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Solid Earth
Discussions

Interactive comment on "Lithospheric and sub-lithospheric deformation under the Borborema Province of NE Brazil from receiver function harmonic stripping" *by* Gaelle Lamarque and Jordi Julià

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In order to make answers as clear as possible, we copy in black the reviewer comments, and answer with blue color. Proposed changes in the original text are indicated in red.

Anonymous Referee 2

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The manuscript under review presents a detailed accounting of lithospheric anisotropy through the use of Ps receiver function analysis and data collected at 75 seismic stations within the Borborema Province of NE Brazil. The importance of their analysis rests in the fact that they can provide firm constraints on anisotropic boundary depth, in contrast to shear wave splitting which is a path integrated measurement. Their results show a clear correlation between tectonic deformation and orientation of seismic anisotropy. Within the continent, they find that the orientation of anisotropy is coincident with the orientation of large-scale shear zones thought to be associated with the Brasiliano-Pan African Orogeny. On the coast, anisotropy is oriented perpendicular to the coastline, suggesting that rifting is the process responsible for generating anisotropy. In places where anisotropy is absence, it is inferred that heating by the asthenosphere may have destroyed any preexisting lithospheric fabric.

Comments regarding methodology: Overall, the methodology is thoroughly and carefully described, and proper citations were given. My only question is regarding the cut-off for the minimum number of bins with data (lines 24-26). The authors require a minimum of 9 bins with data (90 degrees), which can be either continuous or discontinuous. Why was this minimum chosen? Is there an appreciable difference in how well the harmonic decomposition works? Do the authors have synthetic example they could show to demonstrate their reasoning? The reason I ask is because this seems to be the primary reason for reducing the number of stations from 75 to 39.

The reason for selecting stations that display data in at least 9 bins (10 degrees wide) is purely geometrical. Recall that we use receiver functions to map anisotropy with either 2-lobed (plunging fast axis of symmetry) or 4-lobed (horizontal fast axis of symmetry) back-azimuthal patterns. In the case of a plunging fast axis of symmetry, 9 bins corresponds to half the period for a 2-lobed pattern (90 degrees); in the case

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of a horizontal fast axis of symmetry, 9 bins corresponds to a full period for a 4-lobed pattern (90 degrees). By requiring 9 bin coverage (10 degree wide), we are able to reliably display either a 2-lobed or a 4-lobed pattern.

We propose to modify the text in the manuscript as:

"Next, the migrated radial and transverse receiver functions for each station were grouped by back-azimuth in 36 non-overlapping, 10° wide bins, and averaged within each bin. A given station was then selected if it presented at least two averaged receiver functions (one radial and one tangential) in at least 9 bins. This selection criterion ensured a sampling of at least 90° in back-azimuth, either continuously or discontinuously, around the station.A back-azimuthal coverage from at least 9 bins (each 10° wide) allows the mapping of either half the period for a 2-lobed pattern (anisotropy with plunging fast axis of symmetry) or a full period for a 4-lobed pattern (anisotropy with horizontal fast axis of symmetry).A total of 39 stations were thus selected for anisotropy analysis. An example of stacked and migrated receiver functions is displayed in Figure 4."

Comments regarding results: I appreciated the inclusion of the harmonically decomposed results within the supplementary materials. They clearly exhibit evidence of anisotropy. I did however wonder how the authors dealt with cases where more than one anisotropic boundary was present within either the crust or the mantle. I may have missed where they spoke to this, but could not find it upon reexamining the manuscript. A clearer description would have been greatly appreciated.

Our goal in this paper is to examine the direction of the dominant anisotropy within two depth windows, which correspond to the crust and the lithospheric mantle. We make the assumption that in the case of several anisotropic layers, the layer with the strongest anisotropy will dominate the results. We are aware that results can reflect the average value from different anisotropic layers, or from different types of anisotropy in the case of similar anisotropic strength.

We propose to modify the text in the manuscript as:

4. Results - Anisotropy parameters were examined for each station at two depth-window ranges: (1) crust (Figure 6A), which was assumed to be located between 0 and 33 km depth, in agreement with the 32-40 km range estimated by Luz et al. (2015b) under the Borborema Plateau and 30-33 km under the surrounding basins; and (2) lithospheric mantle, which was taken to be between 33 and 100 km depth (Figure 6B).We assume that the layer with the strongest anisotropy will dominate the results in the case of several anisotropic layers. However, it might happen that results reflect the average value from different anisotropic layers, or from different types of anisotropy in the case of similar anisotropic strength.All results are indicated in Table 1. An inspection of Figure 6A reveals that the crust of northeast Brazil ..."

Comments regarding interpretation: My only significant concern with the manuscript was that while regional patterns of deformation matched the fast direction, it was not always clear to me that the material properties would necessitate such an answer. For example, while the LPO of olivine typically means that the A-axis of olivine is oriented in the same direction as strain, the crust is significantly more complex, as several candidate minerals can generate different types of anisotropy, in addition to the possibility of shape preferred orientation of different materials. I would encourage the authors to think more carefully about crustal anisotropy in particular.

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We agree with this remark. A complex combination of LPO and SPO could be present in the mantle, although LPO is likely to dominate (Nicolas and Christensen, 1987; Silver 1996; Mainprice et al., 2000); fractures and cracks or fine layering, could additionally contribute in the crust. For that reason, our interpretations focus dominantly on mantle anisotropy, consistency of anisotropy within the lithosphere (crust and mantle), and regional-scale trends. And, to avoid a bias related to local features, we refrain from interpreting small-scale variations in anisotropy within the crust.

Comments regarding figures: Figure 6: It would be useful if the names of the stations were more clearly written as they appear washed out and are difficult to read.
Done.

References : * Mainprice D., Barruol G.. Ben Ismaïl W.. Karato S.-I., Forte A., Liebermann R., Masters G., Stixrude L.. The seismic anisotropy of the Earth's mantle: from single crystal to polycrystal, Earth's Deep Interior: Mineral Physics and Tomography From the Atomic to the Global Scale, 2000 American Geophysical Union doi:10.1029/GM117p0237 * Nicolas A., Christensen N.I.. Froidevaux K., Fuchs C.. Formation of anisotropy in upper mantle peridotites: A review, Composition, Structure and Dynamics of the Lithosphere, 1987 Asthenosphere System (pg. 111-123) * Silver P.G.. Seismic anisotropy beneath the continents: probing the depths of geology, Annu. Rev. Earth planet. Sci., 1996, vol. 24 (p. 385-432)