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Interactive Comment

Interactive comment on "Petrophysical constraints on the seismic properties of the Kaapvaal craton mantle root" *by* V. Baptiste and A. Tommasi

Anonymous Referee #1

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The manuscript "Petrophysical constraints on the seismic properties of the Kaapvaal craton mantle root" by Baptiste et al., presents an interesting and rather exhaustive experimental study of a xenolith suite from the Kaapvaal craton. The work complements a previous analysis of the same xenolith suite adding the "seismic" perspective. The work is relevant and worth being published although some modifications/discussion of the results are needed before.

Specific comments:

Introduction

Fig. 1 is a bit unclear. The figure should include at least some geographical coordinates

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or an inset to help the reader. Besides, a topography map could be rather useful to get an idea about the location of the xenoliths

Method.

Some attention is paid to the parameters that characterize olivine anisotropy. This is reasonable, although pretty much the same information can be gathered from the previous paper by Baptiste et al. (2012). In contrast, relatively few details are provided regarding what is specifically new in this work: the estimation of the seismic properties. In particular, as the results of this work are compared to geophysical models, it would be interesting to have an estimate of the errors associated with the T, P derivatives of the elastic properties, VRH average etc. How would an uncertainty in the T estimation of, say 100 K propagate into the derived seismic velocities? In a word, what is the estimated uncertainty in the measurements provided by the authors?

In equation 1 (BA index) there is no description of what the symbols P010, G100 etc. stand for.

Results.

The discussion turns somewhat complicated to follow with the different axis and planes. A figure describing the geometrical setting would be quite useful.

What is the relationship between S-wave polarization anisotropy and S1 and S2 propagation anisotropies? What would be the connection with seismic azimuthal and radial anisotropies?

"Yet, in agreement with previous studies (Ben Ismail and Mainprice, 1998), this variation is not linear: peridotites with J index>4 tend to display a weak 5 variation of the maximum anisotropy values. Coarse-grained peridotites show more variable olivine CPO intensities (J indexes range between 2–11), but their maximum seismic anisotropies are in the same range as those displayed by the sheared peridotites (Fig. 3a, b). This suggests that the modal composition has also an important effect on the

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seismic anisotropy of these samples." This sentence is not clear.

How is the isotropic S-wave velocity determined?

There are a considerable number of vertical and horizontal "alignments" in Fig. 4. What is the reason for that, could it be an artifact in the determination of the petrophysical properties?

Fig. 5 and its discussion throughout all the text. It is repeatedly stated that variation or dispersion of Vp (and rho) is always greater than that of Vs. However, according to the relative values given by the authors, the dispersion of Vp, Vs and rho are 2.3, 3 and 1.9 % respectively. So in fact, Vs shows the greatest relative variation among the three parameters under study. Looking at Fig. 5 a one would be tempted to interpret a bi-modal distribution of Vp values with similar linear slopes but shifted intercepts (at least for the coarse-grained peridotites). A cluster analysis would be helpful here probably. Besides, the ranges of Vp, Vs and rho absolute values in Fig. 4 and Fig. 5 are rather different, what is the reason for this difference? For instance, the dispersion of rho in Fig. 4 is 3 % whereas in Fig. 5 this value is only 1.9 %. Therefore, it is not clear at all if the variation of Vp or rho are "strong" compared to Vs. In addition, if that was the case, what would be the explanation for it? Would it be an issue with the computation of the bulk modulus (as the shear modulus and rho dependency is common to Vp and Vs)?

"At first order, the change from a normal "normal"100 km-thick lithosphere to a cratonic geotherm increases Vp and Vs by up to 2.8 and 3.1 %, respectively. This variation is on the same order of the one resulting from compositionalheterogeneity among the Kaapvaal xenoliths (Figs. 4 and 5)." This is not, however, what Fig. 5 shows: the intervals between the cratonic and 100-km-thick crust are always quite larger than the dispersion shown by the data, particularly for Vs.

"Comparison of the velocity profiles in Fig. 5 with one-dimensional P wave velocity profiles for the Kaapvaal highlights that most P wave models show an increase of velocity with depth between 50 and 200 km depth, consistent with James et al. (2004)

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data, but which we do not observe (Fig. 5a).” Not entirely clear from the figure. The data seem to have a an increase with depth, particularly if we consider a bimodal behaviour. In any case, the statement is too speculative.

“Between 70 and 90 km, P waves velocities estimated for our xenoliths are higher than those in most seismic models. However, these depths are not well constrained in the present study because of the small number of xenoliths analyzed (2).” Could it also be related to the spinel elastic model chosen?

“If we consider a 150 km-thick homogeneous anisotropic mantle lithosphere, which is consistent with 190 km-thick lithosphere inferred from the geotherm of Baptiste et al. (2012),” Then why not just using 190 km instead?

To help the discussion Fig 7 could be completed with a table describing how the different cases match SKS and/or surface-wave data.

“contrast. In addition, Peslier et al. (2010) and Baptiste et al. (2012) did measure a marked decrease in OH concentrations in olivine at depths greater than 160 km. Yet the resulting change in elastic properties is probably too weak to explain the receiver function signal.” The second sentence is too speculative. If the authors think so, they should offer some (quantitative) arguments for it.

“A change in the orientation of the foliation and lineation might also produce an impedance contrast, but Rayleigh waves azimuthal anisotropy does not show significant variation of the fast direction within the mantle lithosphere (Adam and Lebedev, 2012).” True, but this is likely because surface-wave data are not sensitive to that, and therefore the lithosphere is imaged nearly as a single block. For instance, Rayleigh waves at periods of 80-120 s are mostly sensitive to the depth range 100-200 km (according to the maximum in he corresponding kernel). So from that perspective, surface-waves could be averaging the hypothetical discontinuity imaged by receiver functions. What perhaps could be interesting for the discussion is to check if the authors see any trend in their samples in terms of the in situ rock anisotropy (considering, of course, that the effects of foliation and lineation, which are unknown, would be superimposed in the total, seismically measurable anisotropy). For instance, in their Fig. 6 there seems to be a change at around 140 km in the S1 velocity from something random-z axis aligned to something x axis aligned. Could this be relevant?

Conclusions.

"Vp does not show a clear trend; it is highly variable at all depths, probably reflecting a greater sensitivity to modal composition changes" This should be tested quantitatively (see my comment above). The explanation given is clearly insufficient.

"Models considering end-member orientations of the foliation and lineation in the subcratonic mantle lithosphere show that the simplest model that might produce both the coherent fast directions over large domains, but low delay times imaged by SKS studies, and the low azimuthal surfacewaves anisotropy with SH faster than SV in the subcratonic mantle lithosphere is the presence of 45 dipping foliations and lineations.Horizontal or vertical lineations both fail to explain the observed seismic anisotropy." However, one ends up with the feeling that neither of the presented cases (in Fig. 7) is able to match, at the same time, SKS and surface wave observations after reading the corresponding discussion section. What would be the geodynamic interpretation of the authors of such an oblique fabric in the Kaapvaal lithosphere?

Interactive comment on Solid Earth Discuss., 5, 963, 2013.