

Response to Reviewer Comments

Nitarani Bishoyi¹, Arun Kumar Dubey¹, Ashwani Kant Tiwari¹

¹ Department of Earth and Environmental Sciences, Indian Institute of Science
Education and Research, Bhopal - 462066, India

Correspondence: Ashwani Kant Tiwari (aktiwari@iiserb.ac.in)

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Dear Editor,

We appreciate your constructive engagement and the opportunity to submit a revised version of our manuscript (MS No: **egusphere-2025-6202**). We have carefully revised the manuscript in response to the comments from Reviewer #2.

Below we provide the detailed response to all four comments and outline the changes made to both the manuscript and supplementary information.

We sincerely hope that the revised manuscript is now suitable for publication in Solid Earth.

Sincerely,

Nitarani Bishoyi and co-authors

Response to Reviewer #2

Comment 1: One issue that remains concerns the two methods described for measuring anisotropy. The authors state that the results are based primarily on the second method, but it is unclear whether all reported results are derived from this method or whether both methods were used for different measurements. If both methods were used, which measurements correspond to each method, and what was the rationale for selecting one over the other? Typically, when two methods are applied, their results are compared and measurements are retained when they agree within uncertainties. If the authors instead preferentially select one method for individual measurements, the rationale should be stated. This could be clarified by indicating in the table which method was used for each measurement and why, or by describing only the method ultimately used to obtain the reported results.

Reply: Thank you for highlighting this point. We would like to clarify that two methods were evaluated during the analysis; however, the final set of reported SK(S)S splitting parameters was consistently obtained using the Transverse Energy Minimization (TEM) method. We did not choose between the two methods based on individual events or stations. We have revised the method section and Supplementary Table 1 in the revised manuscript for better clarity.

The RC method provides an independent estimate of the fast polarization direction (FPD) and delay time (δt), whereas the TEM method derives these parameters from the minimum of the transverse-energy surface. The output from the TEM methods includes the lower confidence limit, best-fitting value, and upper confidence limit for both the fast polarization direction and delay time. These confidence limits were used to assess how well the splitting parameters were constrained.

Additionally, we considered the consistency of the TEM solutions with their corresponding RC estimates, together with the waveform quality and the stability of the transverse-energy minimum. This approach allowed us to distinguish well-constrained splitting solutions from those with greater uncertainty. The final reported FPD and δt values were consistently taken from the TEM solutions, ensuring that the measurements used in the subsequent analysis were obtained using the same methodology.

Comment 2: I appreciate the authors adding the depth-dependent spatial coherency analysis based on the ray-piercing points, as this provides useful constraints on the depth and spatial sensitivity of the SKS measurements. However, I am not sure that the term “Fresnel zone analysis” accurately describes the procedure presented here, which appears to be based on ray-piercing-point distributions and variation factors at different assumed depths and block sizes. I

suggest either clarifying how Fresnel-zone geometry is explicitly incorporated into the analysis or referring to it as a “ray-piercing-point spatial coherency analysis” (or similar) to avoid confusion with a formal finite-frequency Fresnel-zone analysis.

Reply: We thank the reviewer for pointing this out. We agree that the term “Fresnel zone analysis” could be misleading, as our approach does not explicitly compute finite-frequency Fresnel-zone geometries or sensitivity kernels. Instead, we use the distribution of SK(K)S ray-piercing points at different assumed depths to examine the spatial coherence of the splitting parameters, together with different spatial block sizes and the corresponding variation factors.

We have therefore replaced the term “Fresnel zone analysis” throughout the revised manuscript with “ray-piercing-point spatial coherency analysis.”

Comment 3: L46 in the manuscript with tracked changes: “reunion” should be “Reunion.”

Reply: Thank you for pointing this out. We have corrected it in the revised manuscript.

Comment 4: Regarding my previous Comment 6, I appreciate the authors’ expanded discussion and their efforts to address the possible relationship between the observed anisotropy and Cretaceous Réunion plume activity. I would nevertheless recommend slightly more caution in interpreting the reduced SKS delay times as a direct signature of Cretaceous plume magmatism. Their depth-coherence analysis places the anisotropic signal at approximately 230 km, below the LAB, making a simple interpretation in terms of preserved fossilized lithospheric anisotropy less straightforward. In addition, the stations are located at the periphery of the Deccan Traps, where a clear spatial correspondence with the hotspot track is not evident. Plume-related upwelling could also have weakened or overprinted pre-existing lithospheric fabric, while the presence of Cretaceous dykes or magmatic rocks does not by itself establish that these structures are responsible for the present-day SKS measurements. Given these uncertainties, I suggest framing plume influence as one possible contribution to the asthenospheric anisotropy, rather than as a definitive causal explanation for the low delay times and null measurements.

Reply: We thank the reviewer for this helpful clarification and agree that our earlier wording was too strong. We have now revised the discussion section related to the observed asthenospheric anisotropy and its possible geological origins. We also agree that the present-day locations of the stations do not merely correspond spatially with the Réunion hotspot tract or

the main extent of the Deccan traps. Consequently, we discuss the Reunion plume as a possible geological process that may have modified the pre-existing mantle fabric and affected asthenospheric flow beneath the Indian plate during the Cretaceous period. This modification could involve weakening, reorientation, or overprinting of an earlier anisotropy rather than simple preservation of a fossil plume-related lithospheric fabric. Accordingly, the reduced delay times and null measurements at SG02 and SG09 are now considered as observations that may be consistent with spatially heterogeneous asthenospheric anisotropy in regions that experienced significant Cretaceous magmatism, rather than definitive signatures of Reunion plume activity.

We now write:

Lines 303-314 in the revised manuscript:

The relatively lower δt (0.74 sec) observed at SG02, together with 4 null measurements, indicates substantial spatial or azimuthal complexity in the anisotropic structure beneath the station. SG02 is located in a region affected by significant Cretaceous magmatic activity associated with the Narmada-Tapti region, where occurrences of dykes provide independent geological evidence of this magmatic history (Shukla et al., 2022). However, the presence of these crustal intrusions does not, by itself, establish their contribution to the present-day SK(K)S anisotropy. The spatial coherence analysis indicates that the primary anisotropic contribution is at approximately 200 - 250 km depth, suggesting a significant upper-mantle anisotropic contribution and making it improbable that the observed splitting is directly attributable to the shallow crustal dyke systems. SG09, exhibiting a relatively low δt (0.95 sec) along with 2 null measurements, is located close to the DVP area that experienced substantial Cretaceous-Paleogene magmatic activity associated with the Deccan volcanic episode (Krishnamurthy, 2020). Although the spatiotemporal association between this magmatic history and the present-day anisotropic structure is noteworthy, plume-related thermal and deformation processes may represent as one possible mechanism through which the pre-existing mantle fabric has been modified or overprinted, potentially contributing to the present-day heterogeneity in upper mantle anisotropy.

Lines 357-359 in the revised manuscript:

The lower splitting delay times ($\delta t < 1s$) observed at seismic stations SG02, SG06, SG07, and SG09, along with the associated null measurements, may reflect modification or overprinting of pre-existing mantle fabric due to plume-related thermal and deformation processes.