Solid Earth Discuss.,
doi:10.5194/se-2016-134-AC1, 2016

Solid Earth
Discussions

Interactive comment on “Seismic anisotropy inferred from direct S-waves derived splitting measurements and its geodynamic implications beneath southeastern Tibetan Plateau” *by* Ashwani Kant Tiwari et al.

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Received and published: 15 December 2016

Answers to reviewer #1

Reviewer #1: By what metric it is robust and coherent? (In-line comment, Page 2, line 32.)

Reply: Robustness is more related to the increased splitting observations by providing additional constraints when the direct S-waves are involved in splitting measurements, particularly in regions (e.g. Indian sub-continent) where station deployment times are too short to obtain more than a handful of SKS measurements, mostly not enough for

robust estimates of shear-wave splitting. The effect of using relatively large data sets in splitting observations on the reliability of results is now presented in the modified version of Figure 8 of the manuscript. Coherency is interpreted due to the new type station average splitting parameters with relatively less lateral variations most likely due to the averaging of comparable number of enhanced splitting observations at closely spaced stations. Now, it is clarified in the text.

Reviewer #1: Page 3, around line 10. The authors discuss a possibility of isotropic Indian plate lithosphere. This appears to come from some previous observations of null splitting, but I would be surprised if a continental lithosphere was really isotropic. More likely, the SKS splitting sampled a near vertical path where either down-welling mantle forms a coherent downward flow pattern or the existing anisotropy is aligned vertically. Direct S splitting may sample the region differently and therefore its anisotropy comes from a different raypath.

Reply: Yes, a continental lithosphere is not really isotropic. Isotropic nature seems to come from previous observations, which is based on limited SKS splitting measurements.

Reviewer #1: Page 4, line 9. Why did you choose to remove the instrument response? Is that to improve the correlations between the target and reference stations? It’s reasonable to do so, but since splitting measurements typically come from a single station, it is not commonly necessary.

Reply: We estimate splitting parameters at the target station as a result of a kind of process that compares the receiver-side anisotropy corrected horizontal components at reference and target stations in a grid-search scheme. This means we utilize two-stations and their recordings. The instrument properties for reference and target stations may not be always identical. To overcome biases related to the potential use of different stations (at reference and target sites), prior to our procedure, we have removed instrument response from both reference and target stations. It is now mentioned in the text.

Reviewer #1: Why you re-sampled the seismographs at 20 samples per second? (In-line comment, page 4, line 10)

Reply: Re-sampling the seismographs at 20 samples per second clearly constructs the waveforms in the appropriate frequency range (0.03 -0.2 Hz) with no aliasing. After your question, we have resampled the waveforms at higher rate (30 and 40 sample per second) and do the processing again but we get the similar type of the waveforms as we have obtained with 20 samples per second (Fig. 1).

Reviewer #1: What are the selection criteria you have used in forming the station pairs? (In-line comment, page 4, line 16.)

Reply: We used two main criteria when establishing station pairs: i) station pair must pose horizontal recordings available both for reference and target stations.
ii) The interstation spacing between reference and target stations should not exceed 300 km. This issue is now much better clarified in the modified text.

Reviewer #1: How does a 45 s time window starting 15 s before the theoretical onset of the direct S waves effect the coda waves and converted phases? (In-line comment, page 4, line 19.)

Reply: We used a 45 s analysis window starting from15 s prior to the theoretical S-wave onset. This has been previously suggested by Eken and Tilmann (2014) which reports that 30s-window after the S-wave onset was usually adopted since this window length would minimize the effect of coda waves and contamination by converted phases as it excludes crustal S multiples in the thick Tibetan crust as an undesired influence. It is now mentioned in the text.

Reviewer #1: Page 4, line 26. You discuss “arbitrary” splitting parameters. My understanding is these are not arbitrary, but the result of a grid search. Maybe it would be better to state that you search over certain bounds in fast axis and delay time?

Reply: We have corrected this part clearly as the text now emphasizing splitting parameters within certain bounds for fast axis and time delay are being tried in a grid search scheme.

Reviewer #1: Page 4, line 30 and figures 4,5,6. You show figures for one target with one reference. How do the results for a given target station vary if you vary the reference station? Ie could you use multiple reference stations to get at a distribution of splits? This may have important consequences based on the inter-station azimuth.
You present results for the station pairs, but what if you use a different reference at a specific target? (In-line comment, page 4, line 30)

Reply: For a given target station, we have several station pairs including all possible reference and target stations (with available horizontal components for the same event) within a interstation distance less than 300 km. Therefore, for this particular target station we have station pairs from many reference stations and final FPD are obtained by taking the circular mean of the obtained FPDs from each of different station pairs. So, different reference stations (within 300 km) are used for a specific target. We also make a plot of splitting parameter (ϕ and δt) versus inter-station distance of the target and reference station for a particular teleseismic event C101703B (-05.47°, +154.15°), that is recorded at target stations ES04, ES34, ES36, ES38 and ES39 and their corresponding reference stations. As can be seen clearly from the Fig. 2, we have observed no particular trend of the splitting parameter with inter-station spacing of target and reference stations.

Reviewer #1: At multiple points in the document, visual inspection or manual quality control are indi-cated and appear to have a significant effect on the final dataset. What criteria are used during this step?

Reply: 1st visual quality inspection: First of all, we selected only those waveforms with signal to noise ratio (SNR) $\geq$ 2.5. After that, we applied the visual quality check to eliminate the low-quality waveforms, to identify more accurately the phase onset, and to discard waveforms with no clear, direct S phase. During this process, we have checked the waveform energy on the transverse component by seeing the individual S waveforms and allow only those waveforms that have clear, direct S phase.
2nd visual inspection: We selected only the waveform pairs, which would manifest clear-cut splitting of the fast and slow component and free from any distortion in all the components due to signal processing. We also avoid any type of the contamination from the other phases or their multiples. This is now clarified in the text.

Reviewer #1: Page 5, line 11. What is confidence level used?

Reply: In our study, we have used 95% confidence level. It means that there is a probability of 95% of our splitting result is reliable.

Reviewer #1: Page 5, line 15. You have described what causes nulls, but more importantly, why would a station shows a null in SKS splitting and non-null direct- S splitting?

Reply: SKS phases sample a region with near vertical path where a downwelling mantle forms a consistent downward flow pattern, or the existing anisotropy is aligned vertically or steeply dipping while direct S-waves sample the region in a different manner. Thus its anisotropy comes from a different ray path. One strong possibility for observing a null anisotropy is due the lack of SKS measurements at a particular station. In such cases a given station may be represented with small times delays of station averaged splitting that is considered to be a null case (possibly associated to the events from very restricted directions). Contrary to the SKS phase direct-S waves provide much better azimuthal coverage and thus averaging. There are various instances where such discrepancies are reported (e.g. Saikia et al., 2010, JGR, Indian

shield). These points are mentioned in the result section.

Reviewer #1: Page 5, line 32. What do you mean by significant? Usually, that requires a statistical test at some p value. Maybe better to simply state that the splitting delay times are non-negligible or non-zero or on a similar scale as is typical for SKS splitting measurements?

Reply: We have used the term significant in terms of splitting time delays. We observe significant anisotropic time delays (mostly $>$ 1s) in our study by using direct S-waves. Our results show that the stations where no or negligible anisotropy was reported earlier (Sol et al., 2007) are supplemented with new measurements having clear splitting. It is now mentioned in the text.

Reviewer #1: Page 6, line 24. The splits do not appear “very complex” at all. There is largely an east-west orientation through the west and center of your array, with a rotation to northwest-southeast in the eastern region. This is pretty much what we might expect from the SKS splitting and the idea of material extruding to the east and the flow patterns being perturbed by strong blocks.

Reply: We agree with the reviewer. We have observed “significant” delay times evidencing for mantle deformation for stations where null measurements have been reported earlier inferred from SKS measurements. We have modified this part of the text.

Reviewer #1: Page 7, line 31. I do not know what a “crush” zone is. Please clarify.

Reply: It is a highly deformed zone which is a result of collisional tectonics ongoing between the Indian and Eurasian plates in northern and eastern Tibet (Zhao eta l., 2014, 2010). It is rephrased in the modified text.

Reviewer #1: Page 9, line 2. I think the orientation of the splits are NW-SE, not NE-SW.

Reply: Yes, the orientations of the splits are NE-SW. Now it is corrected in the modified text.

Reviewer #1: Page 9, line 7. Absolute plate motion does not require one of the plates to be fixed. So not sure what you are testing with different reference plates. It could be no-net rotation or hot spot reference frames?

Reply: Establishing a reference frame is one of the key steps for the plate motion calculation. This reference frame may be relative (movement of one plate about the fixed one) or absolute. Every reference frame has its advantages and limitations. In absolute reference frame, the absolute velocity of the plates is calculated via two methods namely hot spot frame, which is mostly suitable for HS2-NUVEL1A or HS3-NUVEL1A models and no-net-rotation (NNR) reference frames. The choice of mantle reference frame for the present day is best understood, but the determination of even the present-day mantle reference frame is difficult.
By using different plots of the plate motion (APM of Eurasian and Indian plate referenced to NNR frame or relative plate motion of Indian Plate referenced to Eurasian plate), we aimed at understanding the role of the APM on observed anisotropic variations. However we have not seen any correlation between both of these plate motion directions in different reference frames and FPDs.

Reviewer #1: Page 9, line 23. What evidence does sol present for a point?

Reply: Sol et al., 2007 observed the similar trend of the anisotropic and geodetic measurements for the entire southeastern Tibetan region, and on that basis, they discuss the coupling of the crustal and mantle material.

Reviewer #1: Page 9, line 26. No, receiver functions are not sensitive only to fossilized fabrics. They reflect the current state of the velocity structure.

Reply: No doubt, receiver functions reflect the current state of the velocity structure. We can even say station-specific receiver function analyses for anisotropic modeling may often introduce a risk of trade-off between heterogeneity and anisotropy both controlled anomalous energy on tangential component. In the current manuscript we favor anisotropy related signals on receiver function by starting from a recent study by Sherrington et al. (2014) that reports the depth varying anisotropy attributed to the indication for presence of fossilized fabric associated with amalgamation of terrains in Tibet may also contribute to anisotropy. 4–14$\%$ anisotropy at different depths in Tibet is reported by Sherrington et al., 2014 using inversion of RFs. They attribute is to both fossil fabrics and more recent deformation. This is now clarified in the text.

Reviewer #1: Page 10, line 4-6. You've just spent the prior paragraphs tying deformation and crust mantle coupling to splitting parameters and geodetic measurements, so why have this sentence?

Reply: We use that sentence since we cannot elucidate the issue of coupling between the crustal and mantle material only by comparing splitting measurements with geodetic measurements (GPS and APM). GPS measurements do not reveal the deformation of the whole crust but could be rather associated with the deformation of the shallow crust (Chen et al., 2013). For coupling/decoupling, we may need additional constraints on the crustal anisotropy either by the inversion of RFs or by other data set such as ambient noise, splitting of shear waves on local earthquakes etc.

Reviewer #1: Page 10, line 11. I'd say the results are inconsistent with the isotropic mantle hypothesis, but as mentioned above, that seems like a weak hypothesis anyways.

Reply: We have reformulated that part based on your suggestion.

Reviewer #1: What is rolling rack? (In-line comment, page 10, line 15.)

Reply: Rollback refers a geodynamic process in which a steep subducted tectonic plate sweeps back through mantle like a paddle to restore its original position. Now it is rephrased.

Answers to reviewer #2

Reviewer #2: The whole analysis relies on SKS splitting measurements being a useful way to correct receiver-side splitting in S waves. This is probably true at long distances, but probably less and less accurate as epicentral distances get shorter, when the ray path and incidences angles are quite different. The authors should, I suggest, show that there is no systematic variation in the S splitting parameters with distance. It would also be a good idea to test that excluding shorter-distance observations (say, less than 50°) does not change their average splitting parameters.

Reply: We have re-estimated average splitting parameters at all the target stations by incorporating only those teleseismic events within the epicentral distribution of 50°-90°. A comparison between resultant splitting results for both type of event distributions are presented below now. As can be seen clearly from the Fig. 3, we have observed no drastic pattern change for the lateral variations of the station-averaged splitting parameters after including events only from the epicentral distances of 50° to 90°.

Reviewer #2: A related point: do the SKS measurements that are used as corrections show any ‘complex’ behavior? That is, variation of splitting parameters with back-azimuth? The cited reference (Sol et al., Geology, 2007) does not list individual splitting measurements so it is impossible to knowâĂŤbut given the complexity of the region and the apparent ‘null’ stations (as the authors argue), it seems very likely that multiple anisotropic layers might be needed to explain observations. To that end, what is the uncertainty in these S wave splitting measurements due to (a) the variation in SKS splitting with back-azimuth, and (b) the fact that SKS and S polarizations may not be the same?

Reply: Corrections for receiver-side anisotropy beneath the reference station within Reference Station Technique were initially performed over S-waveforms collected from the northeast Tibet under the assumption of single layer anisotropy with a horizontal axis of symmetry in Eken and Tilmann (2014). Results from earlier SKS splitting stud-

ies (León Soto et al., 2012; Eken et al., 2013) have indicated that apparent splitting parameters would be independent from the event back azimuths implying that the assumption of a single anisotropic layer with a horizontal axis of symmetry is sufficient to explain the data.
However, beneath the regions with complex anisotropic structures as reviewer noticed, i.e., in case of a well-developed continental lithosphere with dipping axis of symmetry (i.e. stable cratonic regions, see Plomerová et al., 2008) or existing double-layer anisotropy (Silver and Savage, 1994), significant directional variation of apparent splitting parameters (FPDs and TDs) will be likely expected. In regions with such complicated anisotropic structures, an average value of splitting parameters as reference knowledge of seismic anisotropy in our method could be misleading since this average value cannot be representative for the events from different directions. However due to the fact that our approach certainly provides more splitting observations from an increased amount of back-azimuths, these new directionally enhanced apparent S-derived splitting parameters help in resolving the actual orientation of the anisotropic structure by using more sophisticated modeling strategies which will be within the scope of our current interest in another ongoing project.
To conclude, variations in apparent splitting parameters if exist due to the large-scale complex anisotropic structures will be similarly resolved when for both SKS- and SKS dependent direct S-wave splitting measurements. Differences in polarization properties will not have a drastic influence on the estimates of apparent splitting parameters since our approach tends to eliminate source-side complications on S-signals that are recorded from relatively distant earthquakes at two closely spaced stations.

Reviewer #2: The authors use epicentral distances down to 30°. At this distance, it's likely that shear-coupled P phases will interfere with the S arrival and may induce apparent splitting (e.g., Wookey & Kendall, JGR, 2004). Again, checking for distance-dependence in splitting parameters may help identify this, and applying wavefield decomposition (e.g., Kennett, GJI, 1991) can be done to see if this makes a difference.

Either way, it is a concern which should be addressed, because I see that very many events have been used which is very close (in terms of teleseismic splitting).

Reply: After overall processing, we have left with 501 waveform pairs recorded for 25 teleseismic earthquakes. The epicentral distribution of these events is plotted in Figure 6. From Figure 6 it is evident that only two events are close to 30°. Thus we do not think there will be a serious influence of interference of direct-S phases with shear coupled P phases on our final station-averaged splitting results.
Here we should note that Eken and Tilmann (2014) has, earlier, tested a variant of this technique using synthetic data in order to explore the influence of converted phases. To achieve this they first decomposed the three orthogonal components in to P, SV, and SH components, according to the technique explained by Bostock (1998), and then used the SH and SV components instead of the horizontal components. Decomposition process resulted in removal of converted wave energy from the SV components as expected under the assumption of flat-lying layers. Then results from the application of the RST to decomposed components (SH and SV) indicated very similar apparent splitting parameters to those inferred from direct usage of horizontal components (N-S and E-W) implying conversions from flat-lying layers did not substantially degrade the splitting estimates (Fig. 4).

Reviewer #2: How deep are the events used here? Shallow earthquakes can lead to interfering depth phases which induce apparent splitting. It would also be useful to know, as a test of the efficacy of the removal of the source term, what the source side splitting measurements are which are retrieved using this method. This information is also useful (Lynner & Long, JGR, 2014; Nowacki et al., G3, 2015) for other purposes. One would expect, as seen in these papers, a decrease in the source-side splitting with depth until the transition zone, where it seems to level off, but not a dramatic change in the amount of splitting in the top $\sim$ 100 km. The presence of this might indicate contamination with depth phases.

Reply: This is a good point and we thank reviewer for raising the issue of possible contamination by depth phases, which could appear as apparent source-side splitting theoretically. However, it should not be an issue for our approach that assumes the identical source-side effect on recorded S-signals at closely spaced two stations since this becomes just another detail of the effective source time function (assuming the move-out between depth and main phase is negligible over the epicentral range of stations compared).
On the other hand, the depth of the teleseismic events is mentioned in Table 1. To show simply negligible effect of possible contamination of depth effect, we have prepared Fig. 5. It presents variation of split time delays as a function of the source depth. We observe that time delays do not seem to be correlated with the source depth implying that our measurement are most probably associated to the receiver side anisotropy with possible but negligible contribution from source side anisotropy as obtained by Saikia et al.(2010) for the Indian continent.

Reviewer #2: Despite reading both the manuscript and Eken & Tilmann (BSSA, 2014), it took me some time to grasp the method, and I think it could be a bit clearer. My current grasp of it is the following: (1) An SKS measurement previously made at the reference station is removed from the reference station traces by application of the appropriate inverse splitting operator. (2) The best-fitting fast orientation and delay time at the target station is recovered which makes the uncorrected target and corrected reference signals match; this is done by grid search over all splitting operators. This corresponds to finding the splitting operator which restores the target trace to the reference trace with only the source-side splitting imposed, thus giving the receiver-side splitting. I think many readers would appreciate a slightly fuller exposition of how this all works to remove the source-side splitting. I think the authors do a good job explaining the assumptions, however.

Reply: Yes, our approach follows an algorithm exactly as reviewer describes. Because of similar critics from both reviewers we have now introduced more clarification to introduction and method sections of the main text in order to give a clearer insight into the reader about the methodology.

Reviewer #2: The authors report splitting measurements at each stationâĂŤthese are I think respectively the circular and linear means of the fast orientation and delay time for the set of reference stations used at that target station, but which reference stations went into each target station? All those within a 300 km radius? I can't find this information, which is necessary to reproduce the study. It should probably be added to Table 1. (See also my later point regarding supplementary material.)

Reply: For a given target station, all other possible stations within a 300km radius can be potentially used as a reference station. Once defining possible names for target and reference stations, then we checked whether both horizontal components for a given earthquake at that station pair are available for the reference and target station. We have reformulated this process to overcome any misunderstanding in the modified text. We have also provided now the list of the contributing reference station at each target station in Table 2.

Reviewer #2: The authors should also supply a list of the earthquakes used alongside Figure 2.

Reply: After overall processing, we are left with 501 waveform pairs that are contributing from 25 teleseismic events. We have already plotted only those teleseismic events in the epicentral distribution (Figure 6) and now also provide the list of those earthquakes in Table 1.

Reviewer #2: Have the authors implemented the updated F-test as described by Walsh et al. (JGR, 2013)? Or the one originally used by Silver & Chan (JGR, 1991)? The former has pointed out some errors in the original formulation which leads to underestimates of uncertainties.

T. Eken: We are not using either formula in original given in (Silver & Chan, 1991 - SC91) or the revised formulas in Walsh et al (2013). We simply perform the F-test calculation with a fixed value of one degree of freedom per second, based on a comment in the appendix of SC91 that this is a typical value irrespective of sampling rate. Here it is worth mentioning that we could be indirectly affected as this observation is based on the application of the faulty formula in SC91. With the current shape of the errors estimated from the F-test may be better used for the assessment of the relative reliability of different observations. Now, we notice this issue in the manuscript and plan to modify error calculation in the code for further studies.

Reviewer #2: p. 1, l. 2. '. . .splitting of the direct S-waves.' This is the first of many misplaced or absent articles, both definite and indefinite, throughout the manuscript, making some of it incorrect grammatically. I haven't corrected or noted all of these. These issues should be fixed before publication.

Reply: We have gone through possible grammatical issues with the help of a careful reading by reviewer #1. Addition to this we have taken the manuscript within an internal reading process and made some extra modifications on necessary places needed to be corrected in the text. After all, modified text has a much better shape in terms of being purified from its inverted sentences; typos, etc. that were present in the earlier version.

Reviewer #2: p. 2, l. 22. Particle motion does not have to be elliptical if splitting delay times are large compared to the dominant frequency. It will then be cruciform.

Reply: It is better clarified in the text.

Reviewer #2: p. 2, l. 27. 'Vertical transverse models . . . with [a] horizontal axis of the [sic] symmetry.' 'Vertical transverse' isotropy only applies when the axis of symmetry

is vertical. The authors mean 'horizontal transverse isotropy' (HTI), which is hexagonal symmetry with a horizontal symmetry axis.

Reply: It is now corrected in the text.

Reviewer #2: p. 4, l. 14. '. . . show the characteristic of the [sic] splitting with clear energy on the transverse component . . .' S waves are not polarised parallel to backazimuth, so there will in general be energy on both radial and transverse in isotropic models, depending on the source polarisation.

Reply: It is now rephrased in the text.

Reviewer #2: p. 4, l. 29. The authors use an upright triangle symbol for the time shift, but I think they mean to use a capital Greek delta (Δ). This is true throughout.

Reply: It is now mentioned in the text.

Reviewer #2: Table 1. Please supply this information electronically (i.e., in a plain-text ASCII file or spreadsheet). It would be a great help to future researchers who can then avoid trying to get the PDF file's information into a more useful form.

Reply: We have provided the list of the average splitting parameter in Table 3 in plain text ASCII format.

Reviewer #2: Figure 1. It would be helpful to include station names. Either the authors could use a subfigure, or simply enlarge the region around the stations.

Reply: The new version of the manuscript now includes modified Figure 1 with enlarged details of the study region including station names.

Reviewer #2: Figure 3. A scale bar for delay times is needed here too.

Reply: We have added a scale bar in this figure. But now it is named as a Figure 2.

Reviewer #2: Figure 7. There appears to be a plotting error where the fault traces are all cut off at 98° longitude. I also think that the subfigures could be larger and focused more on the region around the stations, to make the splitting bars easier to see. They could also be thicker.

Reply: We have corrected all these issues in Figure 7.

Reviewer #2: Figure 9. What is the scale for the lengths of the bars, GPS velocity arrows, and plate motion arrows?

Reply: We would like to thank reviewer for noticing this technical issue. We have revisited Figure 9 and added scale bars for splitting parameters, GPS velocity and plate motion.

Reviewer #2: Figure 9. What is the physical significance of the no-net rotation frame used for absolute plate motion vectors? I do not see why this was used rather than one based on hot spot tracks, for instance.

Reply: Establishing a reference frame is one of the key steps for the plate motion calculation. Every reference frame has its advantages and limitations. The choice of mantle reference frame for the present day is well understood, but its determination is difficult. In absolute plate motion, the absolute velocity of the plates, i.e., the velocity of the plates is expressed relative to the presumed strong deep mantle beneath the weak asthenosphere (e.g.,Cadek and Ricard, 1992, Kreemer et al.,2006, Argus and Gorden,1991, Rudolph and Zhong,2014) and is measured via two different absolute frameworks namely hot spot and no-net-rotation (NNR) reference frames. In hot spot reference frame method, it is assumed that the hotspots are stationary relative to the mesosphere and that age progressions and trends of linear island chains reflect plate

motion. The NNR reference frame method arises from the requirement that no net torque is exerted on the lithosphere, i.e., the integral of v * r over Earth' surface, where v is the plate velocity at position r (Kreemer et al., 2006, Argus and Gorden, 1991). In this method, if the coupling between the lithosphere and asthenosphere is assumed to be laterally uniform, and torque associated with the plate boundaries is assumed to be applied symmetrically to the two plates meeting at an edge then the no-net-rotation (or mean lithosphere) reference frame specifies the velocity of the plate relative to the mesosphere. In the tectonic zones, motion between plates is known much better than the motion of hotspots relative to the plates.
By using different plots of the plate motion (APM of Eurasian and Indian plate referenced to NNR frame or relative plate motion of Indian Plate referenced to Eurasian plate), we aimed at understanding the role of the APM on observed anisotropic variations. However we have not seen any correlation between both of these plate motion directions in different reference frames and FPDs.

Reviewer #2: Figure 9. Subfigure (c) is hard to see with the choice of colors. Perhaps choosing something other than orange would help.

Reply: We have replaced orange color with chocolate color to avoid confusion due to choices of color.

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/se-2016-134/se-2016-134-AC1-supplement.zip

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Interactive comment on Solid Earth Discuss., doi:10.5194/se-2016-134, 2016.

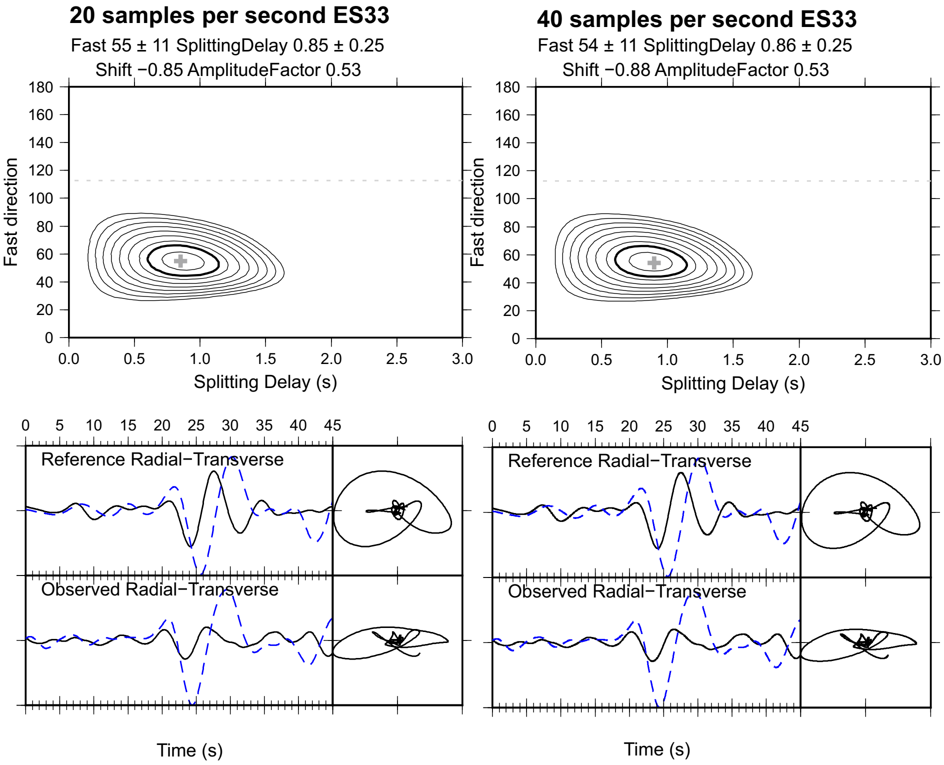

Fig. 1. Example of the direct S-waves measurement at a target station ES33 with 20 (left) and 40 samples per second (right) for a station pair ES12 (reference station) - ES33 (target station).

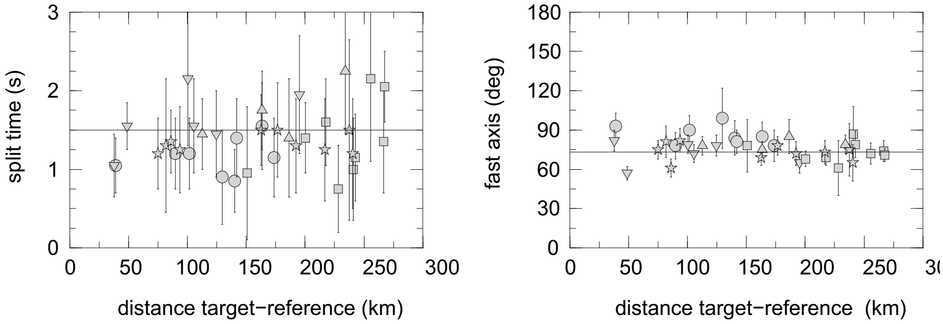

Fig. 2. Plot of splitting parameters (φ and δt) as a function of inter-station spacing of the target and reference station for a particular event C101703B, CMT ID (-05.47°, +154.15°) which recorded at target

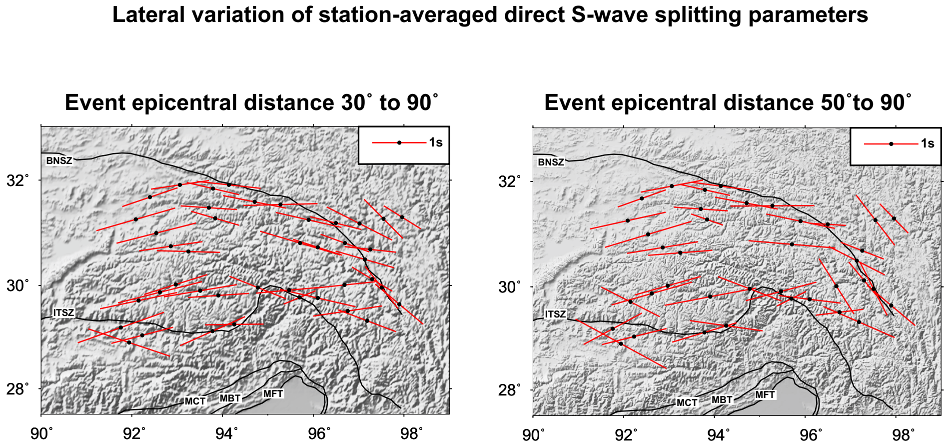

Fig. 3. Lateral variation of station-averaged S-derived splitting parameters (left) resultant average splitting parameters when all events within an epincetral distance range of 30°-90°. are in use (right) re

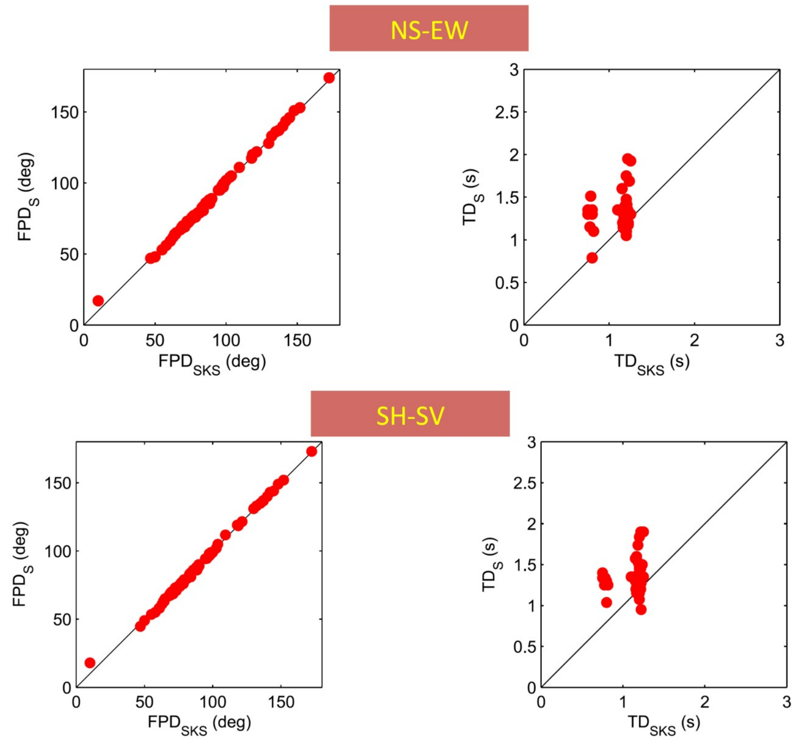

Fig. 4. Scatter plot of station averaged splitting parameters (FPDs and TDs) inferred from both synthetic S and SKS phases. (top) apparent splitting results inferred from N-S and E-W horizontal components. (b

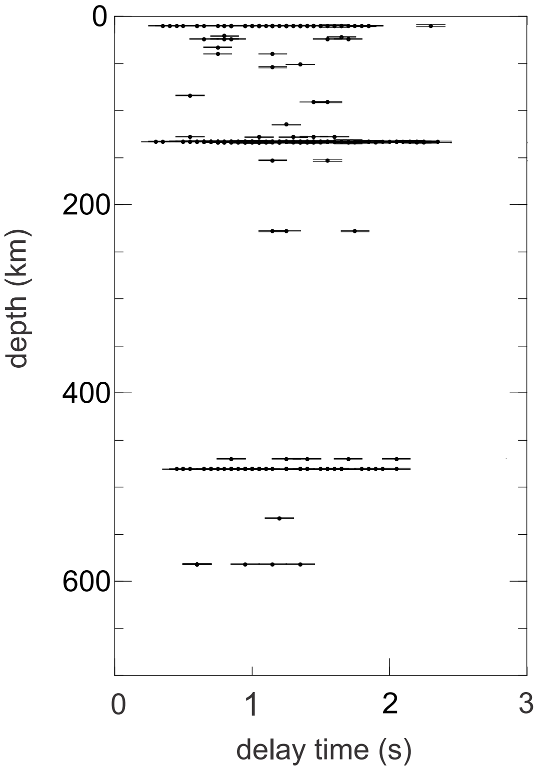

Fig. 5. Variation of split time delays with hypocentral depths of the events in this study.