Dear Dr. Lipovsky,

We appreciate the insightful comments from both of the reviewers, and have provided detailed responses below.

In addressing reviewer comments, we found some issues with our inversion, affecting portions of our velocity model. Namely, in some cases our burn-in period was insufficiently long, and the downsampling of our Markov Chains was not large enough to allow for true sample independence. Addressing these issues has allowed us to arrive at a more stable model. In general, the story of our paper remains the same, although we now predict a deeper basin depth and a thinner slumped block.

Thank you,
Eli Bird, on behalf of co-authors

Reviewer 1:
Comments:
1. Although the title says "Imaging with Converted Phases", I found the ambient noise tomography seems to be a more important part of the work, especially in constraining the main velocity structure. I suggest the authors to consider revising the title to better reflect the content of the work.
We have changed the title to "Imaging the North Bishop Block with Ambient Noise and Converted Phases Observed through Fiber-Optic Seismology"
2. The 2.6 inversion workflow is a bit complicated and not clear to me. I think the general audience may also find it difficult to follow. There are two things that I think the authors should clarify: (1). Why use two steps, i.e., sparse and denser? For example, what are the results from the first sparse inversion, and how do they help the denser inversion? How much the results change after the denser inversion?
Thank you for this suggestion, we have modified the text to address these points of confusion:
Lines 186-197 "In our first, sparser step, we invert for one-dimensional velocity models at 500 meter steps. We apply a 2-kilometer moving average to the dispersion curve, delay time, and relative amplitude data in this run. Through the MCMC inversion at a given channel, we are able to capture the posterior distribution of our model space. Summing the posterior distributions across all spatial steps, we can approximate a global probability distribution across the extent of the fiber. **This global probability distribution is the only result from the sparse inversion which is carried over into our second step.** We expect a degree of lateral continuity in earth structure, and we would like to ensure that any deviations from a homogeneous structure are required by our data. To this end, the global probability distribution for layer velocities (as calculated in the sparser inversion) is taken as a model prior in our second step. This choice means that in an instance where two velocity models are equally preferable at a location along the fiber, the denser inversion will select the velocity model whose layer velocities are more typical of global values. **In the absence of the global distribution, sections of the fiber with poorly constrained**

dispersion data would become unstable, as a wide variety of models could fit the data within error."

(2). Since there are three information sources (ambient noise, time and amplitude of converted phases) included in the inversion, it is not clear to me how significant each of them is. For example, what are the inversion results if only using one of them? Are there any trade-off or necessary weightings to combine them? The authors mentioned that "the relative amplitude of direct and converted phases allows for a tightened constraint on velocities within the layer." It would be informative to show how the inversion results change if the amplitude information is not included, which also helps to justify the importance of the converted phases.
We have tried to clarify the weighting scheme by explicitly defining the loss function of our MCMC (particularly in the first three equations). Please see our response to reviewer 2 for more details. We now provide a comparison of the NBB velocity with and without amplitude constraints.

3. How to interpret the uncertainty of the inversion results? The imaged anomaly has a really low uncertainty, is that trustable? The uncertainty does not seem to increase with depth, which is inconsistent with the general understanding of seismic imaging. I think the authors should discuss this in more detail.
We thank the reviewer for tis comment. We have expanded our discussion of uncertainty, in general, largely to address the concerns of Reviewer 2. On the subject of the low uncertainty on the NBB velocity, we have added lines 249-253: "The uncertainty of the NBB velocity in our model is lower than that of surrounding layers. While surrounding layers are primarily constrained by dispersion curve data, the NBB velocity sees significant constraints from the P-Ps amplitude ratio (as demonstrated in Figure S7). Conversely, any uncertainty in the observation or forward-modeling of the P-Ps amplitude ratio should almost exclusively carry over to uncertainty in the NBB velocity."
4. There are many details involved in the inversion method. I think the authors should make sure that the readers can follow each one clearly. For example: (1) What are subarrays (central channels, sub-array stations) of ambient noise processing? Where does the 1.5 to 3 wavelength / 0.5 wavelength come from? One or two sentences to explain the intuition behind these choices would be helpful.
We appreciate this suggestion. We have significantly revised this section of the text, and feel that this issue is now addressed.

(2) Many smoothing parameters are used in the inversion. I think the authors should discuss how these parameters are chosen and how sensitive the results are to them.
We have reduced the use of smoothing in our inversion and have added lines 230-234: "While we smooth our data at several steps throughout the inversion, this smoothing is generally on a much shorter lengthscale than the features resolved by our model. We generally spatially average on a lengthscale of 500 m, while the layer thicknesses and velocities tend to vary on lengthscales of kilometers. While we perform a more intense

spatial averaging in our initial, sparse inversion, the spatial averaging should not meaningfully bias our final, dense inversion.

(3) The modeling of Ps relative amplitude seems to have large uncertainties. I think the authors should discuss how these uncertainties affect the inversion results.
We have attempted to address this issue, as seen in our response to comment 3.
(4) It would be helpful to add an illustration in supplementary materials to explain Eq 1.
We have added such an illustration (supplementary figure S3), which we think should help in clarifying the equation.
5. As uncertainty quantification is one strength of the MCMC method and also important to help interpret the results (Figure 4). I would suggest combining the uncertainty quantification with the inversion results in the same figure.
We agree and have made this change.

Reviewer 2:
General comments –

- The paper would benefit from a clearer (and somewhat more detailed) description of the model parameterization procedure (e.g. what is fixed, what is allowed to vary? See more detailed questions below) and of the implementation of the inversion workflow. Are you conducting a truly joint inversion of all these observations? If so, how does it work? Do you apply a weighting scheme of the different observations depending on their reliability/uncertainty, or do all observations have the same importance in the inversion scheme? Please provide some more information in the text.
  We thank the reviewer for their feedback. We have expanded portions of the text to address this concern. The changes are described in our detailed comments, below.
- A slightly more detailed evaluation of the results and their uncertainties is needed. As I mention below, layer thicknesses, for example, have relatively large uncertainties of several hundreds of meters for a model that only reaches 2.5 km depth. Some more explanation on what could be the reason for these uncertainties would benefit the clarity of the results. For example, it would be interesting to comment on whether these relatively large uncertainties are coming from the inversion framework or from the uncertainties associated with the data itself. If they stem from uncertainties in the observations, which of the inverted parameters have a larger effect? How about the uncertainty on P-Ps amplitude ratio in relation to DAS directionality? We appreciate this suggestion. As we now highlight in the text, the model uncertainties are largely tied to uncertainty in the dispersion curve data. We project the predicted particle motion for *P* and *Ps* onto the direction of the fiber before comparing the two, so DAS directionality should not be the first-order control on uncertainties in the amplitude ratio.

Data and methods

- Figure 3: Caption refers to (a), (b)… but the panels have no labeling. Please label accordingly. The same applies to all figures in the Supplementary material.
  We have added labeling as suggested.
- You choose to apply the STA/LTA and maximum amplitude picking at each channel after spatially averaging the data over 500 m. Given a gauge length of 100 m, 500 m is quite a large spatial averaging window. Can you comment on why you choose this particular spatial length and how could this spatial averaging affect the spatial resolution of your final velocity model? Have you tried averaging with different spatial lengths?
  We have tried multiple averaging windows and found that reduced window sizes caused the STA/LTA algorithm to pick noise. In general, the gauge length should not necessarily be used as a metric for the required smoothing, as the smoothing needed is dependent on the ambient noise level and the amplitude of the observed converted phase, which may vary independently of the DAS configuration.
  We have now added the lines 105-107 to the text, which we hope will clarify our choice: "This smoothing allows for more robust arrival time picks (reduced smoothing leads to more false picks), and should not significantly impact on our final model, as we smooth our data on a similar spatial scale prior to inversion."
- Lines 121-122: " Thus, depending on the thickness and depth of the block, it may lie between two of the other layers or within a single layer, resulting in a five or six-layer model, respectively." I am not sure I understand the model parameterization for the channels with Ps phases. Is the depth of the block imposed? If so, how do you choose the depth at each channel? Moreover, from the explanation of model parameterization in channels with no Ps phases, I understand that the number of layers is fixed, and you allow layer thickness and velocity to vary in your inversion. How does it work in the section where you assume the presence of the block? You don't fix the number of layers anymore? Please provide some clarification on how you parameterize the model in both cases (with and without Ps phase present).
- We have added some clarification in lines 138-144:
- "We make no assumptions regarding the location of the block relative to other model layers, instead parameterizing the feature in terms of its location below the ground surface. **That is to say, we set the depths and thicknesses of the other four model layers and the slumped block is imposed on top of the four-layer model, replacing a portion of the model.** Thus, depending on the thickness and depth of the block, it may lie between two of the other layers or within a single layer, resulting in a model with up to six layers. **This approach allows for a consistent definition of the four non-block layers, both in the presence and absence of a block.**"
- Lines 123-124: "When a block is present, we invert for the velocity structure on the basis of dispersion curves, the observed delays between P and Ps phases from local earthquakes, and the ratio of amplitudes between said phases." - Is this a truly joint inversion of all these parameters at once? Please clarify.

Yes. At lines 145-150, we have adjusted the text to clarify: "**For the *i*th channel, assuming a converted phase is present,** we invert for the velocity structure on the basis of dispersion curves, the observed delays between P and Ps phases from local earthquakes, and the ratio of amplitudes between said phases, **with all data considered in a single loss function:**

$$L_i = \frac{1}{2}\left(\frac{(G(m_i)_A - \mu_A)^2}{(\sigma_A)^2} + \frac{\left(G(m_i)_t - \mu_t^i\right)^2}{\left(\sigma_t^i\right)^2} + \sum_{k=1}^{m} \frac{\left(G(m_i)_v^k - \mu_v^{ik}\right)^2}{\left(\sigma_v^{ik}\right)^2}\right)$$

Here we define $G(m_i)_A$, $G(m_i)_t$ and define $G(m_i)_v$ as the predicted amplitude ratio, travel time, and dispersion curve data, respectively, for a given velocity model, mi ." We now explicitly define the each observation (μ) and its uncertainty (σ) in the text.

- Lines 156-158: " In predicting DAS amplitudes, one must also account for the polarity of P and SV particle motion relative to the direction of a span of fiber" - How do you deal with this in your forward modeling?
  We have added a couple sentences to clarify this point at lines 177-181 stating "**To account for this effect, we forward model the expected particle motion associated with a given phase and project it in the direction of the fiber.** Channel-by-channel measurements of the amplitude ratio appear noisy, so we fit a global estimate of this ratio and its uncertainty (Figure S4). **Note that although we are measuring strain, our invocation of particle motion is valid, as our modeling assumes both arrivals have the same ray parameter and we observe that both arrivals have a similar period.**"
- Line 174: "… spatially averaging data at 800-meter windows." - Why do you choose 800 m windows if you average your data at 500 m windows for the STA/LTA, for example? The choice is somewhat arbitrary, as we just intend to average on a length-scale significantly less than that of the features resolved by our model. We now use a length-scale of 500 m for consistency.
- Lines 178-179: "Note that to achieve a stable solution, we have smoothed on a lengthscale of approximately one kilometer during pre- and post-inversion steps." - Why is further averaging after inversion required to achieve a stable inversion if you are already averaging the data at a lengthscale of 800 m (which is quite significant)? We have now removed our post-inversion smoothing. We take the median sample when plotting, rather than the best-fitting one, which allows for a more stable model. The spatial stability has also been improved by the changes to the inversion described above.
- Line 185: "…following lateral smoothing on a lengthscale of 800 meters)" - Shouldn't this be "a lengthscale of 1200 m" ?
  This was a typo, although the post-inversion smoothing is now removed as described in the preceding comment.

Results and discussion

- I agree that the fit of the model to the observed P-Ps delay times and dispersion curves is relatively good and within uncertainty. However, the predicted amplitude ratios between P and Ps, although within your uncertainty band, are systematically

lower than your global estimation. There is also a prominent peak at ~20 km distance from channel 5999 which goes beyond the uncertainty bound. Can you comment on why your prediction is lower than the observed mean value and how that affects your final velocity model and uncertainties?
We believe that both of these features have been addressed following the changes to our inversion, described above.
There also seems to be a relatively large uncertainty in the thickness of each model layer, with deviations in the order of a few hundred meters from the best fit and median value in some cases. I think that this can be considered significant given that the depth scale of the model is only 2.5 km. There also seems to be significant uncertainty in velocities in some regions of the model. Could these uncertainties be improved? Can you comment on the most likely source of these uncertainties?
This is an important point to clarify, but unfortunately we do not feel that these uncertainties can be easily improved, as they are somewhat inherent to the inversion of dispersion curve data with appreciable uncertainties. We have added a discussion at lines 225-229: "Certain model parameters have high uncertainty, particularly the thicknesses and velocities of the second and third model layers along much of the array. These model parameters are expressed in the data through fine-scale changes in the slopes of the dispersion curves (whereas the velocities of the top and bottom layers are tied to the asymptotic behavior of the curve). Thus, any uncertainty in our dispersion curve observation leads to significant uncertainty in the behavior of the intermediate model layers."

- Lines 191-193: "The top three layers have velocities consistent with valley fill under increasing compaction and lithification, while the bottom layer is consistent with an igneous bedrock." - Can you provide some references for that? Are there any seismic investigations in the area (or nearby areas with similar geology) that you can compare to?
  We now cite a paper on the seismic velocities of Southern California rocks, as a reference for this claim (line 219). While of course we cannot uniquely map velocities to lithologies, the paper shows that the top three layers in our model have shear velocities consistent with alluvium transitioning to sedimentary rock, and the bottom layer is more consistent with a crystalline rock. As discussed below, we now compare our interpretation of the geology with that described in Pakiser & Kane (1962) on the basis of seismic and gravity data (although that study considered P wave velocities, so our profile is not directly comparable).
- Lines 198-199: "This depth slightly exceeds that proposed for the block on the basis of resistivity data (400 m)…" - Can you provide a reference here?
  We now include a reference at line 249.
- Lines 203-204: "Given that a southern portion of the block was not accounted for in past gravity based inversions for the broader subsurface structure, such studies may have underestimated the depth of Owens Valley under Bishop, CA." Can you provide the references of these studies? Also, can you comment on what could be

the reason why these previous studies did not observe the southern termination of the block that you interpret here? Is it because of depth resolution issues?
We now include a reference, and add some explanation for the lack of resolution through gravity data at lines 246-249: "We would expect that gravity-based studies could miss this unmapped portion of the NBB. At its unmapped locations, the NBB extends to increased depths, where it is likely bounded by higher density material. As the block was initially identified due to a density contrast with its surroundings, this could make observation through gravity data more challenging (Bateman et al., 1965; Hollett et al., 1989)."

- In the introduction (line 37), you mention that one of the studies (Pakiser and Kane, 1962) conducted and active-source seismic experiment in the area that they compared to other geophysical surveys. However, you do not discuss here how your seismic velocity model compares to these seismic refraction profiles. Was the seismic experiment conducted near your dark fiber array? Can you compare the estimated depth and thickness of your block with that interpreted from the seismic refraction profile? If this information exists, a comparison is granted in this section.
We appreciate this comment and feel that the paper is bolstered by a clearer discussion of this existing seismic survey. We have added the following discussion at lines 254-263: "We may compare our results with the interpretations of seismic refraction and gravity data reported in Pakiser and Kane (1962), though our fiber is displaced from all lines described in that study by at least several kilometers. As in that study, we predict that the sediment-bedrock interface dips to the west. We also observe a maximum depth for the interface of around three kilometers, which approximately agrees with gravity data. The seismic data in Pakiser and Kane (1962) suggests a shallower interface, on the order of one to two kilometers. One could reconcile these results by claiming that the deepest layer imaged in the study was in fact the NBB, and given that the authors were solely interpreting travel-time data, distinguishing a thinner, high velocity layer from the true bedrock interface would likely be challenging. Indeed, the depth of the bedrock observed through seismic refraction is more consistent with the depth of the NBB in our model. As the seismic survey in Pakiser and Kane (1962) is located several kilometers to the south of our fiber, one could also explain the observed disagreement by claiming the basin shallows to its south."

Minor comments

- Figure 1 caption: "The earthquake plotted in Figure 2…" - should be "earthquakes".
Fixed.
- Figure 2: although it might be obvious, it would be good to label the figures with Event 1 and Event 2.
We have made this change.
- Line 97: closing bracket is missing after "Figure 2".
Fixed.
- Line 155: there are two "the" at the end of the sentence.
Fixed.
- Line 220: There are two "the" near the start of the sentence

Fixed.

Supplementary material

- Figure S1 caption: “Dispersion curve are produced for many virtual sources and stacked to produce the background image.” Do you mean dispersion spectra, instead of dispersion curve?
  Yes, we have now changed the wording.
- Figure S4: I think that the blue shading is the uncertainty in the global estimate, is that right? If so, please specify that in the caption.
  We now refer to “uncertainty plotted as a blue shading” in the caption.

Dear Dr. van der Lee,

We greatly appreciate your time and feedback in editing our paper. We have adjusted the text to address your comments and give detailed replies in the document below.

Thanks you,
Eli Bird
(on behalf of co-authors)

- Main paper:

* The legend and/or caption of Figure 1 should comment on the orange and purple dots/circles along the same-colored fiberoptic lines.
We have added the following text to the caption: “Orange and purple circles along the fiber denote every five-hundredth channel.”

* l.104 has two "using a"
This typo has been corrected.

* The caption of Figure 3 should mention which velocity (phase velocity) is shown in panels b and c. It would facilitate comparison if the x axes of panels a, c, and e had the same width. The meaning of panels e and f, for an "infinitesimally thin converting boundary" (or layer?), need to be explained and detailed in the text.

We appreciate this feedback and believe the issues have now been addressed. In the text, we now state “the fit to data is approximately identical for a block of 0 m (Figure 3e-f) and 200 m (Figure 3c-d) thickness. In the former case, we assume that there is a layer in the subsurface producing a converted phase, but take said layer to be infinitesimally thin, such that it has no influence on the modeled dispersion curves. While such a layer is not physically realistic, comparing the infinitesimal result with one wherein the block has a finite thickness allows us to determine at what thickness the block becomes seismically visible through our dispersion curve data.”

The caption reads “Fit of model to data. a) Modeled and observed time delay of {\it Ps} phase from direct {\it P} arrival with observation uncertainty plotted in light blue b) observed Rayleigh phase velocity dispersion curves c) modeled dispersion curves, and d) dispersion curve residuals (normalized by observation uncertainty). We also plot e) modeled dispersion curves, and f) dispersion curve residuals for an inversion using an infinitesimally thin converting layer.”

Finally, we have adjusted the x axis of panel a, to align with the other panels.

* Section 2.4 presents a Loss function. While it might be obvious that you mean to minimize this function when you say you invert for velocity structure or use it to define a likelihood function, it is better to explicitly state what you use the Loss function for. The actual inversion workflow is presented in section 2.6 which discusses likelihoods and

posterior probabilities that remain unlinked to the stated loss function in section 2.4. In terms of determining the thickness of the slumped layer, it would be helpful to merge l.166-168 with l.225-227.

For greater clarity, we flip the sign of the loss function and now refer to it as a log-likelihood function. We add the following text in section 2.6 to clarify how the function is implemented: “At each iteration of the inversion, the algorithm proposes a step to a new location in the model space. The log-likelihood function defined by equation 3 is used to determine the relative probability of the current and proposed locations, defining an acceptance ratio in moving to the proposed location.”

We combine the text in reference to the thickness of the slumped block, as suggested.

* I appreciate the explanation of eqn 4, and find that this equation assumes that below the converting boundary the P and Ps ray paths are parallel ("arriving energy has propagated as a plane wave") and re-converge at the receiver. If one assumes that the P and Ps ray paths are the same (below the converting boundary) and diverge after passing the converting boundary then eqn 4 does not apply as written, but applies with the cosines moved to the denominators, which does not change the units.
We agree with this description. We have also attempted to clarify this point, adding Figure S4 and altering the text describing the ray path geometry (altered text is italicized):
“We *roughly* approximate the ray parameter between a given event/channel pair as the delay between the source time and arrival time, divided by the lateral distance of the source from the receiver. We find that *the approximate* ray parameters are relatively consistent across all event/channel pairs, typically ranging from 0.19 to 0.25 s/km. Thus, for each channel, we calculate a mean ray parameter across all events, used in forward modeling the {\it Ps}-{\it P} delay. Note that this approximation of the delay time assumes that both arrivals follow *approximately* the same ray path *at depth* and *remain parallel until they reach the converting boundary*. This should be approximately true, as our results show that the ray parameters of the events are roughly equal to the velocity of the deepest layer in our model. This means that both rays should travel nearly horizontally through said layer. *The geometry of these ray paths is clarified in Figure S4.*”

* l.183-185 discuss a crude approximation of the ray parameter, which is probably fine, but I disagree with "This approximation assumes that the subsurface is locally made up of homogeneous horizontal layers". Even more than one homogeneous horizontal layer will change the ray parameter p from a direct wave p to a smaller, refracted-wave p beyond a certain distance. With more layers, even homogeneous horizontal ones, p will vary with distance.
We have removed the text in question.

* two "the" in l.187
Corrected.

* line 191" "diverge as they exit the layer." pertains to the deepest layer, whereas your P to S conversion takes place at the top of the slumped layer, buried in the shallowest layer.
We intended to say that the parallel rays were approximately colocated when subhorizontal in the deepest layer and began to separate as they entered shallower layers (despite staying parallel). The phrasing was confusing, though, so the text has been removed.

-- Supplement:

* Fig S1 has an orange and a blue line, without a legend or ID in the figure caption. More importantly, why does the slowness vary along the cable? Is it correct to interpret the varying slowness as a result of varying incidence angles or would it be more appropriate to infer varying near-surface velocities?
We have changed the caption and figure to clarify this point. The caption now reads “Average predicted incidence angles for earthquakes used in the study, using the mean observation of horizontal slowness at a given channel and the seismic velocities at the surface from our output model. {\it P} and {\it Ps} incidence angles are plotted in blue and orange, respectively. This result demonstrates that even with the observed shallow velocity variations (the primary control on the plotted variations), we still expect earthquakes to arrive at steep incidence angles.”

* Please place the references after the Figures.
We have made this change.

* l.29 "desity"
We have fixed this typo.

* Fig S4: are "predicted" in the legend and "fitted" in the caption the same? Please use the same term. It also seems as if the "observed" and "predicted" labels are flipped.
We have taken this comment into account. The labels are not flipped, but we are plotting a prediction of the amplitude ratio which varies laterally along the fiber against a single, global estimate of the observed amplitude ratio. To clarify this point, the caption now reads:

“Comparison of observed and predicted amplitude ratio data. Note that we are only considering a global observation of the amplitude ratio between {\it P} and {\it Ps}, plotted in blue, with uncertainty plotted as a blue shading. In contrast, we calculate the amplitude ratio predicted by our model at differing locations along the fiber, plotted here in black.”

Dear Dr. van der Lee,

We greatly appreciate your work as editor on our paper. Below are our detailed responses to your comments. Note that during this revision, the code has been edited to produce additional figures. Currently, the repository listed in our data availability statement still has the old version of the code but we will upload a new version to CaltechDATA if the manuscript is accepted.

Thank you,
Eli Bird (on behalf of co-authors)

You state that Owen Valley and the NBB are important on account of volcanic activity and its role in supplying LA with water. At the very least, can you say when Long Valley Caldera was last active as a volcano (and cite a paper), and provide more details on how and how much water is supplied to LA, also citing pertinent papers/documents?
We have made the recommended adjustments.

2. Figure 1: Please label "Sierra Nevada Mountains", "White Mountains", and "Long Valley Caldera" on the map.
We have made this adjustment.

3. While Owens Valley is referred to as a "tectonic basin", NBB is referred to as a "tectonic block". Please elaborate on the meaning of these definitions.
For clarity, we have changed “tectonic basin” to “rift basin,” rather than defining the term where it appears in the abstract. We have also removed the term “tectonic block,” as it implies more detailed knowledge of the slumping process than we have.

4. Shortly after stating that within the extensional Basin and Range Province, the NBB is a piece of White Mountains that detached and slumped westwards and downwards before being buried by 400 meters of sediment, your write that the NBB's origins are poorly understood. Does that imply you do not believe that this slumping was the result of the Basin and Range's extensional forces?
Thank you for this comment. Our initial text was unclear. We do not believe invoking the extension is enough to explain how the block would slump. We have edited it to say (Line 45): “Producing a feature of this size would clearly require broad-scale forces along a span of the mountains, potentially of great interest to the area's geologic history, but the exact way that extensional forces in the basin and range province would translate to slumping of material onto existing valley fill remains unclear.”

5. At the end of section 1, you say "fiber-optic seismology only constrains along-fiber strain". Please elaborate what that means? Is this shear strain or compressional strain, or both, and if both, are they distinguishable? What quantity is shown, filled with blue and red colors, in Figure 2? Strain? If yes, what type (please list its components).

Displacement (inferred from strain?)? And what about the "along-fiber strain" makes it possible to detect both P and Ps waves?
We have adjusted the text to say (Line 59): "Despite its advantages, fiber-optic seismology only constrains along-fiber strain (i.e., changes in the length of a span of fiber over time, which will appear in response to both P and S arrivals from an arbitrary source location. Note that as the reference frame is the initial length of the fiber, both a shearing of the fiber and a compression or extension can produce a strain, which cannot necessarily be distinguished.)." The caption of Figure 2 now begins with "Example along-fiber strain rate time series for Events 1 & 2…" to clarify the point.

6. Figure 2: Please also label the coherent arrival 2+ s after P for event 1 and 4+ s after P for event 2.
We now label the arrival as the direct S arrival.

7. Section 2. What does it mean when an optic fiber has a channel spacing of x m and gauge length of y m? In other words, what is a channel and what is a gauge in the context of an optic fiber?
We now state (Line 73) "Note that gauge-length is the spatial interval which is utilized in producing estimates of strain and may be treated as a moving spatial window over which strain is averaged before being output at denser channel spacing."

8. The end of section 2.1 discusses the Ps arrival having higher amplitudes than the P arrival. The original version provided an explanation based on incidence angles, which is now deleted. Please provide another explanation? If the explanation is based on incidence angles, please provide their values for P waves from event 1 and event 2. I note that you provide an average ray parameter range in line 177, but please also provide the corresponding incidence angles.

We have added text (Line 79): "In contrast, converted S arrivals may take on higher amplitudes than P arrivals, especially when approaching the array at steep incidence angles. Incidence angles for P and S arrivals are plotted in Figure S1.

Incidence angles will vary with event and location along the fiber (based on distance from the event to the receiver and the velocity of the shallow subsurface below the receiver). Thus, rather than listing the incidence angles for the two events, we plot the mean incidence angle at each fiber channel for both *P* and *S* arrivals in Figure S1

9. l. 67-68 "After the array crosses onto the previously proposed location of the North Bishop Block, earthquakes observed on the fiber begin to present" --> "Earthquakes recorded on the fiber where it crosses the previously proposed location of the North Bishop Block, present"
We have made this adjustment.

10. l.79 delete "take steps to preprocess these raw data,"
We have made this change

11. l.82 "source" --> "shot gather"? I am not sure the term "virtual source" is used correctly in the subsequent text.
We made this change and generally edited text that referred to a "virtual source." While the term is still used in some cases, we believe the problem is addressed.

12. l.96-97 please specify that you approximate the wavelength by dividing an estimated velocity of 2 km/s by the wave's frequency/frequencies.
We now state (Line 101) "…using a velocity of 2 km/s (dividing 2 km/s by the frequency in Hz)"

13. l.98 replace with "The mean, mu, and variance, sigma-squared, of the frequency-dependent phase velocities that define the dispersion curves can then be defined as"
We have made this change.

14. l. 110-111 Arrival times and thus STA/LTA ratios change a lot over 500 me, so you need to elaborate a bit on more on what "We spatially average the STA/LTA on a length scale of 500 m before picking arrivals." means. Figure S2 did not help me understand what you did.
In our view, arrival times generally do not vary significantly over 500 meter distances in the plots shown. In general, we see smooth variation in arrival time of the phase on the order of 1 second over the >1000 channels considered, so smoothing our STA/LTA over 50 channels (500 meters), seems valid and works in the sense that our STA/LTA based picks are reasonable when checked visually. Is it possible that you are looking at the variation in travel time over 500 channels (5000 m)? To clarify this, we now state (Line 115) "…on a length scale of 500 m (50 channels) before picking arrivals…"

15. l. 127 Please specify what couples to what in the concept "coupling".
We replace "coupling" with (Line 131) "…coupling of ground motion to the fiber…"

16. l 141-142: P-velocity and density, in particular, are not always positively correlated with S-velocity, especially in the crust. It is good practice to also, for comparison, do an inversion that fixes P-velocity and density and only inverts for perturbation in S velocity (and optionally layer thicknesses).
We have added supplementary text (S1) and figures (S6-S7) to address this point.

17. Figure 3: It is good practice to use a different color scale for standard deviation than used for velocity, especially if one switches units from km/s to s.d. This figure also needs a dispersion data fit subplot for a simpler 4-layer model without a "slumped block".
We have made these adjustments.

18. Eqn (4): For incidence angles i (for P) and j (for s), the length of the ray segments in a layer are h/cos(i) (for P) and h/cos(j) (for s), instead of h*cos(i or j), where cos(i) is usually written as sqrt(1-(p*alpha)**2). Your notation works too, but the cosine needs to move to the denominator. If eqn 4 was implemented as written, it could have led to overestimation of the depth estimate to the top of the "slumped block".

We appreciate this comment, our initial text conflated two values, which were not in agreement with one another. The first was the vertical slowness multiplied by the depth (this was the value in the equation and what we used in the actual analysis) and the second was the length of the raypath divided by the seismic velocity (which was described by our figure). We have removed the figure which conflated the values and explained the equation in more depth where it is introduced.

To expand on things a little more, consider the vertical slowness η:

$$(\eta \cdot \alpha)^2 + (p \cdot \alpha)^2 = 1 \rightarrow \eta = ((1 - (p \cdot \alpha)^2)^{0.5})/\alpha$$

This vertical slowness should be multiplied by the depth to produce a travel time (in dividing, one would produce an output with units m^2/s), which will result in our initial equation. Assuming the rays are approximately collocated at depth, multiplying the depth to the converting boundary by the vertical slowness should be sufficient in determining the difference in arrival time.

19. l. 168-169: rephrase.
We now state (Line 172) “At any given channel, we forward model the delay between the {\it P} and {\it Ps} arrivals. We assume that the arriving energy has propagated as a plane wave across a locally horizontal, homogeneous medium.”

20. l. 193 I don't understand what the ray parameter and period have to do with particle motion. Do you mean to point out that the measured strain is longitudinal (along the fiber) and proportional to particle motion, regardless of whether it is measured in displacement or velocity (hence the mention of period?)?
The mention of the ray parameter was unnecessary and has been removed. We have clarified our discussion of the period (Line 193) “Note that although we are measuring strain rate, our invocation of particle motion is valid, as we observe that both arrivals have a similar period (such that amplitude ratios will be preserved when taking the derivative in time).”

21. l. 199 500 m steps in what?
We now clarify (Line 200) “at 500 meter steps along the length of the fiber.”

22. l. 201-202: rephrase.
We have rewritten this as (Line 202) “Running the MCMC inversion on data from a given channel, we produce a model posterior distribution local to said channel.”

23. Figure 4: You need to say which velocity is plotted. And please indicate which interface is assumed to be responsible for the observed Ps arrival? It looks like the velocity contrast between layer 2 and the bottom of the "slumped block" should generate nearly as strong of a negative converted phase as the top produces a positive one. This needs to be discussed.

We now state which velocities are plotted. We have edited the text to say (Line 141) “Note that based on the interpretation presented here, a converted phase from lower bound of the block is also likely recorded, but will closely follow the {\it Ps} arrival from the top of the block, and thus be difficult to distinguish from its coda."

24. Results: With a thickness of 200 m, the "slumped block" looks more like a rumpled bed sheet? How do you go on to call it a "block"? Please explain how it might have formed or what it is. Stevens et al. also discuss a Bishop Tuff, could this play a role in generating the Ps phase?

Thank you for this comment. It is likely worth clarifying that our results cannot definitively be tied to a slumped block and that said interpretation is used primarily because it is consistent with past work on the region. We now discuss this point and directly address the tuff:

(Line 281) "Note that our data do not conclusively indicate phase conversion by a slumped block, as our model could be consistent with a variety of geologic features with anomalously high velocity at depth. However, the feature we image is consistent in depth and location with that reported based on gravity and resistivity data, so we maintain an interpretation consistent with that past work Hollett et al. (1989). Further, while a variety of features could produce the same data in practice, such alternative features have not been identified in the literature. Note that while volcanic ejecta from the Long Valley caldera, known as the Bishop Tuff, would likely take on similarly high wave speeds, well logs indicate that the tuff is significantly shallower than our observed anomaly in the region Bateman et al. (1965)."

25. l. 261-265 Please clarify. What particular relevance? How would it underestimate the depth of the valley? What does "depth of valley" mean here? How are hydrological maps affected?
We now state (Line 256) "Given that a southern portion of the block was not accounted for in past gravity-based inversions for the broader subsurface structure, such studies may have underestimated the depth to igneous bedrock beneath sedimentary valley fill in the Owens Valley under Bishop, CA (as a layer of igneous rock may take on higher densities than the surrounding valley fill). These new insights into the subsurface require further consideration and may affect subsurface hydrological maps of Owens Valley (as the igneous base of the valley and the overlying sediment will likely differ in their porosity and permeability), a critical product in water resource management."

26. l. 276: Do you predict or infer this?
We have edited the text to say "infer" rather than "predict"