1.5 s, not 1 s.

This typo has been fixed

Response to editor comments:

Thank you for your comments, further changes have been highlighted in brown.

1. In your response to reviewers you write having retitled the paper to "A new technique for measuring SKS attenuation anisotropy gives new insights into melt near the Main Ethiopia Rift", but the submitted manuscript carries two different titles, one on page 1 and another on page 2. In relation to the new title, can you be more specific about the "new insights", such as "rift-perpendicular melt inclusions"? Alternatively, either of the titles on page 1 or page 2 are, in my opinion, appropriate, with the one on page 1 being favored over the two others.

Apologies for this, the revised title was re-revised to the one on page 1 and the response to reviewers must not have been updated. On page 2, I mistakenly thought this was for a summary page-header title and therefore contracted it to “Shear-wave attenuation anisotropy”. The full title should “Shear-wave attenuation anisotropy: a new constraint on mantle melt near the Main Ethiopian rift” and the latex has been edited to reflect this.

2. You added the following sentence to the abstract. Please clarify whether and/or specify how your findings agree and/or complicate previous analysis/interpretations. "This agrees with previous SKS shear-wave splitting analysis which suggested a 1% melt fraction beneath FURI, which complicates previous interpretations of sub-vertical melt inclusions aligned parallel to theMain Ethiopian Rift."

Previous work interprets the 1% melt fraction, which can explain our observations. The complication comes in the fracture orientations which are required to be rift-perpendicular to fit the observed attenuation anisotropy when using a poroelastic squirt flow model. Where this paper agrees and disagrees with previous work has been made more explicit by editing lines 26-7, 28-9 and 32-3. A similar edit has been made to the non-technical summary to reflect this.

3. Section 3.
   In the following sentence, which includes a "less" and a "more", please add what you are comparing instantaneous phase matching to. "Instantaneous frequency matching has been shown to be less sensitive to noise (Matheney and Nowack, 1995; Engelhard, 1996) and gives more robust estimates of isotropic mantle attenuation for teleseismic shear-wave phases (Ford et al., 2012; Durand et al., 2013)."

The comparison here is to measuring attenuation using spectral ratios. This has been made more explicit.

4.
   The following sentence you added in response to reviews might be more confusing than clarifying to some readers, especially because there does not exist a single ("the") spectral frequency. I presume "suitable" can also be

described more precisely or a rather more intuitive explanation of the difference between the frequency types could be provided. Please reword.

"It should be noted that instantaneous frequency is not the same as spectral frequency and only approaches the spectral frequency if there is suitable, damped, weighted averaging of the signal."

This comment has been removed and partially incorporated into the previous line. As:

“When weighted by instantaneous amplitude the instantaneous frequency of a signal approaches the centre frequency, or spectral mean, of the signal's Fourier power spectra for a sufficiently large analysis window (Saha, 1987; Barner, 1993)”.

5. Section 3.2
   I recommend that you reduce the text on lines 255-259 as follows:
   "...with the sign of the phase delay term being chosen to ensure that D(ω) produces a causal signal (Supplementary Figure 3). This depends on the choice of reference frequency and the sign convention of the fast fourier transform (FFT) implementation used."

   Done

6. Section 5
   When citing the GSN, please add the corresponding DOI to the reference as listed in the reference to the references section. You can find these DOIs on the FDSN web site.

   Done

7. You added the following sentence. Please also add how many of the 73 SKS waveforms fell in this category of polarization-backazimuth mismatch.
   "However, any SKS phases where the difference between source polarisation and backazimuth is greater than 10◦ are removed from the dataset."

   This line has been added in error and has been removed. We do not filter for these mismatches as attenuation anisotropy can affect how source polarisation is measuring in the shear-wave splitting process. We instead rely on manual QA of waveforms to ensure data quality.

8. Section 6
   In line 546, I recommend replace "as" with something like "given that" to emphasize the contrast between the preferred mechanism and the alternates.l.547-553: In this added text, please refer to pertinent figures.

   Done

9. Section 7
   line 657 please replace "this result" with something more specific like "this strike and dip".

   Done

10. line 658, please add citations to the mentioned "previous work", given that there are more than one previous works.

    Done

11. In my estimation, your response to the reviewer's comment on the method requiring source polarization stacking is indeed the sort of discussion he requested. Please find an appropriate point in the manuscript for where to include this discussion, or a synopsis of it, even if only an acknowledgement that the data example does not have the same source polarization diversity as the synthetic examples.

    The discussion of why this method is valid for SKS, and the underpinning assumptions has been collated in lines 483-489. Additional discussion, taken from our response to the reviewer, on potential issues applying the method to other S phases is added on line 490-4 and at the end of the discussion section.