**Review 1**

Thanks to both reviewers for very thorough, helpful, and generally supportive reviews. We have improved the manuscript in response to the reviews in several ways. Details follow:

**Response to Reviewer #1**

*My main criticism is the ad-hoc nature of the input "data". One one-hand, you have receiver function measurements derived from a 3D CCP stacking, which is a crude (albeit useful and enduring) approximation of the scattered wave imaging problem for S-wave RF, and, on the other, a reverse-engineered measurement for P-wave RF, extracted from 1D joint-inversions with surface wave data [Shen and Ritzwoller]. It is a curious mix of heterogeneous results and methods, e.g. the RF travel-time 'data' result from first a conversion from time-to-depth in the original studies (via inversion or stacking) then back to time as 'data' for this study which is then mapped back to depth with another inversion. What is gained by this and what is lost?....The idea of inversions of inversions makes one wonder how well the resulting models actually represent the underlying data, particularly the RF traces. My suggestion here is to calculate some examples of predicted receiver functions for your inverted velocity models and to compare these synthetic traces with the actual RF data. This doesn't need to be exhaustive, but a few example stations would be illustrative. Adding this analysis would be a useful check as to the internal consistency of the modelling results and would give the reader a sense of the effects that the modelling assumption might have (particularly given the discussion in Section 4 of the supplement!). You would obviously not be able to match RF from stations sitting in basins (since you are not modelling the shallow crustal structure) but there a plenty of good sites without strong reverbs in the study area.*

We agree that this is potentially confusing, and have clarified the reasons for this choice in the text for Sect. 3.2. Motivated primarily by the desire to better quantify the nature of NVG, the algorithm is designed to utilize both timing and amplitude information on any specified "discontinuity". For the study region (and the continental US in general), such constraints are directly available from S-p receiver function studies for the NGV, but to our knowledge there is no comparable set of Moho constraints (in particular amplitude or velocity contrast) for the study region that come directly from the converted-waves themselves. The closest we could come is the Shen and Ritzwoller joint inversion. Now included in supplementary section S4 is a comparison between the P receiver functions predicted for 3 sites with minimal sedimentation between the model of Shen and Ritzwoller, 2016 and this study (we do not have the observed RFs from SR16, but assume their model fits those well). Some sediment reverberation is always present – but the agreement between the timing and amplitude of the Moho phase is good. This work is referred to in the revised text on Line 266.

*Additional Comment: It is great to see people studying this fascinating area of WUS. However, there are other broadband seismic data in this area which often gets ignored.*

*The CREST array deployment (network XP, 2008) was not technically part of FlexArray (one of the last CD experiments) but recorded high quality data coincident with the Transportable Array. The first study to apply S-wave receiver function imaging around the Southern Rockies was Hansen et al. [2013; G-cubed] who also linked lithospheric structure to the local volcanism (including ultra-potassic). They also identify the crustal low-velocity anomaly below the San Juans that you observe. At the very least, it is worth citing this paper somewhere.*

Indeed, we definitely have tried to capture many of the excellent regional studies, and we have added the suggested reference with text making the connection to the San Juan Mountains clearer on line 297. Thanks for catching it.

**Moderate edits:**

*Section 3 could use a bit more detail. It was not clear on first reading what the "amplitude" data are or where they come from when discussed later, e.g. Line 238 and Fig. 6. Make it more explicit that you are extracting two quantities from both RF datasets. The word amplitude in the RF context usually means the amplitude of the converted arrival as observed on the RF trace. Make it clear that you mean a prescribed velocity contrasts.*

We have first clarified the text in section 3.2. Then as the term amplitude is inaccurate, it has been removed from Fig. 7 and line 238 (previous version). The term 'amplitude' does not appear elsewhere.

*I found Section 4 to be quite confusing the first time I read it. You talk about the construction of the random synthetic models in some detail, but the actual model parameterization is different (doesn't use polynomials) and only discussed in the appendix. For example, this makes it quite difficult to understand how the models in Fig. 5 all can come from the same model parameterization, e.g. why the yellow models have no NVG. I suggest cleaning this up and adding the model parameterization in the main text (it is more important in my opinion).*

The model parameterization is now in the main text and Fig. A1 is now Fig. 5. We have also simplified the discussion of the random synthetic models in the main text, and details on their construction (i.e. "polynomials") is limited to the supplement.

*A related clarification, make sure to clearly distinguish what are inverted model parameters, and what are derived quantities from the inversion results, e.g. in table 1.*

We have revised the caption to clarify that each of these quantities are extracted directly from inverted results.

*Figure 4: add figure S1 here as a subplot. This is one of the key datasets and should be included in the main text with the others. Even though it is treated as a constraint rather*

*than data. This also begs the question, why not just use the NVG width as 'data' like the other derived quantities?*

The breadth not quite the same as the other S-p observations because it is baked into the parameterization, and not explicitly fit. For this reason, we prefer to present it as a choice of the parameterization (the impact of which we evaluate in Figure 12), rather than an observation that is modeled through inversion. As noted in the manuscript, the breadth is much less well-constrained than the other S-p observations (see Hopper and Fischer, 2018), and it is not likely to be widely available as an observable in other possible applications of the algorithm. In many applications, this breadth will need to be assumed (like for the Moho here). To help reduce possible confusion, we have clarified in the caption where the breadths are in the supplement. We also have corrected an error on the methods figure (formally Figure A1, now Figure 5) that may have overstated the role of a possible observational "thickness" constraint.

*Line 238: You say here that you are fitting the Ps conversion amplitudes, but this is not right. You are fitting the estimated moho velocity contrast from a previous inversion that included PRF data.*

We have revised the text accordingly.

*In the supplement, you talk about what is controlling the sub-Moho gradient, i.e. the flip from positive to negative see in Fig. S2. I reached the exact opposite conclusion here. Changing the crustal thickness and crustal velocity gradient seems to have no effect (Fig. S2a,b). It is only perturbing the NVG which seems to cause this sign flip.*

The purpose of this figure to explain why the synthetic test in Fig 6b features a positive sub-moho gradient when head-waves are not included. What we find is that models with anomalously thick crust or steep Vs gradients consistently require the use of the head-waves to reconstruct a negative gradient below the crust. Without thick or steeply varying crust, the upper mantle can be accurately imaged without the head-waves. We have also corrected the reference to the figure in the main text, which may been a source of confusion.

**Minor edits:**

*Line 100-102: run-on sentence*

*Line 104: add figure reference here to your map.*

*Line 113-116: Consider adding a statement on the volcanism on the East side of the CP in the SRM. This is conspicuously absent.*

*Line 268: Say San Juan Mountains instead of just the coordinates. This important geological feature deserves better.*

We have adopted all of these recommended modifications to the text.

**Response to Reviewer #2**

*I feel that the authors should add a figure describing the parametrization of their velocity model in the main text, probably by modifying Fig. 15 and bringing it up to the main text.*

Fig. A1 is now Fig. 5 in the main text.

*Line 37: I would prefer "magmatism" over "volcanic evolution" before the former is a more general term encompassing both magmas that solidified at the surface and in the crust.*

The text has been revised accordingly.

*Line 60: I don't understand what "ad hoc" means here. Please explain.*

The comma has been removed, making "ad hoc" an adjective to constraints.

*Line 62–63: Please cite Liu and Shearer (2021), which also presented a high-resolution image of the LAB beneath the Western United States. Their image contains features that also appear in the authors' model, such as a shallower LAB beneath the eastern edge of the Basin and Range Province, and thus might be worth discussing.*

Thanks for pointing this out, we have included the citation on the mentioned line, and again on line 280 (revised text) where the shallower LAB is mentioned in our results.

*Line 83: Again, I don't understand what "ad-hoc" means here.*

We have replaced "ad-hoc" with "imposed".

*Line 101: Again, better to replace "volcanic" with "magmatic".*

We have revised the text accordingly.

*Line 118: "Marysville" should be "Marysvale".*

We have revised the text accordingly.

*Line 164: Please clarify if "spatially varying Vp/Vs" means a varying Vp/Vs only in both the crust and the mantle or only in the mantle.*

We have clarified that *Vp/Vs* is only varied in the crust.

*Line 176: Why don't the authors simply use the spatially varying Vp/Vs model from Schmandt et al. (2015) here?*

We estimate that having made the correction for the RF constraints, there would be no further discernable impact on the model and chose to simplify the modeling procedure.

*Line 191: I cannot find where the authors describe how the velocity in each layer is parameterized in the main text, which is qsuite important for understanding their method. That is why I think there should be a figure showing the model parametrization.*

In concordance with this comment and the comments from the first reviewer, the model parameterization is now given in the main text and Fig A1 is now Fig. 5.

*Line 207: I feel that, compared to absolute velocity changes, velocity-change percentages measured against a reference value can better convey the magnitude of velocity change to the readers as tomographic studies usually show their results in velocity-perturbation percentages. This also applies to other places where the authors cite velocity changes.*

Variations from the mean are certainly of interest and are now included in the relevant figure captions. However, displaying absolute velocities are necessary to our overall interpretation and discussion, and so the figures display those.

*Line 220–221: Are the Chebyshev polynomials used for defining the velocity models that generate the synthetic data or the velocity models used for the inversion? Please clarify.*

We have revised to text to simply point out that we use a wide range of smooth models for the synthetic modeling, and have moved details of the construction of those models (e.g. Chebyshev polynomials) to the supplement. Along with the previously noted improvements to the "Model Parameterization" section (4.2 and Figure 5), the difference should be clear.

*Line 234: Is Intermountain West equivalent to the study region?*

We have revised the text to simply say "the study region".

*Line 236–239: Please explain why 1 is an appropriate threshold for the mean squared error.*

We have included a note that 1 means the data is, on average, fit to the error on line 262.

*Line 251: Liu and Shearer (2021) also see a shallow LAB at the eastern edge of the northern Basin and Range Province.*

We now include the reference.

*Line 268-269: On the contrary, I see a clear correlation between the velocity in the lower crust and the velocity gradient in the lower crust beneath the Basin and Range Province.*

We have revised the text to include this correlation.

*Line 277–278: Please mark the Marysvale volcanic field and other tectonic features at least in one of the panels of Fig. 8 so that the readers don't have to go back to Fig. 1 to find them.*

We have included this information on panel A.

*Line 284–286: I suggest showing a comparison with the P-velocity gradient map of Buehler and Shearer (2017) in one of the supplementary figures. Here is the link for downloading the results: https://ds.iris.edu/ds/products/emc-pnus_2016/.*

To address and expand this comparison, we have expanded the discussion of sub-Moho velocity gradients to include more specific discussion of both Buehler and Shearer, 2017 and Shen and Ritzwoller, 2016 in the paragraph now starting on line 303. We refrained from including the figures in the supplement since we discuss both models, and instead give specific references to figures in both Buehler and Shen on line 308.

*Line 319: The meaning of "Modeling Choices" is not immediately clear. Please consider choosing a different subtitle.*

We have changed the title to "Impact of Modeling Choices", which we feel reflects the points we are trying to communicate by performing a number of tests of the importance of key choices on the resulting models.

*Line 339–340: Is the difference in areas with a thick crust caused by the dispersion data not being able to distinguish a thicker crust and a lower velocity at the top of the upper mantle?*

The crustal thickness is robustly estimated due to the inclusion of Ps times, so there should not be a large tradeoff between thickness and mantle velocity to fit the surface waves. Our synthetic tests suggest that the surface-waves alone capture the average lithosphere velocity (as noted in the text), but they have trouble resolving the gradient below the Moho. This is especially true when the crust is thick, possibility because the longer periods active in the mantle when the crust is thicker have reduced vertical resolution. The Pn constraint, in contrast, strongly influences the sub-moho gradient, driving the difference. We have added a reference to Supplemental Section 2 that explores this issue in depth.

*Line 390–392: Why do the authors assume that the mantle rock is at the solidus? Should it vary with the chosen mantle potential temperature?*

Yamauchi and Takei (2016, 2020) argue for strong control on velocities by the proximity of the temperature to the solidus, with the strongest effect when temperature is close to the solidus (the “pre-melting effect”). The solidus is strongly dependent on volatile content, as noted in the manuscript. Rather than explicitly assuming volatile content, we assume that in the asthenosphere, the temperature is close to the solidus, which is very likely for the western US – see the cited study of Yamauchi and Takei, 2020 for a discussion of this issue. This assumption maximizes the velocity variation in the YT solid state predictions, which allows us to confidently state that velocities below the YT predictions must contain melt.

*Line 440: I guess “vertical shear-velocity gradient” means “shear-velocity gradient beneath the NVG”. Please clarify.*

We have adopted to suggested language.

*Line 450: “than” is missing.*

Thanks, corrected.

*Line 478: Does “geometrical factors at the base of the lithosphere” mean the depth of the NVG? Please clarify.*

We have replaced this phrase with “topography on the base of the lithosphere”.

*Line 500: “oC” is a typo.*

Corrected.

*Line 506: What is the temperature of the solidus?*

We have updated the text to quantify the estimated dry and damp solidi temperature at 95 km depth. The word “entire” for the Colorado Plateau has been removed.

*Line 509: “than” should be “that”. What does “these regions” refer to?*

The typo has been corrected and “high-temperature” added between “these” and “regions”.

*Line 510–512: I don’t understand the argument here. If the temperature estimate at the NVG is lower due to a non-zero melt fraction, does it make the interpretation of LAB less plausible?*

We removed the phrase in question to avoid any confusion as the argument is not key to section.

*Line 517: The Marysvale volcanic field is missing in Fig. 13.*

In the text we now clarify on line 535 that only volcanic fields above regions where we did not infer melting are shown.

*Line 520–522: The model described here is unclear to me. An alternative model is that the lithosphere beneath these regions contains a significant amount of melt, causing unusually low velocities in the lithosphere*

We clarified the language describing this model. In some sense, this is semantic, in that we do not consider regions with either very high temperatures or significant amounts of melt to be lithosphere, but prefer to call them asthenosphere.

*Line 540–541: Why is the LAB depth consistent with the generation of primitive melt?*

We have clarified that petrology constrains the depths at which the melts are generated.

*Line 553–554: Why is recycled oceanic crust ruled out here?*

The text is revised to explain that this can only occur at higher temperatures than inferred in Fig 13.

*Line 557–570: Please explain in further detail how phlogopite is formed in the mantle lithosphere and what caused it to melt.*

We have clarified in the text that how veins of phlogopite form is not well understood on line 592, and that the high temperatures inferred here are above the phlogopite solidus on line 601.

*Line 569: "by" is missing here; Line 575: "allows" should be "allow"; Line 581: "explaine" should be "explain".*

Thanks, each correction has been made.

*The annotations of most of the figures are too small.*

Text in most figures has been enlarged.

*Fig. 6 looks too sketchy and thus needs to be redesigned. Please consider splitting the surfacewave and body-wave misfits into two panels, replacing the crosses with colored markers, and making the axis labels horizontal.*

(now Fig. 7) We have split the figure into two panels (adjusting the main text and caption accordingly), replaced the crosses with colored markers, and made the text horizontal. Bars for minor ticks were added for readability.

*Fig. 10: The stars are difficult to see. Please consider reducing the saturation of the color map to make the stars more visible.*

(now Fig. 11) We have made the background velocity map grayscale and enlarged the stars.

*Fig. 13: Please draw boundaries separating the areas where the NVG represents the LAB and where it represents an MLD.*

(now Fig. 14) We have added a dashed line to panels A and C approximately following the 1300 degree contour and discuss them in the text on line 538. We have now noted that the Black Hills probably feature an LAB on Line 543, as was made clear by this edit.

**Review 2**

We thank the first reviewer and the editor for thorough and constructive comments on the revised manuscript. Below are answers to the questions raised in this round of reviews, with comments from the review in italics. Line numbers refer to the tracked-changes version from the previous round unless otherwise noted.

<u>First reviewer</u>

*The meaning of the model parameters tk seems to be inconsistent between 4.2 and Appendix. In 4.2, it seems to denote the depth to a discontinuity (e.g., if Moho is 40 km deep, tk = 40 km), whereas in Appendix, it seems to denote the thickness of the layer immediately above a discontinuity. This confusion makes it difficult for me to understand the top three equations in Fig. 5.*

Text at the top of section 4.2 was incorrect. The revised text describes *t* as "a vector of the thicknesses of layers above discontinuities in the model, in this application corresponding to the crust and the mantle layer above the NVG".

*The authors use both VSV and m to refer to the velocities at the MINEOS grid points in 4.2 and Appendix, which is confusing to me. I suggest them using a consistent notation instead.*

All references to the MINEOS grid points as m are now vsv, consistent with Fig 5. This notation is now defined in the appendix.

*Lines 214–217: This sentence is unclear. Please try to split it into multiple sentences to make it clearer.*

We have split the sentence in two and clarified the wording in the first half.

*Line 218: Should the semicolon be a colon instead? The comma before "and" should also be deleted.*

We adopted these changes.

*Lines 250–254: I suggest deleting this incomplete description of how the synthetic models are generated, which only causes confusions to me.*

We have removed the text in question and combined the now remnant first sentence with the succeeding paragraph.

*Line 291: Delete the "of" after "NVG"*

Corrected.

*Lines 294–295: How is the lower crust defined here? Is it equivalent to the 10 km thick layer above the Moho mentioned in the previous section?*

We have added a sentence defining the ranges that the gradients are taken over above and below the Moho (which are as the reviewer suspected).

*Lines 321–324: The two comma-separated parts of this sentence are largely unrelated and thus are better split into two sentences. In addition, the author should specify which surface velocity map in Fig 2 they are referring to here (I assume it's B).*
*Line 345: The authors should explicitly say that they are talking about the shear velocity below the NVG here.*
*Lines 480–481: The parentheses around "Hammond and Humphreys, 2000" should be removed.*

We adopted all these recommendations.

*Lines 521–522: What does "as high as any other region on Earth" mean?*

We replaced "on Earth" with "in their model".

*Fig. 5: The left side of the "Dependence on t" equations are missing the subscripts "a", "b", and "c". The same applies to the "Dependence on s" equations. The correspondence between the colors of the boxes around the equations and the arrows in the depth profile is also not easy to see. I suggest using other ways (e.g., line connectors) to show this relation instead*

We updated the figure accordingly.

*Fig. 10: In B and E, "Vs below the Moho" should be changed to "Average Vs between the Moho and NVG" because the former could mean Vs at the top of the upper mantle.*

We have updated the text in Fig 9 (the marked changes manuscript had erroneous figure numbers which apparently occurs with the Latex package that created this copy).

*Fig. 11: Why do the stars in B have different sizes?*

The stars are now the same size.

*Line 722: "course" should be "coarse". "with" should be deleted.*
*Line 735: "5 A1" should be "Fig. 5".*

The text has been corrected.

Comments from the editor

1) *You could make a change to the text to indicate that you tested using Vp/Vs from Schmandt et al., and it made no difference. Maybe you did and I missed it, but the response does not indicate that. Describing the hard work you did testing various possibilities lends support to the result.*

This is now discussed in Section 8.2.

2) *There was a question from the reviewer about why you are asssuming the mantle is at the solidus. Your response is good. However, again, please include it in manuscript (if not there already and I missed it) because it is illuminating.*

Line 420 of the resubmitted manuscript gives the logic of Yamauchi and Takei, 2020, which is the basis of the responses to the reviewer.