Reply of the author on behalf of co-authors to anonymous referee #1 comments.

We really appreciate your detailed technical and syntax comments on our study. They will help to improve the manuscript of our research. Better description of the method and additional details will contribute to completeness of information and clarity. Changes in Figures will highlight the results. The inclusion of proposed comments related to the limits of the method and the results will enrich the manuscript.

We have written each reply followed by the re-phrased text (between quotation marks) just below to each comment (written in bold).

Anonymous Referee #1

Received and published: 24 November 2014

Review of A 3-D shear velocity model of the southern North America and the Caribbean Plates from ambient noise and earthquake tomography by B. Gaite, A Villasenõr, A. Iglesias, M. Herraiz, and I. Jiménez-Munt; Solid Earth Discussions (se-2014-90).

General Comments: This article by Gaite et al describes the construction of a vertically polarized 3D shear velocity model of the crust and upper-most mantle beneath southern North America and Caribbean plates. The model is constrained by a dataset of fundamental mode Rayleigh-wave group and phase velocity measurements derived from both ambient noise and earthquakes; the ambient noise measurements were obtained from a previous study performed by the authors. The main features of the model are interpreted in terms of their tectonic significance, and are compared with previous results and interpretations. Some of the striking features of the model include the low crustal velocities along the USA-Mexico Border, as well as the distinct elevated mantle velocity structure of the Yucatan Block and Isthmus of Tehuantepec from the surrounding lower velocities.

The article is well-structured overall and fairly-well written. The clarity of some of the arguments and interpretations would benefit from additional editing for grammar and syntax. I have suggested some specific changes, as well as highlighted some confusing or unclear sentences in the Technical Comments below. The figures are nicely presented and well-constructed. In Figures 9 and 11, the horizontal slices through the model, it would be helpful to make each of the panels larger. This would allow the font size of the labels to be increased, as currently they are quite hard to read. I'd also like to commend the authors for

making their model available for download, as this dissemination is very helpful for the rest of the community.

Following are a number of specific comments which I would like to see addressed.

Specific Comments:

- Please comment on the effects of neglecting azimuthal anisotropy in the inversions?

Azimuthal anisotropy plays an important role understanding kinematic and dynamic within the Earth because it might be caused by different effects as crack alignments in the crust or lattice-preferred orientation of olivine in the mantle. Different regional studies of Rayleigh wave phase velocity suggest azimuthal anisotropy with amplitudes around 2% for periods from 12 to 100 s, sensitive to the crust and uppermost-mantle (e.g., Yang et al., 2006; Lin et al., 2011; Ikeda and Tsuji, 2014). We obtain 8% and 15% maximum phase and group velocity anomalies, respectively, however we also interpret lesser velocity anomalies.

Considering the azimuthal anisotropy we should obtain information about the tectonic evolution of the area (e.g., insights on the nature of continental blocks as Yucatan and Chortis, mantle flow, mantle wedge or asthenospheric upwelling). If doing this, we could extent the discussion of the results to some characteristics of the dynamic evolution of North American plate and the Caribbean not well determined yet.

However, in this research we describe the lithosphere of this complex region with a simple 3 layer and a half-space shear wave velocity model that fits group and phase velocities of the fundamental mode vertical-polarized Rayleigh-waves. We would obtain kinematic and dynamic information of the region computing the azimuthal anisotropy in well path covered areas, but is out of the scope of this research.

Ikedam T., and Tsuji, T.: Azimuthal anisotropy of Rayleigh waves in the crust in southern Tohoku area, Japan. J. of Geophys. Res., 119, 1-12, doi: 10.1002/2014JB011567, 2014.

Lin, F.C., Ritzwoller, M.H., Yang, Y., Moschetti, M.P. & Fouch, M.J.,.Complex and variable crustal and uppermost mantle seismic anisotropy in the western United States. Nat. Geosci., 4, 55–61, 2011.

Yang, Y., and D. W. Forsyth, Rayleigh wave phase velocities, small-scale convection, and azimuthal anisotropy beneath southern California,J. Geophys. Res.,111, B07306, doi:10.1029/2005JB004180, 2006.

To mark that our velocity model comes from vertically polarized Rayleigh waves, we have re-phrased the introduction (page 2972 lines 16-17) and the first sentence of the conclusions as follows:

“The final objective is to obtain a crust and uppermost-mantle vertically polarized shear-wave velocity model to image the area as a whole.”

“We invert group and phase velocities of fundamental mode Rayleigh waves to obtain a vertically polarized 3D shear-wave velocity model (3DVSAM) of the crust and uppermost mantle of Mexico, Gulf of Mexico and the Caribbean plate.”

Also, to comment on the information that could be obtained including the azimuthal anisotropy in the inversion, we have added the sentence in results and discussion section (page 2978 line 18):

“Deeper insights on the kinematic and dynamic within this region might be obtained from azimuthal anisotropy. It is out of the scope, but a natural extension of this research.”

- Pg 2973, Line 4: (Shapiro and Ritzwoller, 2002; Pasyanos et al 2013). The CUB model is an excellent model, however there are many other global models which would be considered more “recent” which attain similar resolutions. Consider including one or two other additional references (such as SEMum2, DR2012, SL2013sv, SAVANI, etc.), or alternatively remove the word recent.

We have included some additional recent references:

“Schaeffer and Levedev, 2013; Auer et al., 2014”

And we have included them in the reference list:

Auer, L., Boschi, L., Becker, T.W., Nissen.Meyer, T., and Giardini, D.: Savani: A variable resolution whole-mantle model of anisotropic shear velocity variations based on multiple data sets, J. Geophys. Res., 119, 3006-3034, doi: 10.1002/2013JB010773, 2014.

Schaeffer, A. J. and S. Lebedev, Global shear-speed structure of the upper mantle and transition zone. Geophys. J. Int., 194 (1), 417-449, doi:10.1093/gji/ggt095, 2013.

- Pg 2974, Lines 16-25: Please be more specific when discussing the inversion parameters (α,β,σ). If they are going to be given values, a short description of what each is should be included (ie one sentence total). Also, please clarify what is meant by “with less smoothed

damping parameters" when referring to the difference between the two inversions. The smoothing is probably not applied to the damping parameters, but rather to the model. Given the numeric values of the parameters, is the regularization being increased or decreased between the two inversions? The value of α (data misfit damping) is reduced by a factor of 2; the value of σ (Gaussian sensitivity kernel smoothing parameter) is increased from 400 to 500; the value of β(smoothness of the model) is unchanged. To me this suggests the smoothing length is larger in the second inversion compared to the first, therefore the resolution is lower?

We completely agree with the referee comment relative to the smoothing length of the second inversion. In this study, we've considered to increase the spatial smoothing width on the second inversion based on the velocity distribution results (good data fit, stability of the features of the computed models and small model roughness) that we obtained from the inversion. Because of that and, to explain the meaning of the damping parameters of the inversion, I've rewritten the paragraphs as follows:

"This inversion procedure tries to minimize a penalty function (Eq. 15 of Barmin et al., 2001) that depends on three damping parameters. These parameters are: α the data misfit damping, σ the width of the Gaussian Kernel and β the penalty parameter to low path density regions. We perform a large number of inversions varying the value of the damping parameters. We test α values from 650 to 2000 combined with different values of σ (from 100 to 500) and β (from 1 to 100). The final values used are selected as a compromise between good data fit, stability of the features of the computed models and small model roughness. We follow a two-step tomographic inversion similar as the one described in Gaite et al. (2012). At each step we select the damping parameters. In the first step, we invert all the dispersion curves to obtain dispersion maps with damping values α=2000, σ =400 and β =1. In the second one, we remove outliers and re-invert the remaining data, in this case with α=1000, σ =500 and β =1."

- Pg 2975, Equation 1. The symbol δU is used, however it is called a "travel-time" residual. Is this the group velocity (U) residual?

It was a typo in the equation 1. It should be δt instead of δU, referring to the travel time residual. We have changed the Equation 1 as follows:

$$\delta t > 3(SD)$$

- The maximum spatial resolution of the model is said to be 2◦, and limited by the distance between the nodes of the grid. If the computed resolution is limited by the grid, why not decrease the grid-spacing further to take full advantage of the potential resolving power?

As we use "fat rays" on the surface wave velocity inversion with a sensitivity kernel width of 500 km (sigma parameter), we consider that choosing 2º as the distance between nodes is an appropriate parametrization.

- Although the ANT method is well-documented in previous papers (ie Gaite et al, 2012), it would help to have a short description of the procedure (a paragraph or in section 3.2).

As suggested and to better document the data and methodology used, we have added a short description of the ANT procedure.

"To compute ANT we used 2 years and a half of continuous vertical component seismic records from the same stations used in this study. Firstly, we computed 1-day long ambient noise cross-correlations between each station pair and stacked them along their available time period. Secondly, we measured phase and group velocity of the fundamental mode Rayleigh wave. Finally, we inverted the dispersion curves to get phase and group velocity maps with the same method used for earthquake records on this study. The path coverage at periods shorter than 20 s is mostly limited to mainland North America that is well covered from 10s."

-Figure 7 and description on Pg 2977, Lines 19-21. The figures are described to show misfits, but they are labelled as L2 Norms. I would suggest changing the labeling to reflect the fact that they are misfit norm values.

We have changed "L2 norm" by "misfit" in the Figure 7 and its caption.

- Figure 8 and Section 4. In Figure 4a, the black lines denote the best-fitting model. However, few of the models fit the data at periods >=40s for Phase velocity and >80s for Group velocity. Can you comment on the effect this has in the inversion, and why none of the models seem to fit these data. Are the data at longer periods (relatively speaking for phase and group velocity) wrong, or is the 3-layer 4-parameter model too simplified to be able to fit those data?

Inter-event or inter-station distances control the maximum period where is possible to measure group or phase velocities. Since our data set is irregular respect to the inter-event and/or inter-station distances is expected that the uncertainties for larger periods increase. On the other hand is interesting to note that the joint inversion of phase and group velocities don't fit for relatively highest periods. And since we are using a global search algorithm we would expect that the solutions space, with the used parametrization, is well explored. This suggests that the parametrization is not enough detailed to explain differences. One of the strongest assumption, in our inverse scheme, is that P-wave velocity (α) is directly related ($\alpha = \sqrt{3}\beta$) to S-wave velocity (β). We could invert, individually, S and P-wave velocities to explore differences in Poisson ratios (μ) but with an increased non-uniqueness cost. Since the uncertainties for larger periods, we prefer to fix μ, even when the fits are not so good for larger periods.

- How is the "best-fit" model determined (solid black line)? Please specifically state how this is computed, as in the text the misfit criteria are stated but not what constitutes the final best-fit profile. Do you compute the average of each parameter from the ensemble of accepted models (grey lines)? Also, plotting the ensemble of solutions (grey curves) instead as a density of sampling would be more informative to show where the most likely solution lies in the tested model space. This could be done as a 2D histogram, or alternatively, each curve could be coloured by their misfit. The best-fitting (black) curve would be expected to follow roughly the maximum.

We have added the following text in page 2977 line 13 to state how the best-fitting model is computed:

"Simulated annealing is a global optimization method. The algorithm scans the possible solutions space to find the optimum model by reducing the searching vector length when it is close to a minimum and allowing misfit increases to avoid local minimums. The algorithm determines as optimum model the one that minimizes the misfit during a certain number of searching iterations."

In order to become Figure 8 more informative, we have colored each curve by their misfit. As expected, the best fitting (black) curve follows roughly the space of search of the models with the lowest misfit. The optimum model is not computed from the average of each parameter from the ensemble of accepted models, but from the simulated annealing algorithm. This explains why almost all the accepted models have larger misfits than the optimum model.

- Related to the above point, it is stated that tests show the Vs model is sensitive to 5-km-thick layers. This is quite a high vertical resolution for surface waves, even with the short periods obtained from ANT. The results of the tests that demonstrate this resolution should be included in the paper. There would be strong trade-offs between the layer depths and velocities, such that a variety of depths for a given interface can have equal misfit simply by adjusting the velocities in the surrounding layers. Intuitively this would have a serious effect on the resolvable sharpness of the 3 vertical discontinuities that make up the model at each node.

We based the affirmation that the inversion resolution is to 5 km thick layers on tests with shallow layers. We observed that a 5-km thick layer depth was solved by the inversion, but three 2-km-thick layers into the first 6 km where not solved. This test was useful to check the limits of the inversion and its sensitiveness to the models obtained in areas with extended crust, like, for example, from the Gulf of California with a strong velocity contrast at shallow depths (e.g., ~8 km at 103ºW23ºN).

In order to answer the requirement of the referees and to confirm the resolution of the inversion We have computed new tests in other cases (e.g., to solve internal layers with different thickness). Figures 1 and 2 show these tests to check the sensitivity of our inversion to 1st layer thickness (Figure 1) and to the 2nd layer thickness (Figure 2). Tests in Figure 1 show that we are able to retrieve the depth of the first layer for layers from 2 to 10 km-thick, however, there is a trade-off with velocities and other layer depths. In the case of sensitiveness to the 2nd layer thickness (Figure 2) the trade-off between velocities and depths is clear and we do not recover the thickness and the velocity from the second layer. With these new tests I cannot state that our inversion has a resolution to 5-km-thick layers, but only that the inversion is sensitive to 5 km depth velocity contrasts.

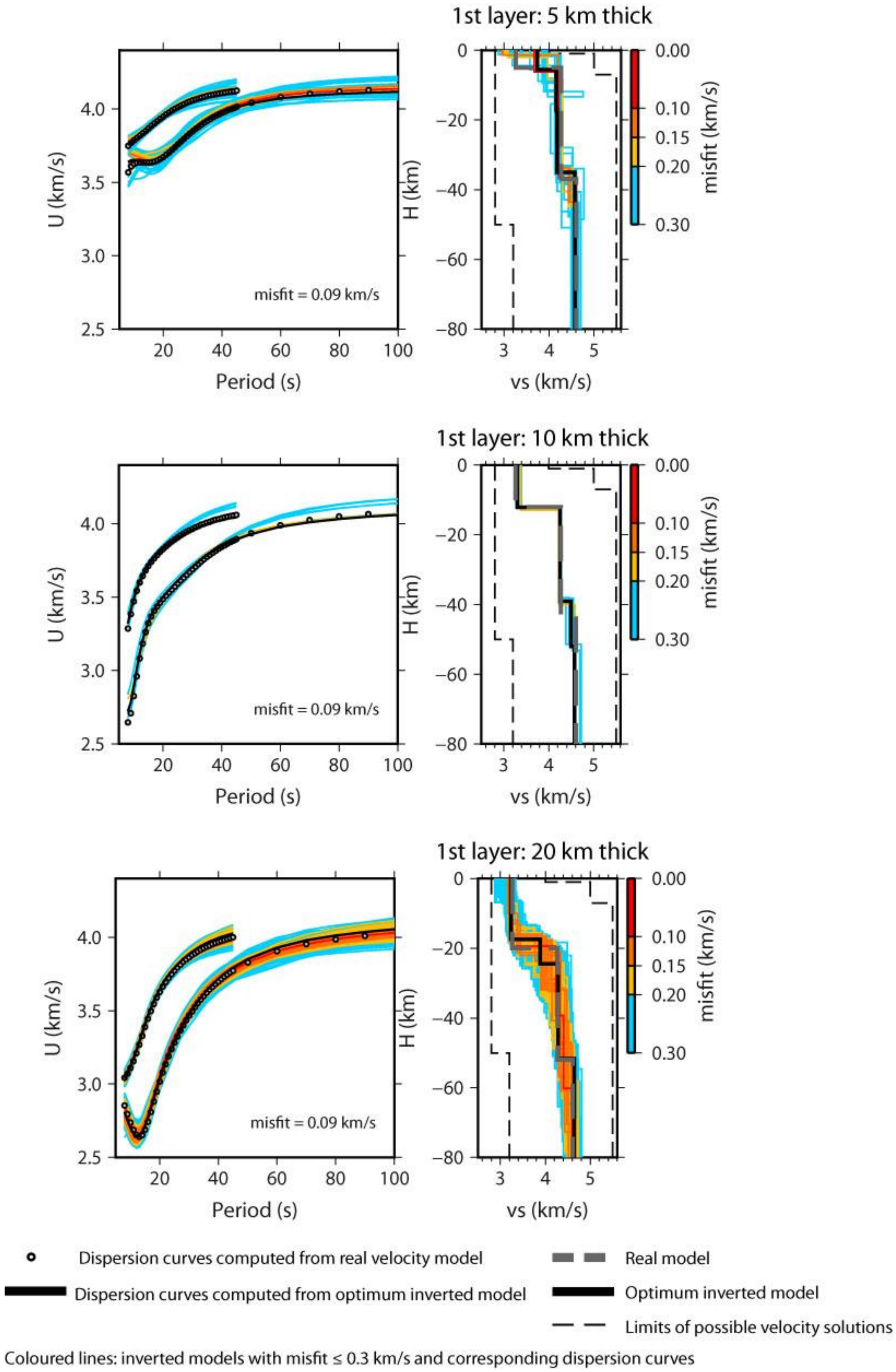

Figure 1: Tests of inversion sensitivity to the shallowest layer thickness.

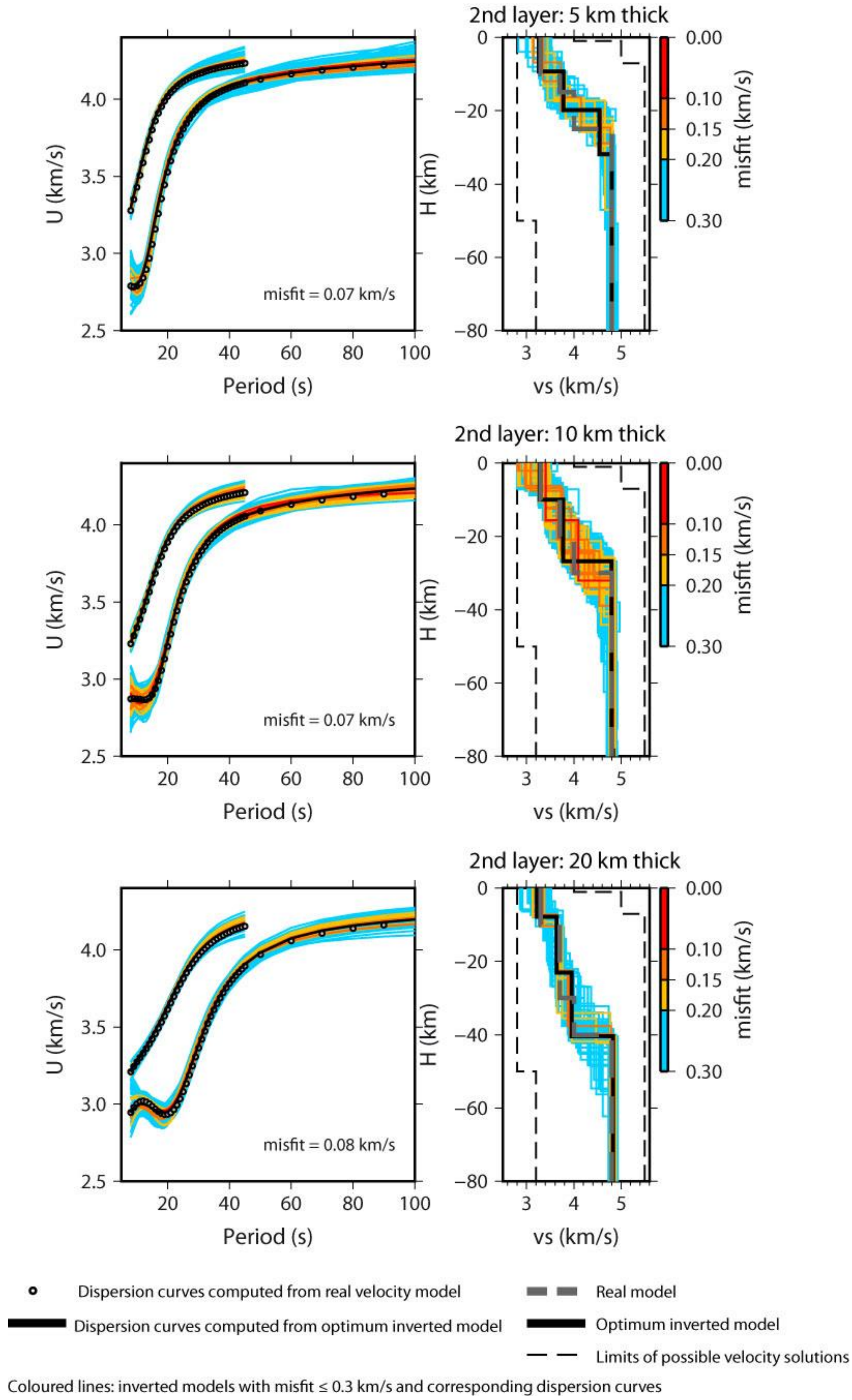

Figure 2: Tests of inversion sensitivity to the second layer thickness.

Considering these tests we have removed the affirmation (page 2978 line 13): "Inversion tests show that our v_S model is sensitive to 5 km thick layers."

And we have re-phrased the text (page 2980 line 22-24) as follows:

"Crustal thickness differences under SMOc and SMOr between the results of this study and previous studies are within the range of our vertical resolution."

- The high velocities at 12km are interpreted to represent the crustal signature of the Laurentian margin (Page 2979, Paragraph 2). If these features delimit the southern extent of the Laurentian margin, should these high Laurentian velocities not also persist further northwards into Laurentia? What could be the explanation for their absence? Is it possible the data coverage is not high enough to capture the high velocities which may extend further north?

Our path coverage is not dense enough in this region to image if Laurentian high velocities will persist further northwards. This region is close to the ANT 250 km resolution contour at 10 s period (Figure 9), that is sensitive to upper crust.

- In discussing the upper mantle (section 4.2), several references are made to specific velocity contours. It would be helpful for the reader to follow the exact boundaries you are discussing by including the actual contour lines superimposed on the figure (Figure 11b for instance).

We have drawn the 4.3 km velocity contour in Figure 11b to help the reader to follow the description of section 4.2.

Technical Corrections:

- Pg 2973, Lines 1-2: "...stations deployments in the last decade facilitates getting a denser path coverage." Reads awkwardly, consider changing to "...station deployments in the last decade has facilitated a higher path density."

It has been changed.

- Pg 2973, Line 4: "...2 or 1◦of resolution..."; remove "of"

It has been changed.

- Pg 2973, Line 27: "coverage on California"; chance to "coverage in California"

It has been changed.

- Pg 2973, Line 1: "has recently improved significantly the station coverage in the Caribbean region."; change to "has significantly improved the station coverage in the Caribbean."

It has been changed.

- Pg 2974, Line 24: "...and invert again the remaining..."; change to "...and re-invert the remaining... "

It has been changed.

- Pg 2975, Line 7-8: "...are well covered for all period range, while the east of the Caribbean for periods longer than 20s." First change "for all period range" to "across all periods" and "...while the east..." to "...whereas the east..." Finally, the sentence is incomplete. Do you mean the coverage east of the Caribbean plate at periods longer than 20s is poor?

I've re-phrased the sentence as follows:

Mexico, GOM and the western part of the Caribbean plate are well covered across all periods, whereas the eastern part of the Caribbean plate is well covered for periods longer than 20 s.

- Pg 2975, Line 10-12: "The tomography method used...sensitivity of the surface waves." Please re-phrase, awkward.

I've re-phrased the paragraph as follows:

"The tomographic inversion used is similar to a Gaussian beam method and considers propagation of 'fat' rays along the great circle. Following this, the frequency-dependent spatial sensitivity of the surface waves is described by Gaussian lateral sensitivity kernels."

- Pg 2975, Line 17: "...lower or equal than..."; change to "...lesser than or equal to..."

It has been changed.

- Pg 2976, Line 2: "...from 8 to 100s of period."; remove "of"

It has been changed.

- Pg 2976, Lines 11 to 18. Awkward wording in these sentences. Please re-phrase to

make it more clear to the reader.

"The averaged difference between velocities obtained from ANT and from earthquakes varies from 0.09% to 1% in their common range of period (from 20 to 50 s) (Fig. 6). This upper limit is a bit larger than in other studies (~ 0.1-0.5%) that compare phase velocity measurements (e.g., Lin et al., 2008; Yang and Ritzwoller, 2008; Yao et al., 2008; Ritzwoller et al., 2011;

Zhou et al., 2012). Our larger difference might be due in part to the fact that we compare group instead of phase velocities."

- Pg 2976, Line 20: "We invert simultaneously..."; change to "We simultaneously invert..."

It has been changed.

- Pg. 2976, Line 23: "...can vary in a wide range..."; change to "...can vary across a wide range..."

It has been changed.

- Pg 2977, Line 1: "...of tectonic domains on the..."; change to "...of tectonic domains in the..."

It has been changed.

- Pg 2977, Line 20: "Figure 7b shows the misfit geographical distribution." Change to "Figure 7b shows the geographical distribution of the model misfit."

It has been changed.

- Pg 2978, Lines 14-16: Awkward, re-phrase.

We have re-phrase the text as follows:

"Its agreement with the main known tectonic characteristics and the recovery of the major crustal features obtained in previous local studies provides reliability on our results while confidence to interpret them on regions with a lack of shear-wave lithospheric information."

- Pg 2978, Lines 24-26: Unclear, please re-phrase to clarify. Really only one of the depths shows the oceanic crust (5km), and none of the profiles extends far off-shore.

In agreement to the comment, we have re-phrased the sentence and eliminated the part relative to the off-shore model results, the model do not extent so far. The new sentence is as follows:

"The model also exhibits a high contrast between upper and lower crustal velocities inland North America plate (Fig. 10)."

- Pg 2980, Line 2: Typo in Basin and Range.

Typo corrected.

- Pg 2980, Line 7: "-GofC-."; change to "(GofC)."

We have changed “-GofC-“ by “(GofC)”.

We have also changed “-GEP-” by “(GEP)” following the same format proposed in page 2980 line 4.

- Pg 2981, Lines 15-19: Please re-phrase to help clarify the point being made.

We have re-phrased the text as follows:

“The thin crust observed in this area is an evidence of an extension process, coherent with the proposed Jalisco Block rifting from the North American plate (Luhr et al., 1985; Allan et al., 1991). Another worth noting feature is that our results highlight a different crustal seismic structure between the U.S. and Mexican Basin and Range provinces.”

- Pg 2982, Lines 22-25: I may have missed it, but I couldn’t see the Veracruz Basin labelled on the figures.

Veracruz Basin was not labeled on the figures so I have labeled it in Figure 9c as “VeB” and added “VeB Veracruz Basin;” to Figure 9 caption.

Besides, I have found a typo and I have changed the text “Fig. 9d” by “Fig. 9c” (Pag. 2982, Line 25).

- Figure 6: The grey contours marking the inversion area are almost impossible to see with the grey colours in the background. Perhaps it could instead be a white line outlined by black (visible on essentially any colour).

The contour line has been replaced by a white line outlined by black. The Figure 6 caption text has been rewritten as follows:

“The white line outlined by black marks the ANT inversion area.”

- In Figure 8b, the light grey lines denote the acceptable models “smaller than or equal to 2 times the smallest fitting.” Please change the wording to clarify what this means.

We have re-phrased the text as follows:

“the colored lines are the models with misfits lesser than or equal to two times the best fitting (0.11 km/s);”

- Figure 9: This is a very nice figure, though its impact could be made greater if it was larger. This would make it much easier to follow the discussion in the text. The labels are quite hard to read; please increase their size (+/- change colour) to make them more clear.

We have increased the size of maps in Figure 9 to make labels readable and provide better display of the results.

Interactive comment on Solid Earth Discuss., 6, 2971, 2014.