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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

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

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Discussion paper

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.

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.

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.

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.

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