

## Peer Review File

**Manuscript Title:** Enhanced Inner Core Fine-Scale Heterogeneity toward the Earth's Center

### Reviewer Comments & Author Rebuttals

#### Reviewer Reports on the Initial Version:

Referee #1:

{Please see Page No.2}

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Referee #2:

This is an interesting multidisciplinary study attempting to constrain the evolution of Earth's inner core (IC), using detailed seismic observations of small-scale scattering to develop models for IC heterogeneities, combined with geodynamical models of growth history. The seismic methodology for extracting IC scattering measurements is established, and presented in more detail in a related paper currently under review at Journal of Geophysical Research, included by the authors as a companion paper. The geodynamics aspect has also been published separately.

The seismological aspect builds upon the JGR paper with a global distribution of earthquakes and arrays and improved modelling. The outcome is a 3D map of IC heterogeneities down to 800 km depth, evidencing strong lateral variations in the uppermost IC, negligible hemispherical pattern, and an increase in scattering strength at around 500 km depth accompanied by a loss in lateral variations. I enjoyed the interdisciplinary approach; however, I have some queries about the interpretation of observations and modelling, particularly related to resolution and lateral heterogeneities.

- Lateral and depth variations in scattering strength are discussed only qualitatively in the context of the figures. These need quantitative statistical analyses. For example, Figures 3 and 4 both show a hemispherical difference in the depth of increase in heterogeneity, as well as the length scales of heterogeneity.
- Similarly, could the growth modelling generate regional variations? I think this is particularly important in light of the many other studies which have detected hemispheres and other lateral heterogeneities, in order that the model is compatible with wider studies.
- For the geodynamical modelling, I would also like to see the porosity model converted to seismic properties, ideally matching the actual data sensitivity. Given the highly heterogeneous data coverage, this would be important for an accurate comparison.
- Figure 3 and lines 389-410 need more clarification in terms of how the authors take various one- or two-layer models then obtain a four-layer map. Or, is the four-layer map generated using four-layer models (lines 145-147)? What is the justification for the selection of depths? What is the lateral resolution of Figure 3? Does it vary per region?
- Figure 4a – what is the resolution? Why are the gray patches unconstrained – no data? Is this an average projected onto the equator or a slice through the equator? A 3D animation would better represent the variations.
- The writing is generally clear throughout, however the improvements upon the JGR paper in terms of the seismic observations and modelling could be stated more directly.

Review: *Enhanced inner core fine-scale heterogeneity toward the Earth's Centre* by Pang et al.

The manuscript describes a study using inner core scattering (ICS) related to inner core boundary (ICB) reflected phases (PKiKP) to study the fine scale structure of the Earth's inner core. Understanding inner core structure is essential for our understanding of basic processes in the Earth and the evolution of our planet such as inner core growth, determination of the age of the inner core, geodynamo action and core-mantle heat flux. The authors have compiled a large dataset of high-frequency seismic records of ICS from seismic arrays and are using array processing to enhance the detectability and analysis of the ICS signal. As such the study can provide better insight into global variations of inner core heterogeneity structure. Although the sampling is more complete than in previous studies, it is not allowing global sampling and the authors are cautious with interpreting the lateral variations they observe. They report strong radial variations in heterogeneity structure globally and use this observation to infer growth scenarios for the inner core using inner core growth models.

ICS as probe for inner core heterogeneity has been well established and there have been recent publications reporting lateral variation (Wang and Vidale, 2022, as well as a publication in print provided by the authors as part of this submission (Wu et al., 2022) - outlining the data processing approach). Both studies use a single (Wang and Vidale, 2022) or two (Wu et al., 2022) arrays for the analysis therefore limiting the geographic coverage of the study. In general, the dataset compiled and analysed by the authors is the largest used for ICS analysis as far as I know. The processing is, in general, well established. The authors use a novel approach to characterize the heterogeneity through the elastic wave diffusivity  $D$  allowing a comparison of different models despite the inherent trade-off of  $\varepsilon$  and  $a$ , as demonstrated in the manuscript, as the two parameters defining heterogeneity in a random medium with a specific autocorrelation function.

The authors characterise the scattered signal in terms of the envelope fit and rise ( $t_{\text{growth}}$ ) and decay ( $t_{\text{decay}}$ ) times and use this to define best-fitting models in a forward modelling approach using an established phonon scattering approach.

The manuscript is very well written and the provided figures, method section and extended data figures support the text well. Nonetheless, I have a few concerns as well as minor points for the authors to consider:

#### Major points

- 1) The authors report lateral variation in scattering structure (Figs 2, 3, 4 and ext. data fig. 8). The authors seem to locate the heterogeneity leading to the ICS at the half-way point between source and receiver, but it is unclear if the ICS is really sensitive to the structure at this location. Extended Data Fig. 2 shows backazimuth and slowness values measured for example events in the three quality categories. These seem to indicate that the coda arrivals actually arrive with backazimuths different from the great circle path (although the small slowness values likely generate very large uncertainties on these measurements, but also might indicate a steeper incidence than expected again indicating a different location of the heterogeneity than assumed here). This seems to indicate that the sensitivity of ICS is not limited to

the ray theoretical reflection point which makes the interpretation of the lateral variation of the inner core heterogeneity more complex than presented in the manuscript. Can the authors clarify the sensitivity of ICS depending on source-receiver combination.

- 2) One of the major results of the manuscript is the radial variation of inner core heterogeneity with reported increase of heterogeneity at depths 500 to 800 km below the ICB with implications for the growth of the inner core and the interpretation of the innermost inner core. There are two issues with this report. First, it is unclear how sensitive ICS is for radial changes in heterogeneity structure. While the authors show the trade-off in  $\varepsilon$  and  $\alpha$ , a similar test for the envelope amplitude, onset and decay times is missing from the manuscript. Secondly, the forward modelling is restricted to a few models due to the very expensive forward modelling approach with Monte-Carlo Phonon scattering. The boundary depths for the multi-layered models are restricted to depths of 150 km, 300 km and 500 km below the ICB. With the boundary constrained in the forward models it is unclear whether the reported increase scattering is well constrained by the data or the result of the model constraints in the forward modelling. Especially the abrupt change in heterogeneity structure reported on line 187 might be more related to the modelling than required by the data.
- 3) The authors use data filtered between 1-2 Hz and 2-4 Hz depending on the array configuration. The wavelengths of P-waves in the inner core for these frequencies are on the order of  $\sim 3.5$  km to  $\sim 8$  km. For the modelling, the auto-correlation scalelength was fixed to 2 km. These scales are similar to scales found for other fine-scale studies of the inner core (e.g. Calvet and Margerin, 2008). But it remains unclear if the trapped fluid during the inferred rapid growth period after core nucleation would lead to such small pockets of (later solidifying) melt in a larger scale multigrain solid matrix. Is there anything in the literature indicating these scales are realistic?

#### Minor points

- i. The description of the misfit on lines 143ff is confusing given the approach outlined in the Methods section. From the text the authors indicate that the growth and decay rates are used to define the misfit. Equation 4 in the Methods section then also contains a term that seems to use the misfit in overall shape in terms of an L2 norm and the growth and decay times are used with an arbitrary weighting. Since the authors normalize the amplitudes of the synthetic and data envelopes, they are discarding important information on the inner core heterogeneity. This may be valid based on the uncertainties of inner core attenuation. Nonetheless, a better description of the misfit approach is needed that clearly states the advantages of the chosen approach.
- ii. Related to major point 1) the authors only report that they are studying 32 well sampled but size of these regions or how they are defined (geographic area or sampling density) is not reported.
- iii. The authors are very cautious about the changes of ICS and heterogeneity structure in the quasi-Eastern and quasi-Western hemispheres. I applaud this as the signal seems very weak. I wonder if the apparent effect, which the authors still mention, is

due to the different sampling of the different hemispheres and if a statistical test is possible to confirm this?

- iv. Line 140 indicates that the authors use a heterogeneous Earth model containing mantle heterogeneities in the forward modeling. What is the parametrisation of this heterogeneity and what is the impact of the mantle heterogeneity on the ICS signal after the processing that aims to remove this signal. Is the mantle heterogeneity necessary and given that studies seem to indicate lateral and radial variation in mantle heterogeneity (the latter is included it seems) does that affect the inner core heterogeneity results?
- v. Can the authors show an overview of all inner core radial heterogeneity models tested and how the best fitting models relate to these in the extended data figure?
- vi. The authors report on line 326 that envelopes from Cat III data can be used if they are stacked. The authors stack envelopes in geographic regions but it is not clear if this includes Cat. III data. Could the authors please clarify.
- vii. For the phonon scattering code the authors cite the work by Mancinelli and Shearer (2016) which is an extension for rough interfaces and not purely volumetric heterogeneity. Citing the original Shearer and Earle (2004) might be more appropriate unless the authors use the rough interface approach.
- viii. The authors use a 200 s time window around the ICS arrival time window to fit the coda decay. For the Cat. II example event in Ext. Data Fig. 2 this leads to a poor fit to the general P-wave coda due to mantle heterogeneity which is not observed in the other examples. I would have expected that the chosen parametrisation would provide a good fit throughout the P-wave coda. Can the authors explain the sub-optimal fit?
- ix. Extended Data Fig. 5 – typo in the label of the x-axis and line 110 typo in the reference to the Methods section.

## Author Rebuttals to Initial Comments:

### Comments from Reviewer #1

The manuscript describes a study using inner core scattering (ICS) related to inner core boundary (ICB) reflected phases (PKiKP) to study the fine scale structure of the Earth's inner core. Understanding inner core structure is essential for our understanding of basic processes in the Earth and the evolution of our planet such as inner core growth, determination of the age of the inner core, geodynamo action and core-mantle heat flux. The authors have compiled a large dataset of high-frequency seismic records of ICS from seismic arrays and are using array processing to enhance the detectability and analysis of the ICS signal. As such the study can provide better insight into global variations of inner core heterogeneity structure. Although the sampling is more complete than in previous lateral studies, it is not allowing global sampling and the authors are cautious with interpreting the lateral variations they observe. They report strong radial variations in heterogeneity structure globally and use this observation to infer growth scenarios for the inner core using inner core growth models.

ICS as probe for inner core heterogeneity has been well established and there have been recent publications reporting lateral variation (Wang and Vidale, 2022, as well as a publication in print provided by the authors as part of this submission (Wu et al., 2022) – outlining the data processing approach). Both studies use a single (Wang and Vidale, 2022) or two (Wu et al., 2022) arrays for the analysis therefore limiting the geographic coverage of the study. In general, the dataset compiled and analysed by the authors is the largest used for ICS analysis as far as I know. The processing is, in general, well established. The authors use a novel approach to characterize the heterogeneity through the elastic wave diffusivity  $D$  allowing a comparison of different models despite the inherent trade-off of  $\varepsilon$  and  $a$ , as demonstrated in the manuscript, as the two parameters defining heterogeneity in a random medium with a specific autocorrelation function.

The authors characterize the scattered signal in terms of the envelope fit and rise ( $t_{\text{growth}}$ ) and decay ( $t_{\text{decay}}$ ) times and use this to define best-fitting models in forward modelling approach using an established phonon scattering approach.

The manuscript is very well written and the provided figures, method section and extended data figures support the text well. Nonetheless, I have a few concerns as well as minor points for the authors to consider:

We appreciate acknowledgement from the reviewer of the novelty and significance of our study. We thank the reviewer for the constructive comments. We address each one below, point-by-point, and note when new material has been added to the revised manuscript using the relevant line numbers. Please note that the *line numbers correspond to the “clean” version* of the revised manuscript.

### Major points

- 1) The authors report lateral variation in scattering structure (Figs 2, 3, 4 and ext. data fig. 8). The authors seems to locate the heterogeneity leading to the ICS at the half-way point between source and receiver, but it is unclear if the ICS is really sensitive to the structure at this location. Extended Data Fig. 2 shows backazimuth and slowness values measured for example events in the three quality categories. These seem to indicate that the coda arrivals actually arrive with backazimuths different from the great circle path (although

the small slowness values likely generate very large uncertainties on these measurements, but also might indicate a steeper incidence that expected again indicating a different location of the heterogeneity that assumed here). This seems to indicate that the sensitivity of ICS is not limited to the ray theoretical reflection point which makes the interpretation of the lateral variation of the inner core heterogeneity more complex than presented in the manuscript. Can the authors clarify the sensitivity of ICS depending on source-receiver combination.

We agree this is an important point. Inner core scattered energy (ICS) is mainly sensitive to a region near and beneath the *PKiKP* reflection point, though it does have finite sensitivity over a large volume. Figure R1 in this response letter shows a sensitivity map created using a single-scattering ray theory approach modified from Leyton and Koper (2007). This is not technically a kernel, but it conveys similar information by showing the relative time and relative amplitude volume elements (voxels) in the inner core contribute to ICS. Note that the amplitude is a total amplitude that accounts for the scattering effect (assuming uniform scattering throughout the inner core) as well as for anelastic attenuation and transmission through the ICB and CMB. There are subtleties in this presentation, such as the fact that a single scatterer/voxel can contribute multiple ray-theoretical arrivals (with different times and amplitudes) connecting a given source and receiver, however it demonstrates that sensitivity falls off significantly away from the bounce point and it falls off more quickly laterally than it does with depth. In the current manuscript, we have re-averaged envelopes into a smaller number (20) of smoother patches, which better honors the sensitivity of ICS. Please see our answer to the Minor Points #2 below for details of how we define these patches. These details are now clarified on *Lines 389–396* in a new section entitled **Sensitivity of ICS energy and sampling of the inner core**. We also added text in the manuscript stating that the broad sensitivity of ICS may contribute to the blurring of hemispherical changes observed with transmitted inner core energy (*Lines 129–131*).

The arrays considered here are much smaller-aperture than LASA and so slowness resolution is not as high as in the work of Wang & Vidale. For instance, our Extended Data Fig. 2 showed examples from CMAR, which is known to have modest slowness anomalies caused by near-receiver geological heterogeneity (Figure R2, reproduced from Bondár et al., 1999). At very low ray parameter, these anomalies can cause large apparent changes in back azimuth, even though the scatterer location has not changed much. The situation is analogous to how longitude on the Earth's surface can change rapidly over small distances near the north and south poles.

Nevertheless, slowness information within the coda still constrains scattering locations to some extent. We investigated ICS sensitivity further by performing a slowness analysis for the Category I ICS observation in Extended Data Fig. 2. This earthquake also has a clear direct *PKiKP* phase. We performed the slowness analysis for two time windows: a five-second window starting one second before the *PKiKP* arrival and a randomly chosen five-second window deep in the coda (Figure R3a in this Response letter). For each time window, we performed 50 bootstrap iterations to estimate the uncertainties of the optimal slowness vector. We randomly resampled subsets of array elements and repeated the

slowness analysis to quantify uncertainties in the slowness estimates. Similar to Bondár et al. (1999), the back azimuth measured from the direct *PKiKP* phase is offset from the theoretical back azimuth (Figure R3b). Although the back azimuth measured for the coda energy is offset from the theoretical back azimuth, the 2D slowness vector is similar to the one from the direct *PKiKP* phase (Figure R3c). The consistency of 2D slowness vectors between two time windows suggests that the ICS energy is from the same general direction as the direct *PKiKP* phase, i.e., near the great circle path.

We further investigated ICS observations from YKA in Canada, XHDR in Antarctica, and WRA in Australia (Fig. 1). These arrays have larger apertures (over 20 km, Extended Data Fig. 1); thus, they have better slowness resolution (Fig. R2; Bondár et al., 1999). Similarly, we performed slowness analysis in two time windows: direct *PKiKP* phase and coda window. The back azimuths measured from the direct *PKiKP* phase are similar to the theoretical ones because of higher slowness resolution (Fig. R4–R6), although a few false peaks may be selected during bootstrapping because of the side lobes of the array response functions. Similar results are obtained for slowness analysis of ICS observations from ILAR in Alaska (Fig. R7). The back azimuths of coda waves are similar to the direct *PKiKP* phase, supporting the idea the ICS is mainly from the great circle path and mostly sensitive to the geographical region near *PKiKP* bounce points.

We added a new section in Methods named **Sensitivity of ICS energy and sampling of inner core** (*Lines 367–396*) to clarify the ICS sensitivity. In addition, we included Figure R4 in the new Extended Data Fig. 3 as an example of ICS slowness analysis.

- 2) One of the major results of the manuscript is the radial variation of inner core heterogeneity with reported increase of heterogeneity at depths 500 to 800 km below the ICB with implications for the growth of the inner core and the interpretation of the innermost inner core. There are two issues with this report. First, it is unclear how sensitive ICS is for radial changes in heterogeneity structure. While the authors show the trade-off in  $\varepsilon$  and  $a$ , a similar test for the envelope amplitude, onset and decay times is missing from the manuscript.

We tested the sensitivity of the ICS envelopes to various inner core heterogeneity models, especially for the two important parameters—growth time and decay time (Figure R8 in this Response letter). A delayed onset will be observed if heterogeneity is absent near the ICB (Fig. R8a). Fine-scale heterogeneity must exist near the ICB because all ICS observations have onsets starting from the *PKiKP* arrival. This point is now clarified on *Lines 438–440*. We started with the heterogeneity model proposed by Wu and Irving (2017) that has  $a=2$  and  $\varepsilon=0.5\%$  at the top 300 km inner core and then systematically varied the heterogeneity structure. Larger  $\varepsilon$  resulted in higher amplitudes (Fig. R8b) but had relatively minor effects on the growth time, decay time, and overall duration of the ICS envelopes, as highlighted in Fig. R8c. Using the maximum observed  $Q_I$  of 600 for the inner core can slow the decay rate slightly but the effect is minor, especially on growth time (Fig. R8d). Thickening the scattering layer without changing  $\varepsilon$  in the inner core is required to broaden the peak of the ICS and slow down its decay (Fig. R8e).

However, the coda maximum is still too early, and the coda decays too rapidly compared to our observations.

Generally, including stronger scattering in the deeper inner core can significantly increase the growth time and duration of the ICS envelope (Fig. R8f). Conversely, decreasing scattering in the deeper inner core results in faster growth, rapid decay, and shorter duration (Fig. 8f). A sharp peak will occur if scattering is only present within a thin layer of the inner core and the decay rate of the ICS envelope will increase significantly (Fig. 8f). Overall, the decay rate without stronger deep scattering is too fast and results in shorter ICS envelopes than what we observe. We documented details of these tests in Wu et al. (2022), which may not have been fully available to the reviewer at the time the original version of this manuscript was being reviewed. The final version of Wu et al. (2022) has now been published in *JGR-Solid Earth*. We included a summary of these tests on *Lines 432–434* in the revision and officially cited Wu et al. (2022) as reference #15.

Secondly, the forward modeling is restricted to a few models due to the very expensive forward modeling approach with Monte-Carlo Phonon scattering. The boundary depths for the multi-layered models are restricted to depths of 150 km, 300 km and 500 km below the ICB. With the boundary constrained in the forward models it is unclear whether the reported increase scattering is well constrained by the data or the result of the model constraints in the forward modelling. Especially the abrupt change in heterogeneity structure reported on line 187 might be more related to the modelling than required by the data.

As shown in Fig. R8, ICS envelope growth time and decay rate are sensitive to the radial changes of inner core heterogeneity. Our *PKiKP* coda observations are featured as emergent growth and slow decay rates, as shown in Fig. 2. Half of our observations have a long growth time of over 50 s and a slow decay time longer than 110 s. Most ICS observations have extremely long durations, longer than 160 s (Fig. 2). These ICS envelope features suggest fine-scale heterogeneity in the deep inner core. Slowness analysis shows that the slowness vector of late coda energy has similar back azimuth to the direct *PKiKP* window but with smaller magnitude (i.e., steeper incident angle), suggesting that the energy comes from great circle heterogeneity in the deep inner core (Fig. R4, R6, and R7). In particular, the coda waves arriving 100 s after *PKiKP* have much steeper incidence angles than the direct *PKiKP* but with similar back azimuth, supporting the idea that the late coda is mostly sensitive to the deep inner core beneath the *PKiKP* bounce point (Fig. R6 and R7). This idea is further supported by our sensitivity analysis shown in Fig. R1. As we mentioned on *Lines 120–126*, the deep scattering needs to be stronger to generate significant energy that can travel to the surface due to the high intrinsic attenuation within the inner core. Thus, the increased scattering is well constrained by the data, although as the Reviewer mentions the sharpness is less constrained. Therefore more complicated models, such as with gradients, cannot be excluded. This point is better clarified now on *Lines 178–180*.

- 3) The authors use data filtered between 1–2 Hz and 2–4 Hz depending on the array configuration. The wavelengths of P-waves in the inner core for these frequencies are on the order of ~3.5 km to ~8 km. For the modelling, the auto-correlation scale length was fixed to 2 km. These scales are similar to scales found for other fine-scale studies of the inner core (e.g., Calvet and Margerin, 2008). But it remains unclear if the trapped fluid during the inferred rapid growth period after core nucleation would lead to such small pockets of (later solidifying) melt in a larger scale multigrain solid matrix. Is there anything in the literature indicating these scales are realistic?

We are not aware of any constraints on the size of pockets of melt that might form under a super-cooling scenario. We assume that the size uncertainty is similar to that for inner core grain size, which generally allows 2–3 orders of magnitude in variation (e.g., Bergman, 1998; Monnereau et al., 2010; Yamazaki et al., 2017). Therefore, we consider this model to be a plausible mechanism for explaining our observations.

#### Minor points

- i. The description of the misfit on lines 143ff is confusing give the approach outlined in the Methods section. From the text the authors indicate that the growth and decay rates are used to define the misfit. Equation 4 in the Methods section then also contains a term that seems to use the misfit in overall shape in terms of an L2 norm and the growth and decay times are used with an arbitrary weighting. Since the authors normalize the amplitudes of the synthetic and data envelopes, they are discarding important information on the inner core heterogeneity. This may be valid based on the uncertainties of inner core attenuation. Nonetheless, a better description of the misfit approach is needed that clearly states the advantages of the chosen approach.

Although our dataset is the largest ICS dataset to date, it is not large enough to obtain smooth envelopes like those observed in crust or mantle studies, which stack many thousands of traces. The resulting ICS envelopes are not smooth enough and the L2 norm itself is easily biased by outliers. Explicitly including misfits of growth and decay times in the objective function help us more easily identify models with incorrect growth and decay times. The misfits of growth and decay times are in tens of seconds, an order or magnitude higher than the L2 norm, thus they are down-weighted by a factor of 10. Our motivation is better clarified on *Lines 458–470* now.

- ii. Related to major point 1) the authors only report that they are studying 32 well sampled but size of these regions or how they are defined (geographic area or sampling density) is not reported.

Thank you for this suggestion. Previously, we stacked observations recorded at the same array and obtained 32 ICS envelopes. Some of these envelopes may be from overlapping regions, especially given the sensitivity of the coda described above. As we have shown in response to question (1), the ICS is mostly sensitive to the region around *PKiKP* bounce points; thus, envelopes at different arrays sampled in similar geographic regions can be stacked together to obtain more stable ICS envelopes.

Thus, we further stacked envelopes recorded at different arrays if they sampled overlapping geographic areas. Some envelopes from nearby geographic areas are also combined if they have similar shapes based on 500 iterations bootstrapping procedure (Wu et al., 2022). Finally, we obtained high-quality ICS envelopes sampling 20 geographic regions. These regions are now clarified in the new Extended Data Fig. 4. We also added details about how these regions are defined in the revised manuscript on *Lines 389–396* in the new section **Sensitivity of ICS energy and sampling of the inner core**.

- iii. The authors are very cautious about the changes of ICS and heterogeneity structure in the quasi-Eastern and quasi-Western hemispheres. I applaud this as the signal seems very weak. I wonder if the apparent effect, which the authors still mention, is due to different sampling of the different hemispheres and if a statistical test is possible to confirm this?

We performed Student T-tests to evaluate the null hypothesis that there is no difference between hemispheres in the means of  $t_{growth}$ ,  $t_{decay}$  and  $t_{total}$ . We obtained p-values of 0.29, 0.9, and 0.68 if we used measurements sampling 60°E–160° for the qEH and ≥240°E for the qWH. We obtained more significant p-values if we included more data from 180°E–240°E for qWH. P-values larger than 0.05 suggest no statistical difference between the two hemispheres. We added this test in the revised manuscript on *Lines 127–128 and Lines 163–165*.

- iv. Line 140 indicates that the authors use a heterogeneous Earth model containing mantle heterogeneities in the forward modeling. What is the parametrization of this heterogeneity and what is the impact of the mantle heterogeneity on the ICS signal after the processing that aims to remove this signal. Is the mantle heterogeneity necessary and given that studies seems to indicate lateral and radial variation in mantle heterogeneity (the latter is included it seems) does that affect the inner core heterogeneity results?

We used the same mantle scattering profile in all models:  $\varepsilon = 2\%$  and  $a = 4$  km for the upper 600 km of the mantle,  $\varepsilon = 0.5\%$  and  $a = 8$  km for the rest of the mantle, with all density perturbations set to 0.8 of velocity perturbation (Peng et al., 2008). This mantle scattering profile is similar to the mantle scattering model obtained from the global teleseismic *P* coda from Shearer & Earler (2004) but with weaker  $\varepsilon$  in the upper mantle to generate suitably impulsive *PcP* and *PKP* arrivals (Peng et al., 2008). We added these details in our revised manuscript on *Lines 449–453*.

The main reason to include mantle scattering is to mimic the *PKiKP* coda wave propagation in the real Earth as closely as possible. Then we can apply the same processing to data and synthetics, allowing for better comparisons. The *PKiKP* coda is weakly influenced by the crust or mantle, as shown by Extended Data Fig. 3, where similar observations are observed from two arrays with different mantle samplings. Furthermore, we tested other mantle scattering models in the phonon simulation. We obtained the same *PKiKP* coda shapes for the same inner core scattering model,

further supporting the idea that the *PKiKP* coda energy is less affected by the mantle. This point is clarified in the Methods section on *Lines 446–449*.

- v. Can the authors show an overview of all inner core radial heterogeneity models tested and how the best fitting models relate to these in the extended data figure?

We modified Extended Data Fig. 7. Now it shows all phonon models and the best fitting model to one example observation.

- vi. The authors report on line 326 that envelopes from Cat III data can be used if they are stacked. The authors stack envelopes in geographic regions but it is not clear if this includes Cat. III data. Could the authors please clarify

We used all data categories (I–III) in the stacks. We clarified this point in the revised manuscript on *Line 350*.

- vii. For the phonon scattering code the authors cite the work by Mancinelli and Shearer (2016) which is an extension for rough interfaces and not purely volumetric heterogeneity. Citing the original Shearer and Earle (2004) might be more appropriate unless the authors use the rough interface approach.

We used the updated phonon code by Mancinelli and Shearer (2016) to explore if rough interfaces on the ICB can generate the observed ICS. As we described in Wu et al. (2022), a rough ICB cannot explain the observed ICS, therefore we only model the inner core volumetric heterogeneity in the present manuscript. We agree with the reviewer that Shearer and Earle (2004) should be cited and updated it in the revised manuscript as reference #16.

- viii. The authors use a 200 s time window around the ICS arrival time window to fit the coda decay. For the Cat. II example event in Ext. Data Fig. 2 this leads to a poor fit to the general P-wave coda due to mantle heterogeneity which is not observed in the other examples. I would have expected that the chosen parametrization would provide a good fit throughout the P-wave coda. Can the authors explain the sub-optimal fit?

We fit a traditional coda model (Eq. 1; Sato et al. (2012)) to two noise windows to obtain the background coda level. However, the model does not describe multiple interfering codas sufficiently. The relatively poor fit to the general *P*-wave coda is because of some weak signals within the first fitting window near 900 s. A situation like this was the motivation to perform the other stripping method described in Wu et al. (2022), which has more freedom. The agreement between these two stripping methods gives us confidence that we removed the background energy correctly. We tested other window lengths in background noise stripping. For the event mentioned by the reviewer, a 300 s noise window did return a better fit to the general *P*-wave coda as the other two events and the denoised *PKiKP* coda is similar to the denoised *PKiKP* coda presented in Extend. Data Fig. 2. However, a 300 s noise window is too long for many events in our database. On the other hand, a 100 s noise window can be

too short to model the background coda sufficiently. Thus, we used a 200 s window in our results. In other words, we settled on 200 s mainly through a process of trial and error that resulted in a value we could apply consistently throughout our dataset. This point is now clarified on *Lines 341–342*.

- ix. Extended Data Fig. 5 – typo in the label of the x-axis and line 110 typo in the reference to the Methods section.

Thank you, we corrected these typos in the new Extended Data Fig. 6 and on *Line 114*.

We appreciate the detailed and cogent comments from Reviewer #1. We feel our manuscript is much improved having addressed the comments.

### **Comments from Reviewer #2**

This is an interesting multidisciplinary study attempting to constrain the evolution of Earth's inner core (IC), using detailed seismic observations of small-scale scattering to develop models for IC heterogeneities, combined with geodynamical models of growth history. The seismic methodology for extracting IC scattering measurements is established, and presented in more detail in a related paper currently under review at Journal of Geophysical Research, included by the authors as a companion paper. The geodynamics aspect has also been published separately. The seismological aspect builds upon the JGR paper with a global distribution of earthquakes and arrays and improved modelling. The outcome is a 3D map of IC heterogeneities down to 800 km depth, evidencing strong lateral variations in the uppermost IC, negligible hemispherical pattern, and an increase in scattering strength at around 500 km depth accompanied by a loss in lateral variations. I enjoyed the interdisciplinary approach; however, I have some queries about the interpretation of observations and modelling, particularly related to resolution and lateral heterogeneities.

Thank you. We are thrilled to hear that you enjoyed reading our work. Please see below for our point-by-point responses to your comments. Please note that *the line numbers are from the "clean" version* of the revised manuscript.

- Lateral and depth variations in scattering strength are discussed only qualitatively in the context of the figures. These need quantitative statistical analyses. For example, Figures 3 and 4 both show a hemispherical difference in the depth of increase in heterogeneity, as well as the length scales of heterogeneity.

We agree, please see our responses to similar points raised by Reviewer #1. In summary, we redid the spatial averaging of our data (*Lines 367–396* and new Extended Data Fig. 4) to better honor the sensitivity of *PKiKP* coda waves shown in Figure R1. We also added text describing how the averaging properties of the *PKiKP* coda waves could hide sharp lateral and radial changes in scattering strength (*Lines 129–131*). And we added a formal Student T-test to evaluate the null hypothesis that there is no difference between hemispheres in the mean of  $D^{-1.5}$  at different depths. The tests (p-values > 0.2) show the lack of a significant quasi-hemispherical signal (*Lines 163–164*). Note that the uncertainty is complicated by the fact that there is

significant variance in published values for the hemispherical boundaries from studies of transmitted waves.

- Similarly, could the growth modelling generate regional variations? I think this is particularly important in light of the many other studies which have detected hemispheres and other lateral heterogeneities, in order that the model is compatible with wider studies.

The growth modeling was a 1D simulation and therefore we are emphasizing and primarily interpreted the radial variation in scattering strength. Our geodynamical models are meant to illustrate the possible interpretations for this enhanced fine-scale heterogeneity in the deeper inner core. We agree with the reviewer that regional variation of porosity is important and may provide interpretations of the inner core lateral variations. The increased porosity in depth may create 3D variations, but it requires the time scale of instability to be shorter than the age of the inner core. There are also propositions for inner core lateral variations due to other processes, such as asymmetric growth (e.g., Aubert et al., 2008; Monnereau et al., 2010; Frost et al., 2021). It will be necessary to include the asymmetric growth and melt dynamics to explore the regional variations. However, unfortunately, to the best of our knowledge, no related theory/work has been developed yet for 3D modeling of this type. We hope our new seismic results can help inspire others to work on this research topic and decipher the mysteries of inner core evolution.

- For the geodynamical modelling, I would also like to see the porosity model converted to seismic properties, ideally matching the actual data sensitivity. Given the highly heterogeneous data coverage, this would be important for an accurate comparison.

Singh et al. (2000) calculated inner core seismic velocity with the presence of melts using an effective medium theory of composite materials. Elastic parameters of the hcp Fe crystal at 330 GPa and room temperature were used in their work and seismic velocities with different shapes of melt inclusions were calculated (Fig. S1 at Singh et al., 2000). Our preferred porosity model has an average ~3% porosity 100–500 km below ICB and ~8% porosity within the radius of 700 km. Based on Fig. S1 of Singh et al. (2000), 0.8% velocity variation is obtained from ~3% porosity, and 2.5% velocity variation is obtained from ~8% porosity with isotropic melt pocket assumption. The velocity variations are similar to our scattering models. However, these are only rough estimations. As shown in Singh et al. (2000), the seismic velocity is highly depended on the shape of melt pocket that flat-disk like melt pocket could cause more significant velocity variation and thin-fiber like melt cause much less velocity changes. Other factors, such as unknown of inner core iron's crystal structure (hcp vs. bcc), presence of light elements, and uncertainties of elastic parameters of Fe at high pressure and high temperature, are not considered. More importantly, the trapped melt would crystallize later in time making it more complicated to estimate the seismic properties from a porosity model. Overall, as we mentioned on *Lines 205–209*, the increasing porosity changes the volume fraction of trapped fluid. The trapped fluid would freeze later in time, ultimately leading to a distinct texture which in principle cause strong heterogeneity. However, the conversion from porosity model to seismic properties could be adventurous due to the less constrained shape of melt pocket, elastic parameters of Fe at core condition, and the later crystallization of the melt. Therefore, it is more important to decipher the pattern of variation and we retain qualitative comparison between our seismic heterogeneity profile and porosity profile in the revised manuscript.

• Figure 3 and lines 389-410 need more clarification in terms of how the authors take various one- or two-layer models then obtain a four-layer map. Or, is the four-layer map generated using four-layer models (lines 145-147)? What is the justification for the selection of depths? What is the lateral resolution of Figure 3? Does it vary per region?

Our models contain up to two layers in the uppermost 800 km of the inner core, and the four-layer map was obtained by simply splitting the layer and using the same diffusivity. For example, for a model that has an interface 300 km below ICB, we simply used the same diffusivity value from the 0–300 layer in the 0–150 km and the 150–300 km maps and diffusivity from 300–800 km layer in the 300 – 500 km and 500 – 800 km maps. We looked at four depths to investigate the lateral variation of depth dependence.

We did a resolution test in that we shifted the interface from 150 km to 500 km in steps of 50 km and found that at least 150 km is needed for generating distinguish synthetic envelopes. It required a larger separation when the interface is deeper than 300 km below ICB to make a difference in the ICS envelope because of the attenuation. Thus, we chose 150 km, 300 km, and 500 km as interfaces of our models. This point is now clarified on *Lines 440–443*.

We stacked ICS envelopes from individual earthquakes that sample similar geographic regions (at least in a size of  $20^\circ$  by  $15^\circ$ ) to resolve high-quality, low-variance ICS envelopes. The final ICS envelopes represented the average scattering properties of the region. Following Reviewer #1's suggestion, we provided details (*Lines 367–396*) and a new Extended Data Fig. 4 about how these regions are defined and we updated Fig. 3 and Fig. 4. We hope the new Fig. 3 provides a better view of inner core fine-scale heterogeneity structures.

• Figure 4a – what is the resolution? Why are the gray patches unconstrained – no data? Is this an average projected onto the equator or a slice through the equator? A 3D animation would better represent the variations.

Figure 4a shows the average of all latitudes projected onto the equator. We test the radial resolution of the simulations. We shifted the interface from 150 km to 500 km using steps of 50 km. We found that at least 150 km is required to generate distinguishing ICS envelopes (*Lines 440–443*). Our models lose resolution on scattering deeper than 800 km below the ICB due to attenuation (*Line 153–154*); thus, there is an empty circle with a radius of 421 km in the center. We do not have data for some longitudes leading to some patches along the azimuths. Some patches 500–800 km below the ICB exist because scattering at that depth is not required to fit the observations sampling the region, perhaps due to the strong scattering at shallow depth. This point is now clarified in the caption of Fig. 4. We did consider a 3D animation when we made Figures 3 and 4; however, we found that the exhibition is less clear due to the limitation of the dataset. Thus, instead of making a 3D plot, we made separate Figures 3 and 4 to show the variations. More importantly, our geodynamic simulation is solely a 1D simulation. The current Figure 4 can better focus on the radial variation and hence be compared to our geodynamic simulation.

- The writing is generally clear throughout, however the improvements upon the JGR paper in terms of the seismic observations and modelling could be stated more directly.

We used the JGR paper (Wu et al., 2022) to present details of our workflow in detecting and modeling *PKiKP* coda. The paper is officially published in JGR and we cite the paper in our revised manuscript as reference #15. The major novelties in the current manuscript are (1) an increase in the number of seismic arrays used by a factor of ten (from 2 to 20) that allows for a global study, (2) the discovery of increasing scattering strength with depth throughout the inner core, and (3) the development of a plausible geodynamical model and scenario for explaining point #2. We have tried to emphasize these distinguishing characteristics throughout the manuscript, and they have been summarized on *Lines 69–74*.

We thank Reviewer #2 for spending the time to thoughtfully review our work. The comments allowed us to make significant improvements in the manuscript.

#### **New References in the Response Not in the Revised Manuscript:**

- Bergman, M. I. (1998). Estimates of the Earth's inner core grain size. *Geophysical research letters*, 25(10), 1593-1596.
- Bondár, I., North, R. G., & Beall, G. (1999). Teleseismic slowness-azimuth station corrections for the International Monitoring System seismic network. *Bulletin of the Seismological Society of America*, 89(4), 989-1003.
- Frost, D. A., Lasbleis, M., Chandler, B., & Romanowicz, B. (2021). Dynamic history of the inner core constrained by seismic anisotropy. *Nature Geoscience*, 14(7), 531-535.
- Leyton, F., & Koper, K. D. (2007). Using PKiKP coda to determine inner core structure: 1. Synthesis of coda envelopes using single-scattering theories. *Journal of Geophysical Research: Solid Earth*, 112(B5).
- Singh, S. C., Taylor, M. A. J., & Montagner, J. P. (2000). On the presence of liquid in Earth's inner core. *Science*, 287(5462), 2471-2474.
- Yamazaki, D., Tsujino, N., Yoneda, A., Ito, E., Yoshino, T., Tange, Y., & Higo, Y. (2017). Grain growth of  $\epsilon$ -iron: Implications to grain size and its evolution in the Earth's inner core. *Earth and Planetary Science Letters*, 459, 238-243.

## Figures

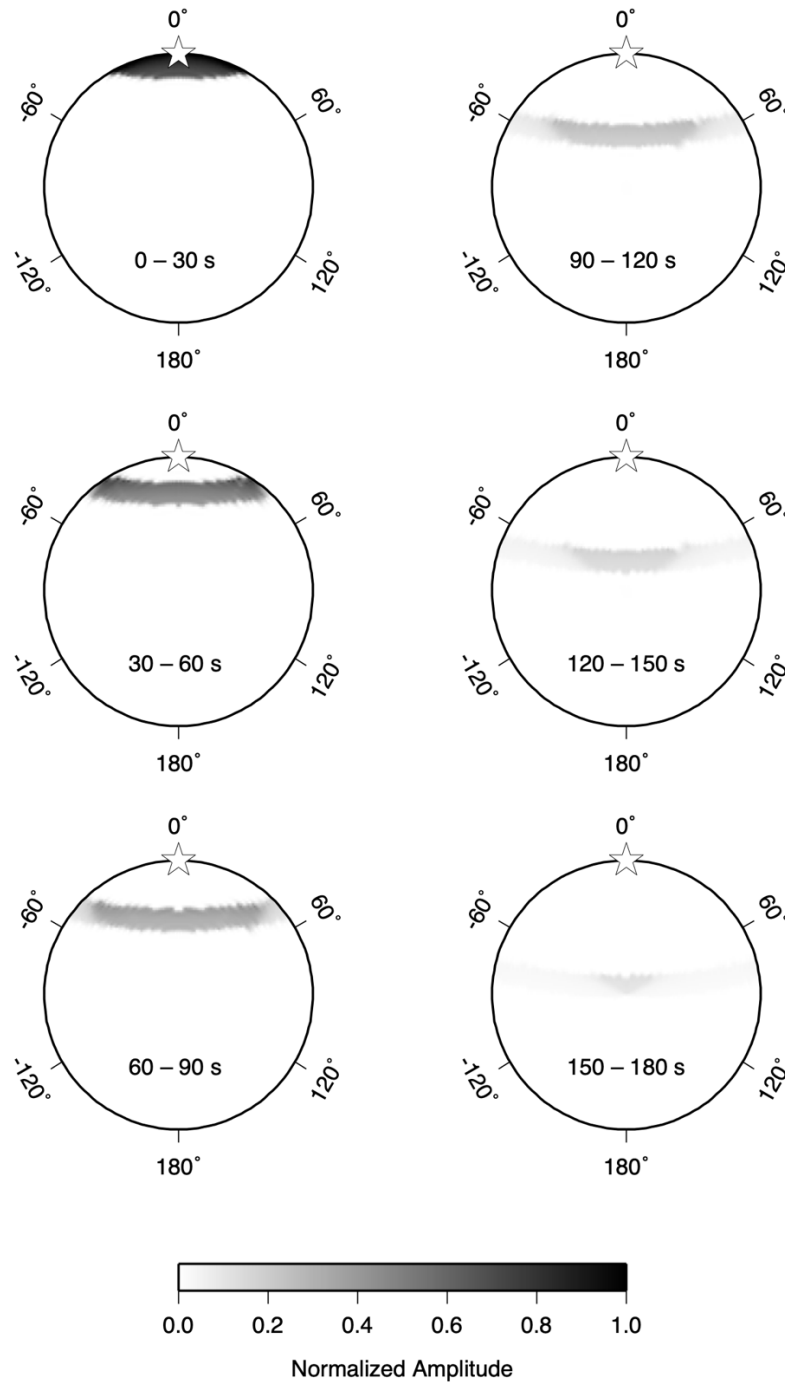


**Figure R1.** Contribution of each volume element within the inner core to *PKiKP* coda waves for a source-receiver distance of  $60^\circ$ . The white star indicates the bounce point on the ICB. The lapse time labeled inside the panels refers to time after the *PKiKP* arrival. The relative amplitude includes effects from scattering, anelastic attenuation, and transmission through the CMB and ICB. Calculations are made using ray theory and a single-scattering approximation.

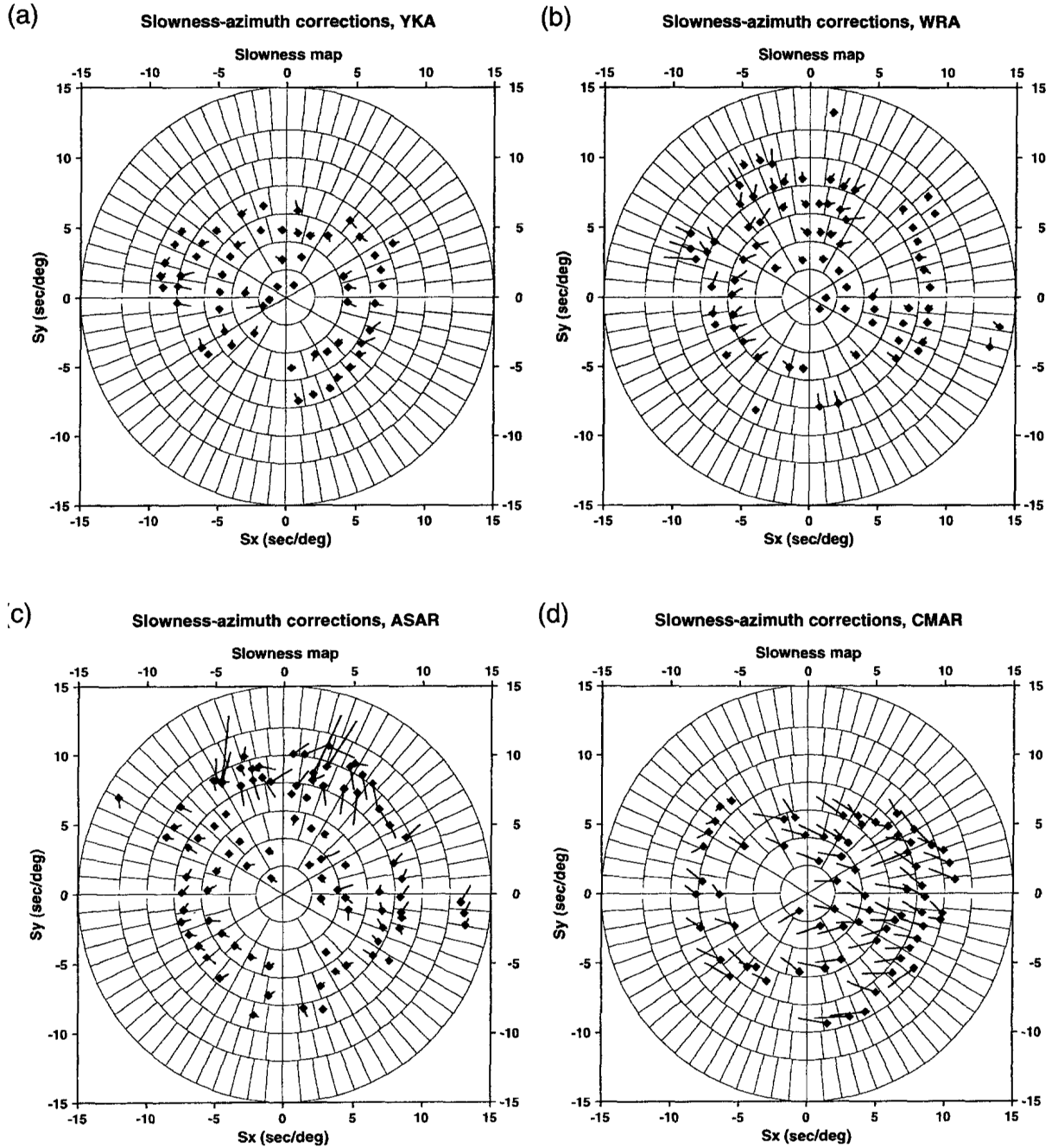


Figure 4. Slowness-azimuth station correction (SASC) map for YKA (a), WRA (b), ASAR (c), and CMAR (d). The vectors point from the observed values to the theoretical (diamonds) ones.

**Figure R2.** Figure 4 of Bondár et al. (1999) shows slowness-azimuth anomalies for IMS arrays. CMAR is shown in panel d. Note that back azimuth anomalies can be quite large near the origin, even when the anomaly in the 2D slowness vector is modest.

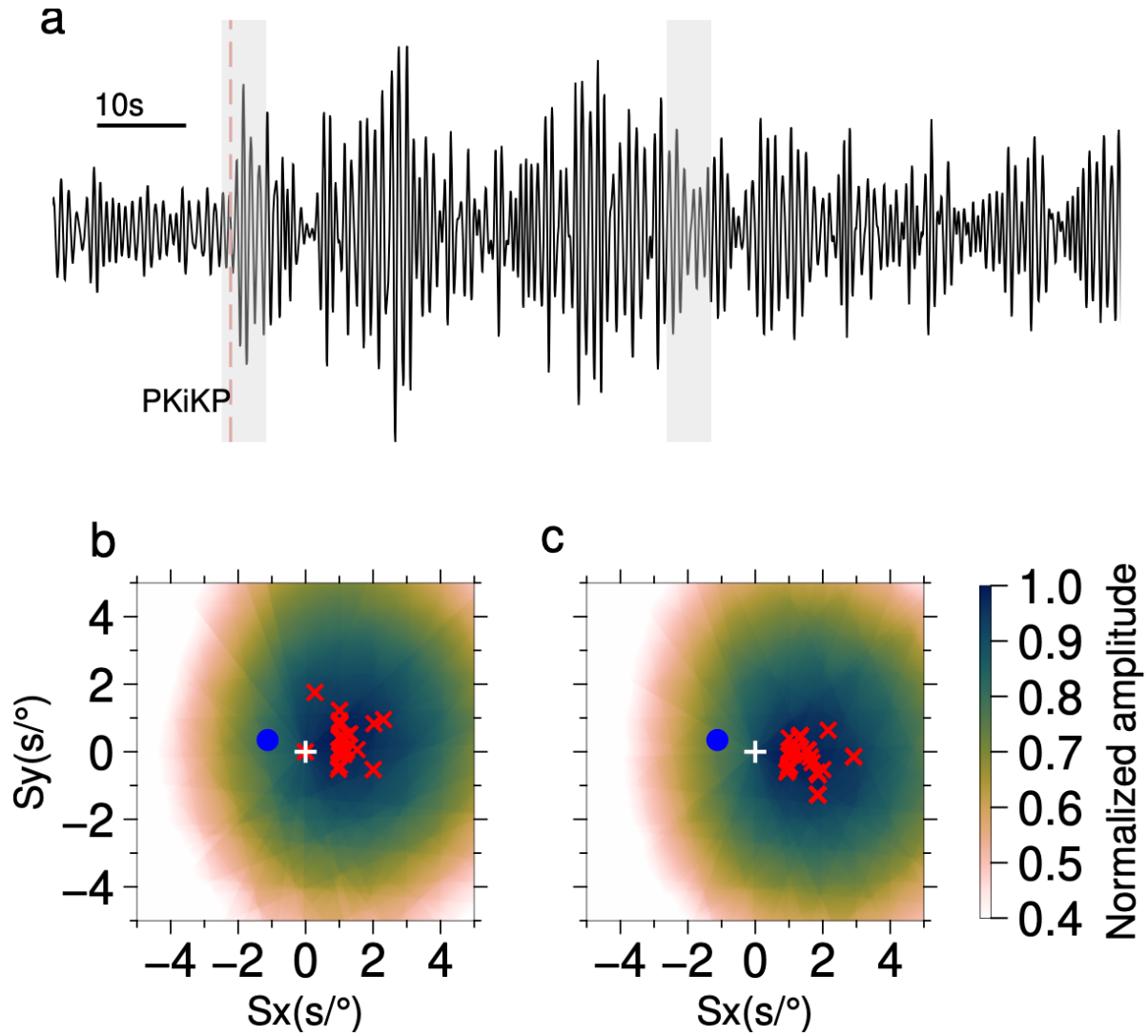


**Figure R3. Slowness vector analysis of ICS energy at CMAR.** (a) Linear beams of *PKiKP* coda waves. The dashed red line marks the direct *PKiKP* phase. Gray windows mark the time windows analyzed in b and c. (b) Slowness analysis for the direct *PKiKP* phase (first gray window). The blue dot marks the theoretical slowness from AK135. The white cross marks the origin (vertical incidence). Red crosses represent bootstrap solutions determined by repeating the grid search for 50 randomly resampled sets of array elements. (c) Slowness analysis for the coda waves (second gray window). Symbols same as (b)

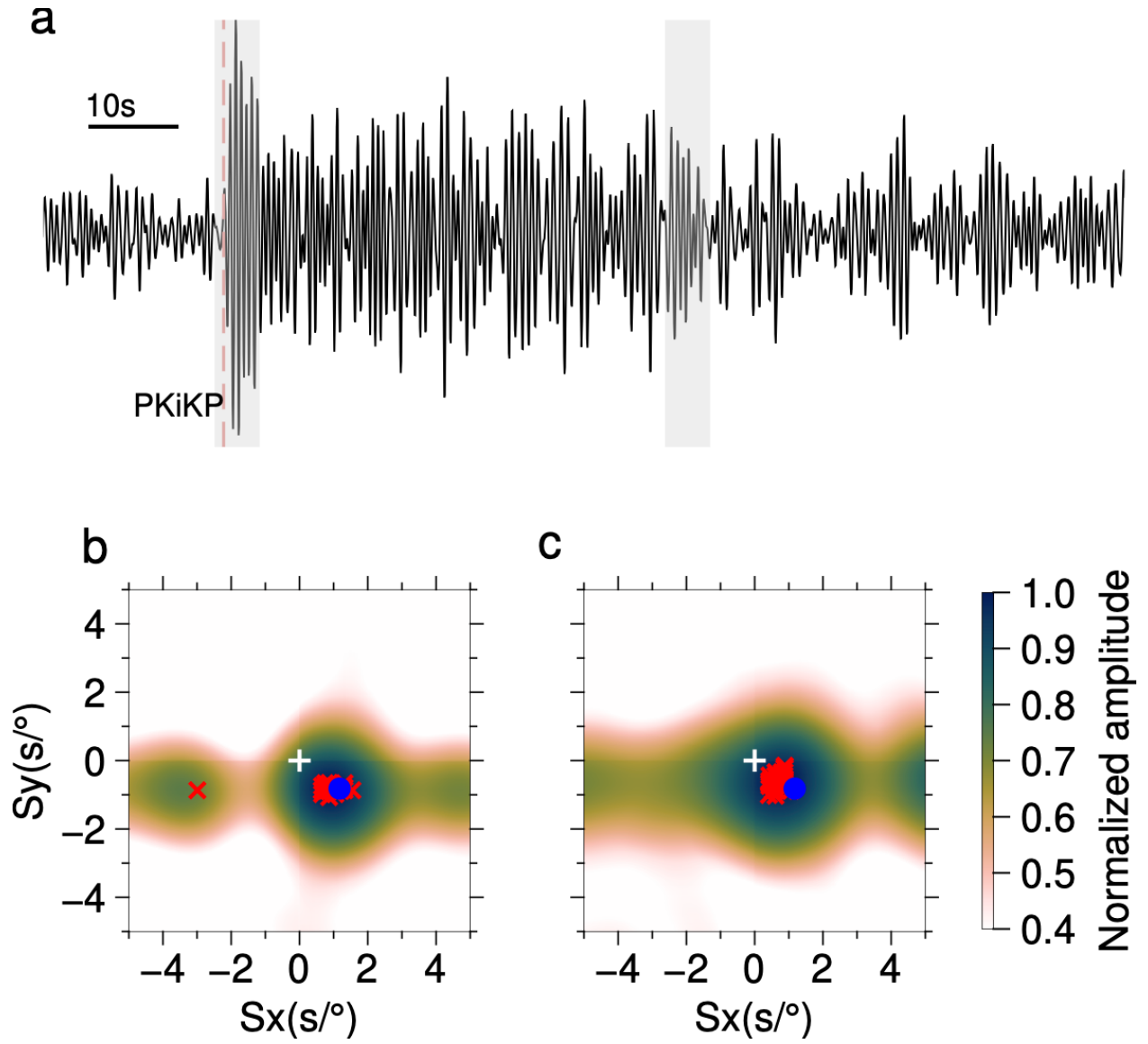


**Figure R4. Similar to Figure R3 but for slowness vector analysis of ICS energy at YKA.** The aperture of YKA is about twice that of CMAR (Fig. R3), hence the slowness resolution is higher.

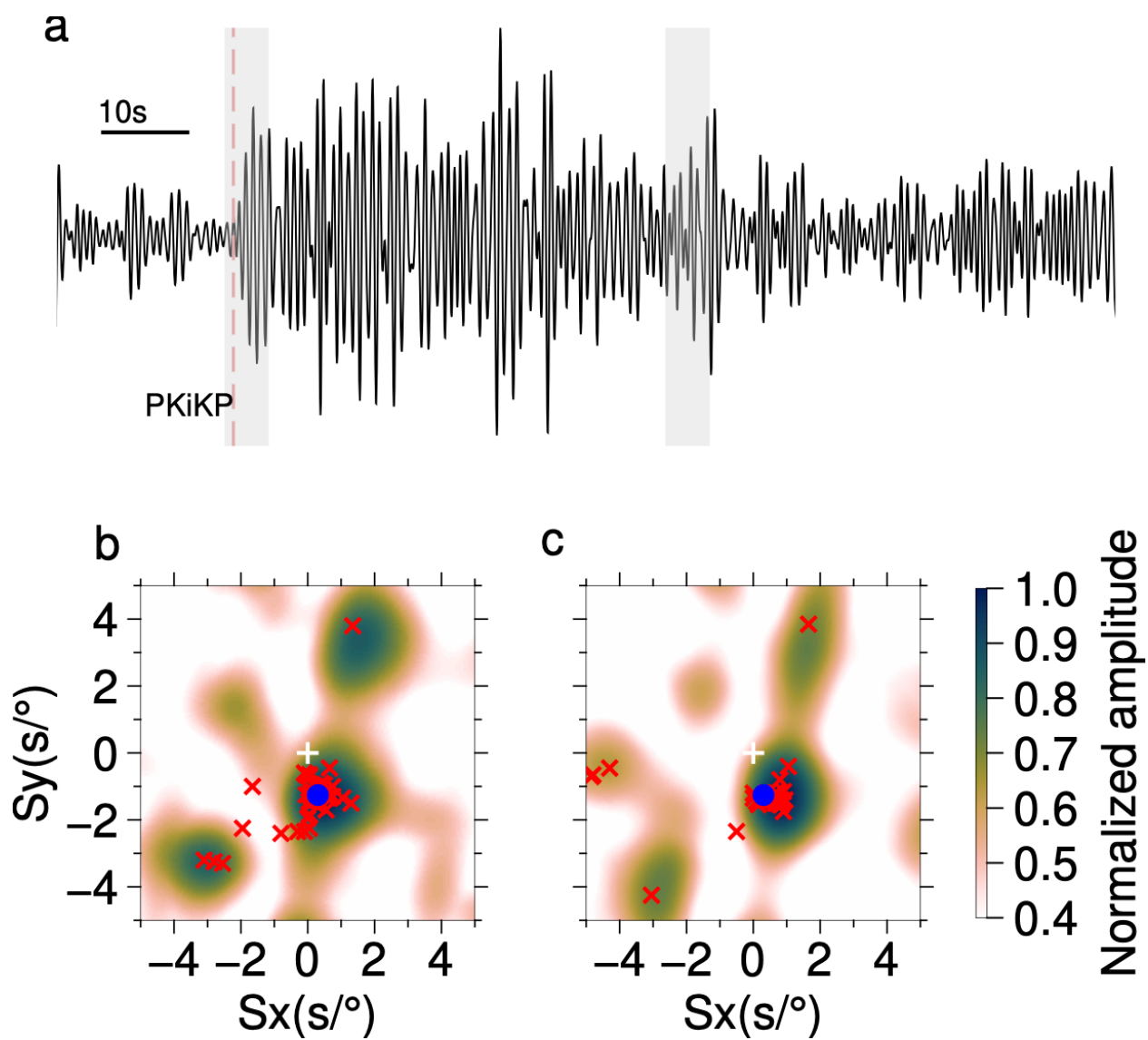


**Figure R5.** Similar to Figure R3 but for slowness vector analysis of ICS energy at XHDR.

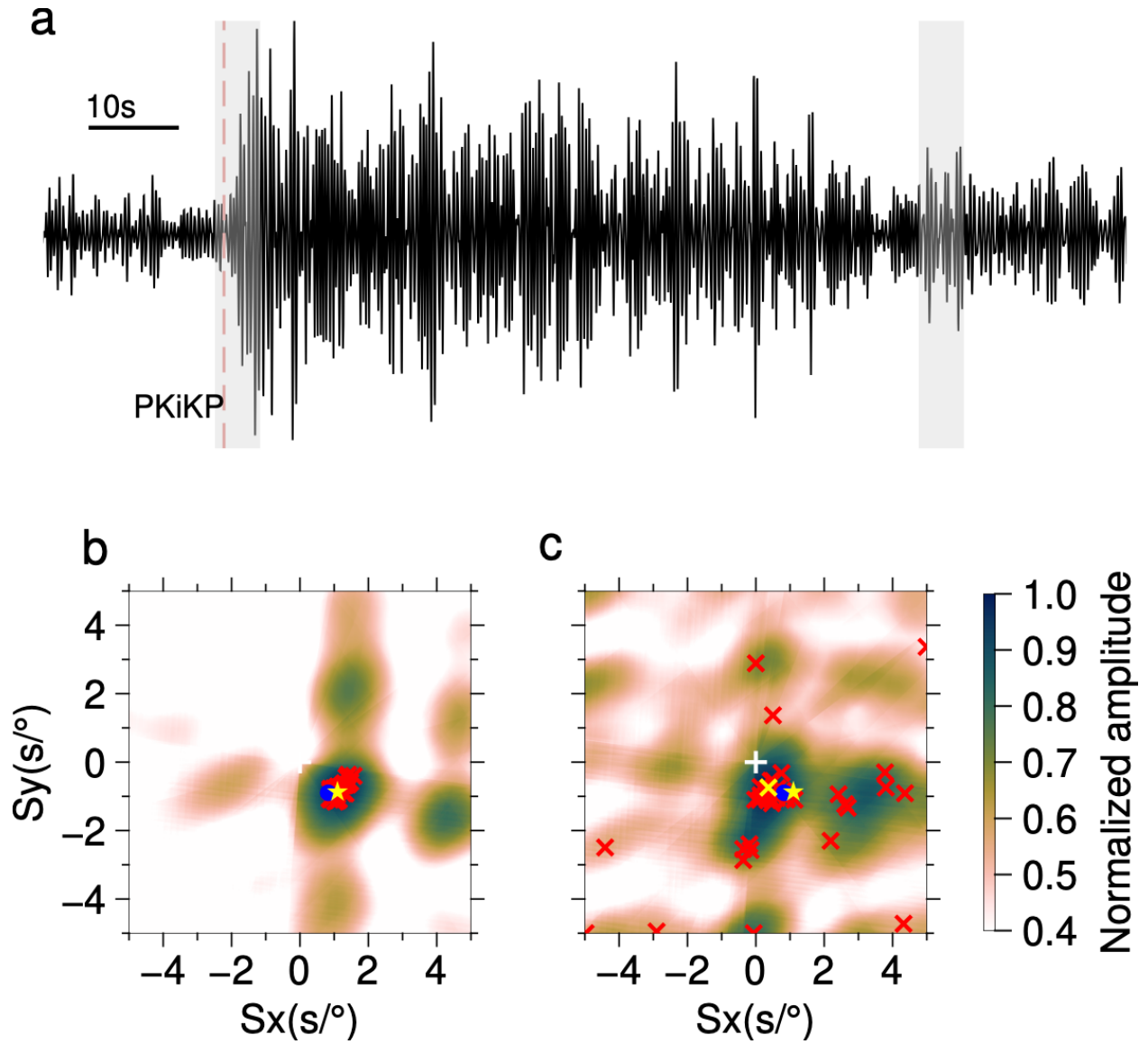


**Figure R6. Similar to Figure R3 but for slowness vector analysis of ICS energy at WRA.** Yellow stars are the optimal slowness using all WRA array elements for the direct PKiKP energy (first gray window in (a)). The yellow cross is the optimal slowness using all array elements for the coda energy (second gray window in (b)). Other symbols same as Fig. R3.

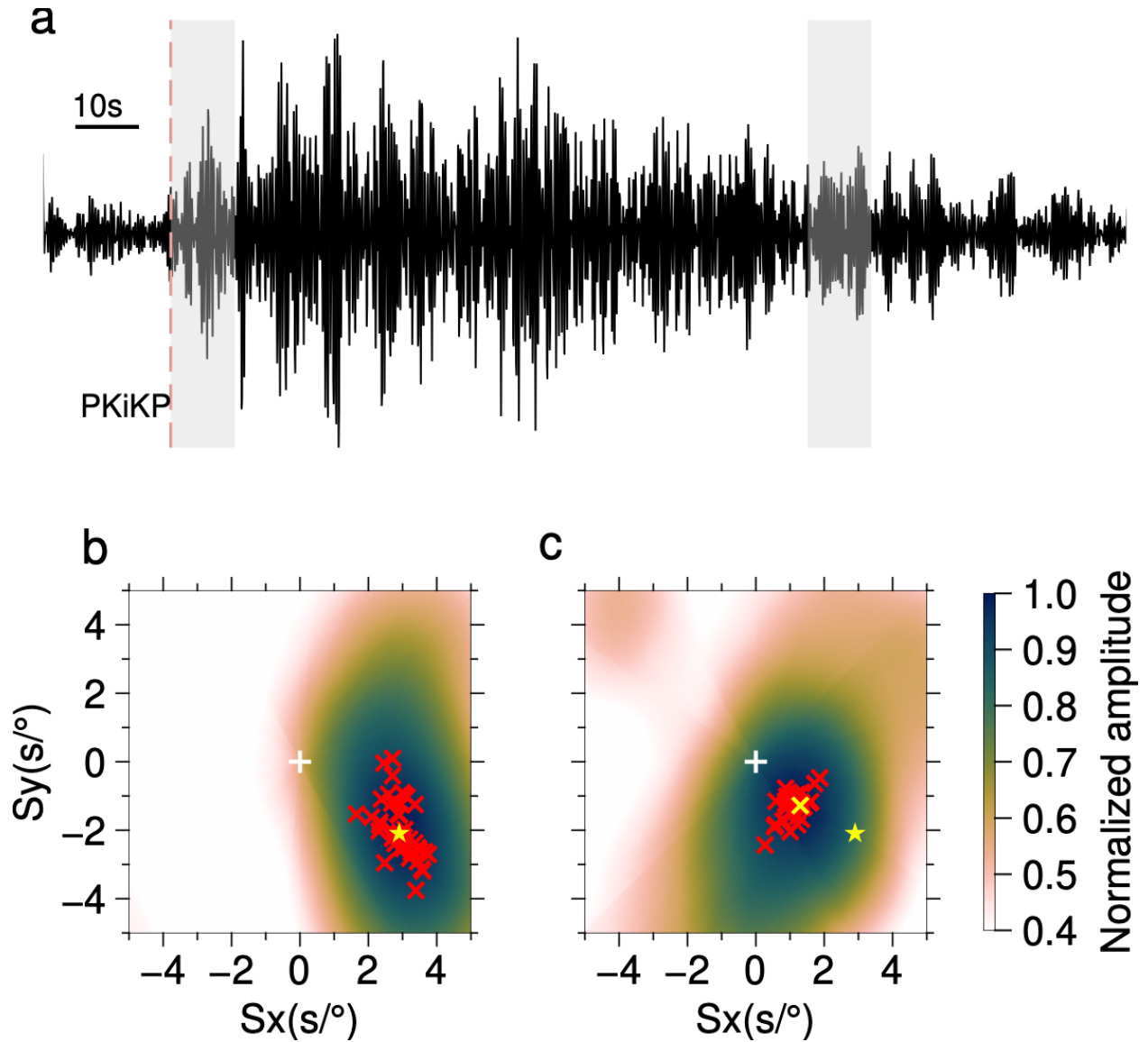


**Figure R7. Slowness vector analysis of ICS energy at ILAR.** (a) Linear beam of *PKiKP* coda waves. This is the same earthquake shown in Fig. 1b. The pink line marks the direct *PKiKP* phase. Gray windows mark the signals used in (b) and (c). (b) Slowness analysis for the direct *PKiKP* phase (first gray window in (a)). Yellow stars mark the optimal slowness using all ILAR array elements. The white cross is the origin (vertical incidence). The red crosses represent 50 bootstrap solutions. (c) Slowness analysis for the late coda waves (second gray window in (a)). The yellow cross is the optimal slowness for the coda waves in the second window. The yellow star is the same as in (b). The red crosses represent bootstrap solutions.

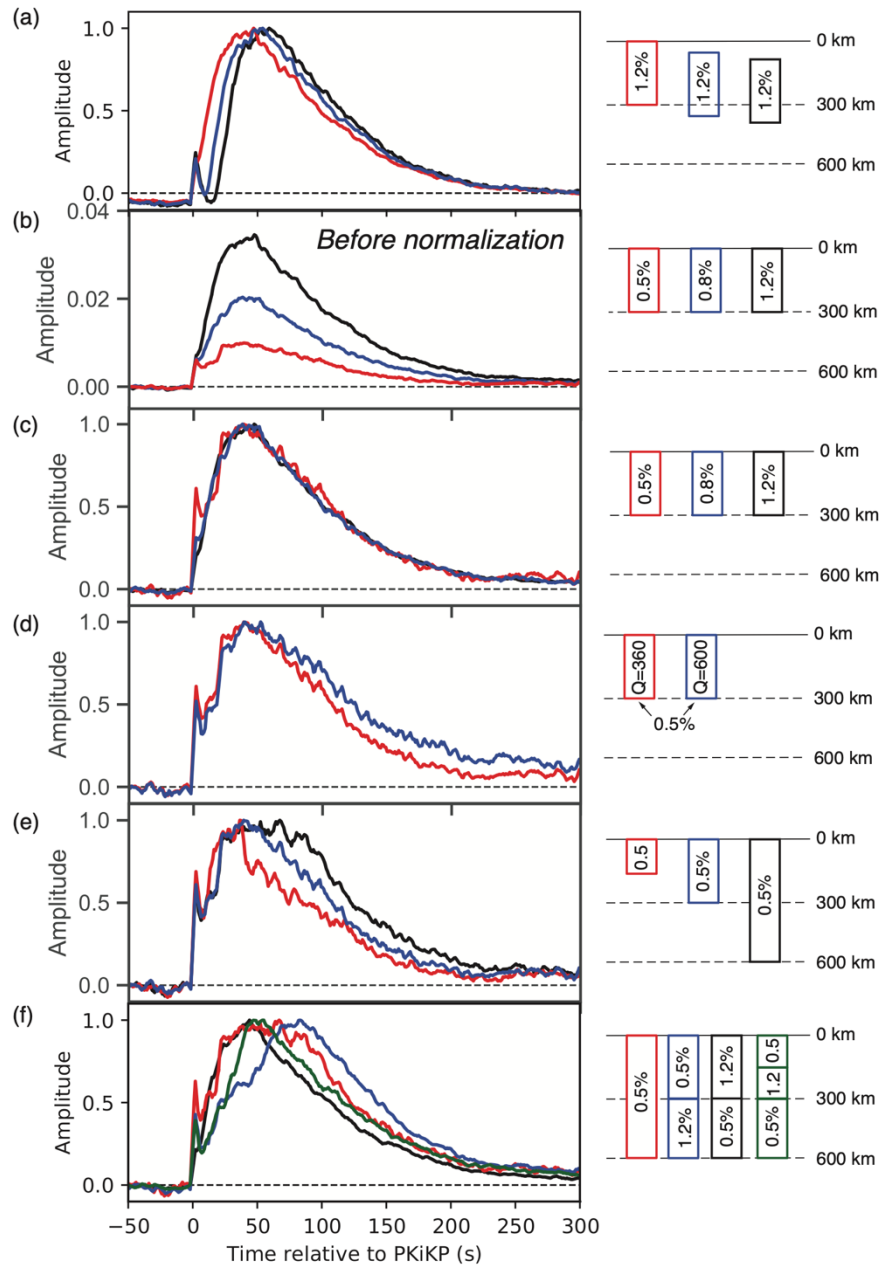


**Figure R8. Predicted PKiKP coda envelopes using various ICS models shown on the right.** This figure is modified from Wu et al. (2022). The curves are stacks of synthetic envelopes at  $50^{\circ}$ – $75^{\circ}$ . Colors are matched between the coda envelope synthetics on the left and the scattering model on the right. The percentage numbers in the models indicate  $\varepsilon$ . The  $Q$  in (d) indicates intrinsic attenuation. The depths shown in the models refer to depth below the ICB.

## Reviewer Reports on the First Revision:

Referee #1:

{ Please see Page No.25 }

\*\*\*\*\*

Referee #2:

The authors have provided detailed and thoughtful replies to the reviewers' comments, performing additional analysis in response to some of the queries. Major additions include: an investigation into the sensitivity of the coda both in terms of sampling location and also to heterogeneous models; redid the geographic regional analysis for larger regions statistical tests exploring hemispherical structure. The authors also provided careful explanations in their responses and the manuscript as to why some of the reviewer suggestions could not be implemented.

I think that these changes have strengthened the paper, and thus I recommend publication following a few minor edits.

Minor comments by line number:

126-127: please define qEH and qWH latitudes used

130: include approximate size of Fresnel zone

170-173: these lines have updated diffusivity values compared to the finer scale regional model. It would be useful for readers to include the original results (perhaps as supplemental material), to highlight the effects of averaging and importance of considering sensitivity.

**Review – Revision:** “*Globally Enhanced Inner Core Fine-Scale Heterogeneity toward the Earth's Center*” by Pang and colleagues

The authors have widely revised the original submission of this manuscript to include the suggestions by two reviewers.

My main points in the review of the original manuscript where the (i) sensitivity of the ICS for structure in the inner core, (ii) the resolution of the radial and lateral change in inner core heterogeneity and (iii) the trade-off between parameters, and the interpretation of the heterogeneity in terms of trapped fluid. Some of the same (or similar points) have also been raised by the second reviewer.

In reply to (i) the authors show the contributions of each volume element in the inner core to the PKiKP coda for a specific distance and also added a new section on the sensitivity of ICS to inner core structure to the Methods section. The authors show clearly that we can expect ICS to arrive roughly along the GCP. The authors also discuss in the reply to reviewers what slowness resolution to expect from the used arrays. The authors added an extended data figure looking into the slowness resolution using a bootstrapping approach. Overall, I am satisfied that the authors have addressed my points and have edited the manuscript to discuss these issues.

In response to (ii) the authors have added a discussion of the sensitivity of ICS to inner core heterogeneity to the reply letter and have added some discussion to the manuscript. Some of these issues are also discussed in the now published JGR paper that is referenced now. In response to the comments by both reviewers the authors also performed statistical tests on the significance of the detected sharp boundaries concluding that evidence for both hemispherical and radial boundaries are not significant. These are now mentioned in the text with the specific tests and significance being discussed in the Methods section. Overall, I am satisfied that the authors have addressed my concern here although it might be good to mention the significance in the text directly.

My final point the authors address only in the reply letter. But I can follow their argument that there are no studies investigating the scales of melt pockets in the inner core and uncertainties could be similar to the estimates for inner core grain size (which might actually be easier to determine). I am happy for that discussion to be not included in the manuscript.

The authors have also responded to the minor points I have raised and have edited the manuscript in places. One comment regarding the mantle heterogeneity structure might be that these models are not well constrained. The model used by the authors has been derived using global stacks of data. With known lateral changes in heterogeneity structure these models are likely underpredicting local heterogeneity. But without clear maps of heterogeneity structure in the mantle the approach chosen by the authors seems valid. They could comment on the lateral changes and point to relevant literature there.

Reviewer 2 raised some similar points which were answered appropriately. Reviewer 2 also added some more queries regarding the geodynamic modeling which is somewhat outside of my expertise. Nonetheless, it seems to me that the authors have answered the queries in depth and have changed the manuscript, both text and figures, appropriately if necessary. A crucial point is the limitation to 1D (radial) growth models, but it seems that the relevant theory does not exist. So, the authors do what is currently possible in this area.

## Author Rebuttals to First Revision:

### Comments from Referee #1

The authors have widely revised the original submission of this manuscript to include the suggestions by two reviewers.

My main points in the review of the original manuscript where the (i) sensitivity of the ICS for structure in the inner core, (ii) the resolution of the radial and lateral change in inner core heterogeneity and (iii) the trade-off between parameters, and the interpretation of the heterogeneity in terms of trapped fluid. Some of the same (or similar points) have also been raised by the second reviewer.

We thank Referee #1 for their thoughtful and thorough reviews. Their comments have been invaluable for improving our manuscript.

In reply to (i) the authors show the contributions of each volume element in the inner core to the PKiKP coda for a specific distance and also added a new section on the sensitivity of ICS to inner core structure to the Methods section. The authors show clearly that we can expect ICS to arrive roughly along the GCP. The authors also discuss in the reply to reviewers what slowness resolution to expect from the used arrays. The authors added an extended data figure looking into the slowness resolution using a bootstrapping approach. Overall, I am satisfied that the authors have addressed my points and have edited the manuscript to discuss these issues.

We are pleased that we were able to address the concerns of Referee #1.

In response to (ii) the authors have added a discussion of the sensitivity of ICS to inner core heterogeneity to the reply letter and have added some discussion to the manuscript. Some of these issues are also discussed in the now published JGR paper that is referenced now. In response to the comments by both reviewers the authors also performed statistical tests on the significance of the detected sharp boundaries concluding that evidence for both hemispherical and radial boundaries are not significant. These are now mentioned in the text with the specific tests and significance being discussed in the Methods section. Overall, I am satisfied that the authors have addressed my concern here although it might be good to mention the significance in the text directly.

We mention in the main text that we find no statistically significant difference between the scattering structure of the two quasi-hemispheres. The text can be found on *Lines 124–125* of the clean version of the revised manuscript. This point is reinforced on *Lines 161–163*.

My final point the authors address only in the reply letter. But I can follow their argument that there are no studies investigating the scales of melt pockets in the inner core and uncertainties could be similar to the estimates for inner core grain size (which might actually be easier to determine). I am happy for that discussion to be no included in the manuscript.

The authors have also responded to the minor points I have raised and have edited the manuscript in places. One comment regarding the mantle heterogeneity structure might be that these models are not well constrained. The model used by the authors have been derived using global stacks of data. With known lateral changes in heterogeneity structure these models are likely underpredicting local heterogeneity. But without clear maps of heterogeneity structure in the mantle the approach chosen by the authors seems valid. They could comment on the lateral changes and point to relevant literature there.

We added comments and relevant literature to point out that the mantle scattering model was from global stacks without considering the regional variations on *Lines 477–479*. Tests with different mantle scattering models show that the mantle scattering has little effect on the shape of PKiKP coda, which is consistent with the cross-path observations shown at Extended Fig. 3. This point is added on *Lines 479–481*.

Reviewer 2 raised some similar points which were answered appropriately. Reviewer 2 also added some more queries regarding the geodynamic modeling which is somewhat outside of my expertise. Nonetheless, it seems to me that the authors have answered the queries in depth and have changed the manuscript, both text and figures, appropriately if necessary. A crucial point is the limitation to 1D (radial) growth models, but it seems that the relevant theory does not exist. So the authors do what is currently possible in this area. We appreciate the comments of Referee #1.

### **Comments from Referee #2**

The authors have provided detailed and thoughtful replies to reviewers' comments, performing additional analysis in response to some of the queries. Major additions include: an investigation into the sensitivity to the coda both in terms of sampling location and also to heterogeneous models; redid the geographic regional analysis for larger regions statistical tests exploring hemispherical structure. The authors also provided careful explanations in their responses and the manuscript as to why some of the reviewer suggestions could not be implemented.

I think that these changes have strengthened the paper, and thus I recommend publication following a few minor edits.

We thank Referee #2 for their helpful and constructive comments, which strengthened our manuscript significantly.

Minor comments by line number:

**126-127: please define qEH and qWH latitudes used**

We are a little confused about the word “latitudes” and assume Referee #2 is referring to the longitudes by which the quasi-hemispheres are defined. We include the relevant longitudes for qEH and qWH on *Lines 123–124* in the revision.

**130: include approximate size of Fresnel zone**

We included approximate size of Fresnel zone on *Line 128*.

**170-173: these lines have updated diffusivity values compared to the finer scale regional model. It would be useful for readers to include the original results (perhaps as supplemental material), to highlight the effects of averaging and importance of considering sensitivity.**

The original results before averaging are shown in Figure 3. Following the editorial comments, we provide Source Data for our figures. Thus, the information requested by Referee #2 here will be uploaded as the Source Data of Figure 3.