Rebuttal Letter

Response to Reviewers:

We appreciate the very helpful and constructive feedback provided and have addressed all reviewer comments. Reviewers' comments are in normal text and our response are in blue. Line numbers refer to the red-lined manuscript. We feel the reviewers' comments were constructive and we addressed all comments below and thank the reviewers for a very thorough analysis of our paper. The comments helped us greatly improve the manuscript. We have added acknowledgement to the reviewers.

Reviewer 1:

Shear-Wave Radiation Patterns from Explosive and 1 Earthquake Sources in Scattering, Heterogeneous Media
by Peter Nelson and Neala Creasy
Seismica

Reviewer: Carl Tape, University of Alaska Fairbanks, ctape@alaska.edu

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SUMMARY

This paper uses 2D elastic wavefield simulations to explore the influence of small-scale heterogeneity on the observed seismic radiation, notably the S wave generated from an explosion source. I am fascinated by the demonstrations, which build upon the results of Burgos et al. (2016). The authors carefully test their numerical setup and carefully design their numerical experiments. (There are dozens of dials to turn, so care is needed.) This is formidable study, given that a wide range of 2D and 3D velocity models are considered, with very small-scale heterogeneities, as well as anisotropy. I like how the emphasis is on 2D, which can be used to make most of the main points. To me, several of the results are not intuitive, yet I can believe them, as presented (both here and in Burgos). I do not have expertise in scattering theory, and I would be interested to hear a scattering expert's assessment of this study. I suspect that several findings would be theoretically expected. In that sense, there could be some stronger theoretical framework and connections, yet this will lengthen the paper. Overall, I think this is a difficult and excellent study, enhanced by a thoughtful Discussion.

I provide many minor comments, none of which (I think) requires performing additional simulations.

We thank the reviewer for their feedback. We agree more theoretical work would be a great contribution. We intend to publish a theoretical study next as we have applied for additional funding to explore this scattering theoretically.

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MAIN POINTS

1. I'm wondering how much of a 3D displacement field can be extracted from a 2D simulation. The focus here is on the P-SV simulations. Does the SH wavefield provide anything useful, especially regarding a transverse component.

This is not something we explored in 2D simulations. We are not sure how much 2D SH simulations would be useful to learn about the effects of near source heterogeneities have on explosive sources because the simulations can only handle SH-SH scattering and there is not much (or any) initial SH energy from an explosion.

2. On a related note, Figure 3 states "Vertical 2D displacement" -- so if the source and receiver are in the horizontal dimensions (map view of the simulation), then is this component normal to the viewing page? It might help to clearly distinguish coordinate systems of the simulation versus components shown in wavefields and seismograms.

We have made the text clearer. There are only two components: X and Z positions. Therefore, to be clearer we have clarified in the text that the vertical direction is really the Z direction (up-down), and the horizontal direction is the X direction (left-right). Therefore, the radial component is calculated by rotating the seismograms, so the seismograms are oriented towards the source (parallel to the back azimuth) and the “transverse” component is perpendicular to the back azimuth. See new lines 114-117.

3. Can the authors provide a couple theory-based sentences (intro, results, discussion or elsewhere) about how this really works? You have 11m pixel-like heterogeneities influencing a wave having a 4.5 km wavelength. Is there anything to help a reader's intuition?

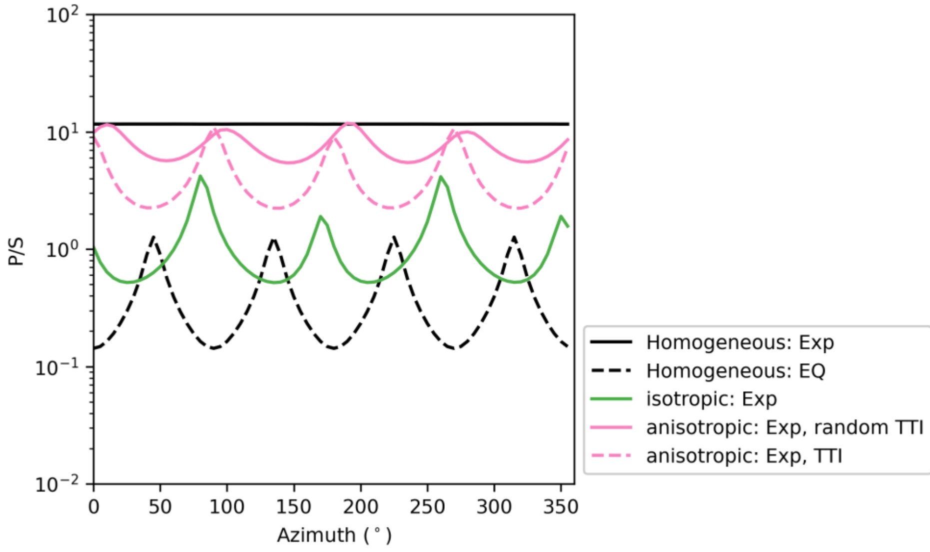
We have added a sentence expanding on how this type of scattering works based on theoretical work at line 60 and line 67.

4. L133 Figure 3d. This is a very interesting finding, that the P wavefield is enhanced for the double-couple source. Doesn't that imply that P wave studies (notably of amplitudes) will be impacted be heterogeneity?

Yes, we agree that the P wavefield could also be influenced by scattering in the near field. Burgos et al., (2016) found the same result.

5. Section 3.3. It might help to set up the anisotropy section more generally. The key perturbations to consider in each pixel are the direction of the fast axis and the strength of the anisotropy. Both of these could be randomized. Instead, you simply things by choosing two possibly fast directions and fixing the strength at 40%. This kind of introduction would help convey the choices at hand.

We have run a subset of simulations with this approach, where we randomly vary the strength of anisotropy and axis of symmetry. The results are below. The results are still somewhat similar, but P/S ratios are higher than the simple approach we did. This is likely because we randomly generated a 1 km region of anisotropic heterogeneities. Another random iteration would likely result in similar results, but the overall P/S ratio would most likely change.



6. Section 3.3. Can you provide some minimal information about anisotropy in 2D? I saw this in the supplemental materials:
   # anisotropic: model_number 2 rho c11 c13 c15 c33 c35 c55 c12 c23 c25 0 QKappa Qmu
   So are 9 parameters needed? In 3D there are 21 parameters for the symmetric 6x6 Voigt representation. How many are needed in 2D? In 3D, TTI requires 7 (5 + 2 angles), HTI requires 6 (5 + 1 angle), VTI requires 5 (5 + 0 angles). What are these analogs in 2D?

In SPECFEM2D, a general 2D case is used for the elastic tensors, where only c11, c13, c15, c33, c35, and c55 matter (total of 6 parameters) as the y direction (notation of 2, 6) is not used. See section 4.3 in the SPECFEM2D manual. The Voigt representation is symmetric as well so C13=C31 (see line 252).

7. Section 3.4. How do the authors know that the 3D simulation results are numerically accurate?

For 3D, we ran a simulation with the same mesh parameterization that had no heterogeneity near the source. This simulation's frequency was well below the empirical resolution of the mesh, and we saw no signs of numerical instability. When we added heterogeneity the R and Z components were basically the same as the homogenous case, so we believe it is unlikely if the simulation was numerically inaccurate as it would only affect the T component by producing a clear S-wave at the right time.

8. Have they performed simulations with the finest-scale velocity model, but using a finer mesh, to show that the 1 Hz wavefield is resolved?

Yes, we have performed a mesh convergence test in Figure 5. We do not observe any numerical dispersion in either the homogenous or heterogenous cases and we are above the 4.5 element per wavelength (35) empirical threshold, so we are confident that we are properly resolving the wavefield.

9. What is the size of the simulation domain?

At line 102, the domain for the 2D simulations is 100 by 100 km. At line 123, the domain of the 3D simulations is 9 by 9 degrees with a depth of 220 km.

10. Arc-length of the two sides, plus depth?

We assume the reviewer is referring to the 3D simulations. At line 122, the domain of the 3D simulations is 9 by 9 degrees in north and east directions with a depth of 220 km.

11. What is the length scale of each pixel-cube?

In 2D, we vary the mesh dimensions, but we mainly use element sizes of ~200 meters at line 104. In 3D, we use 768 elements with an average element size of 1.3 km at line 123. Each pixel, however, represents each GLL point, which is roughly 35 meters.

12. It looks like the simulation domain is 1000 km or so in width.

In 3D, yes, 9 degree is roughly 9*111km = ~1,000 km. In 2D, the domain is 100 by 100 km, as indicated above (line 102).

13. And all the pixel-cubes are inside a 1 km3 region?

Yes.

14. What is the length scale of a pixel-cube?

The pixel-cubes (anomalies) are each GLL point, which is about 35 meters.

15. What is the minimum GLL gridpoint spacing in your mesh?

The output files for SPECFEM3D-globe do not give a minimum, but the average GLL grid point spacing is 325 meters.

<u>Here are some additional mesh details for SPECFEM3D_GLOBE simulations:</u>

! spectral elements along a great circle = 30720

! GLL points along a great circle = 122880

! average distance between points **in** degrees = 2.9296875E-3

! average distance between points **in** km = 0.325766385

! average size of a spectral element **in** km = 1.30306554

16. How may CPU-hours are needed for the simulation?

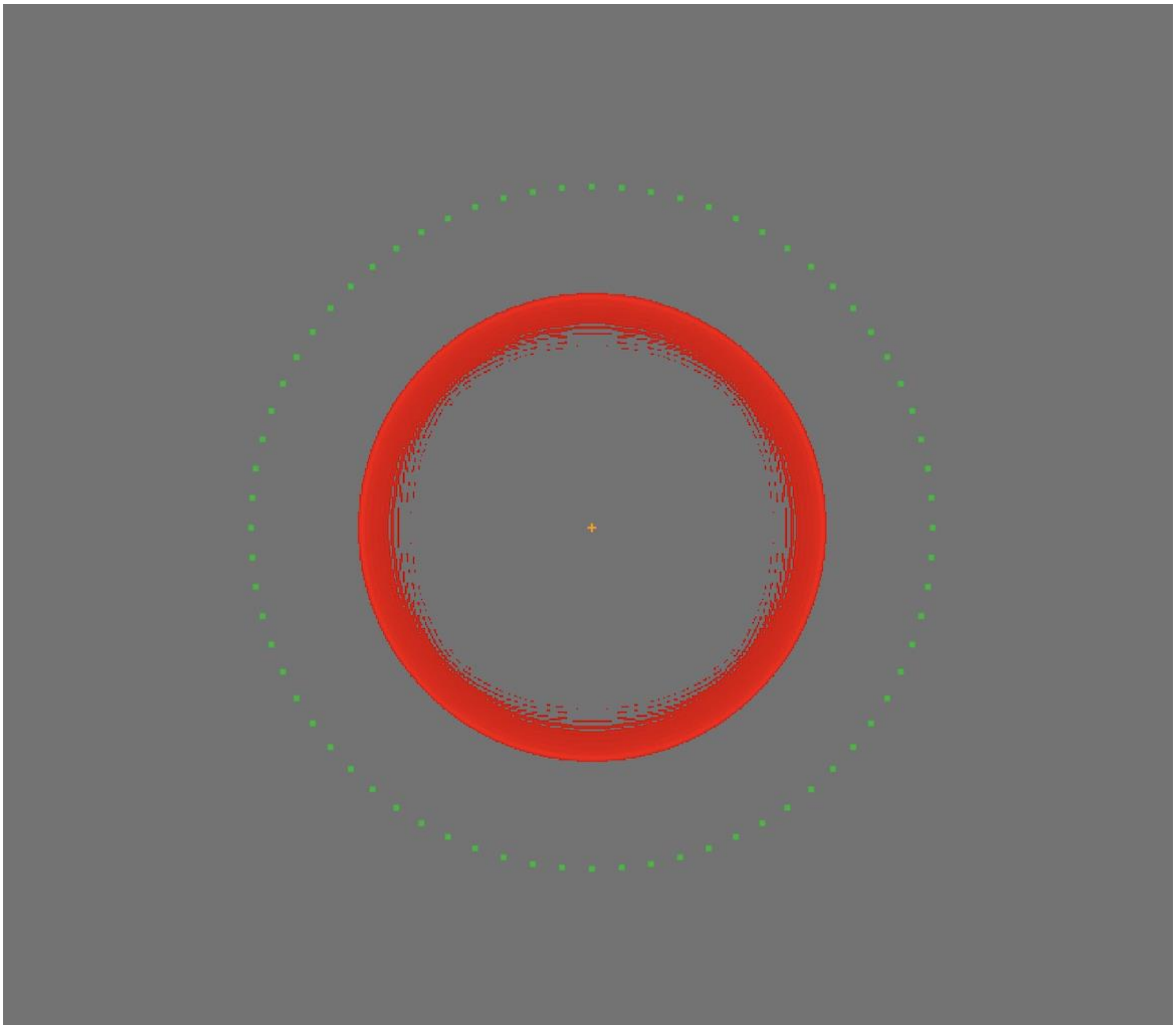
The number of CPU hours will vary based on the length of the seismograms and the computing infrastructure used. We choose not to place these numbers in the manuscript because it depends on many factors, but for us on our computing infrastructure on LANL's Chicoma (AMD Rome EPYC 7H12 processors), it requires on average: ~9,000 CPU hours for one 3D simulation for a 3.5-minute seismogram (4 hours computational time on 2,304 cores) and <1 CPU hour for one 2D simulation for a 13 second seismogram.

17. Related: Is it really a cube or is its upper and lower surface part of a spherical shell? I have not heard of anyone using the global code for such small-scale regions before. I glanced at the supplemental information in https://data.mendeley.com/datasets/54kkx44886/1, which provides the source, stations, and Parameter file, but I could not tell how the 3D model was implemented.

We hardcoded the model perturbations in when the code assigns Vp, Vs, and rho values to each GLL point, i.e., if the GLL point is located <1km away from source, we perturb the mesh parameters. The elements are part of a spherical shell, but our anomaly is as close to a cube as the code allows.

18. Related: Can numerical dispersion within a homogeneous model (e.g., velocity too slow for the gridpoints in the mesh) generate an S wavefield for an isotropic source?

To make sure our simulations were correct, we also explored changing only a single lamé parameter, such as in the Supporting Information (Figure S2). Only changes in μ produced S-waves. Changes in density or λ did not produce any large or noticeable changes in S-wave generation. Here is a snapshot of a simulation where we reduced the S-wave velocity by 10 times and the number of events by 4 times but kept everything else the same. You can see the P-wave front is a little noisy but there is no S-wavefront.



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MAIN POINTS - FIGURES

1. Figure 4. Please show (a) as the epicenter in the center [a re-display of 3b]. This way, the other three will serve as comparisons.

Done.

2. Given the emphasis of Burgos et al. (2016), can the authors comment on homogenization? Perhaps, if possible, they could get some professional feedback from the likes of Capdeville. Or provide their own perspectives on how homogenization could be applied to compute wavefields at lower numerical costs than meshing the pixels. Such an approach could be essential for the 3D realm.

We have added a paragraph in the discussion about homogenization. See line 480.

3. Figure 5. My understanding is that this is primarily a numerical test. If so, I suggest shifting it to an appendix, since most readers will be content to trust a sentence that says "we checked different element sizes to verify the accuracy of our P/S ratios."

We moved Figure 5 to the supporting information (Figure S3).

4. The usage of the word "mesh" in L156 ("mesh convergence test") pertains to the FEM mesh, right? And not to the model of Vp, Vs, rho values? (If not, please clarify.)

The mesh we refer to is the spectral element mesh, also the element spacing. We maintain the same Vp, Vs, and rho values.

5. Figure 6. After 10 minutes, I still don't think I know what was done here, which is why I'm asking the authors to include 8 thumbnail figures (including 0m) that show the models used. Is "anomaly" the same as "region of small-scale heterogeneities" and the same as "patch"? (Maybe stick with one word.) What is a 60 km anomaly in a 50 km simulation domain? Does each model have the same number of heterogeneity pixels? If you wanted to be fancy, you could draw the bounding box of each thumbnail with the line style of each type, then not need the legend at all (if you labeled each thumbnail with 0m, 100m, etc).

Thank you for this comment. We have expanded Figure 6 (now Figure 5) greatly to include more examples of what we mean and added more text at line 212. The “anomaly” is the same as “region of small-scale heterogeneities” and “patch.” The simulation is 100 by 100 km. We have corrected the text. We have checked our whole document and made our reference consistent to heterogenous region (small-scale heterogeneities). As the heterogeneous regions becomes smaller, below ~300 m, we see weaker S-wave generation. We are just expanding or reducing the original 1 km patch to smaller or larger scales. The pattern of alternating percent change in Vp, Vs, and rho are the same as well as the element size. Instead of randomizing the location of the variation, it is a checkerboard pattern.

6. Figure 7c. Please show thumbnails of the models (0m, 150m, ..., 350m).

We have included the wavefield snapshots in a new supplement Figure S5. Figure 7 is now Figure 6.

7. L222. I left Figure 7 thinking that source frequency matters, at least from the sense of tracking the number of wavelengths that has been propagated by a wavefront. Then: "Source frequency weakly influences the P- and S-wavefields we observe..."

“Weakly” may have been the wrong word choice here as we agree there is an effect. We have removed that modifier.

8. Figure 9. I do not see why these scenarios are labeled as HTI and VTI. I can see how H can be taken to be one direction (left-right) and V the other (up-down). But in thinking about a 3D medium, I imaging the plane of the page (with the simulation) as the horizontal plane, and both of these examples would be HTI: 9a would be, say, fast direction north, while 9b would be fast direction east.

Yes, we realize how this is confusing. We have modified the text to be more explicit on the directions of the axis of symmetry and fast directions (line 274).

9. Figure 10. Please show thumbnails of the model. Is it the case that the fast-axis of each pixel is either 0 or 90 deg azimuth?

The 1 km region of small-scale heterogeneities are a checkerboard, where the fast direction is either vertical or horizontal. The thumbnail is just a checkerboard. The elastic tensor components were added to the supporting information (Table S1).

10. "In this test, we alternate between HTI and VTI, ..." I'm not sure what this means. A thumbnail would probably answer it.

What we mean is that the symmetry axis of transverse isotropy varies from vertical (going upward or north) to horizontal (pointing east-west). We provide the elastic tensors used and add this sentence to the text: “In this test, we alternate between the fast direction being horizontal and being vertical (similar to vertical and horizontal transverse isotropy), where the average isotropic velocity is identical to the background medium” at lines (274-275).

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MISC POINTS

1. Perhaps for Seismica: The blended formatting of captions and main text, often with the captions page-split from figures, hindered my reviewing.

We have placed the figures at the end of the text.

2. L35. Delete etc and list other possibilities (if they exist).

Done.

3. L49. Delete etc and list other possibilities (if they exist).

Done.

4. Figure 1 caption. Either in the caption or attached to a beachball, indicate the convention for azimuth, especially since it differs from mathematical convention. At a minimum, say that azimuth = 0 points north.

Thank you. We have added the following to clarify: “Azimuth of zero degrees is pointing upward or north relative to the beach balls.”

5. L52. "In combination with previous work, we intend to" I would say "Building upon the results from Burgos et al. (2016), we explore how small-scale" To me, the core work is Burgos, not the other effects (and references) listed. The authors do state this up front at the start of the Discussion.

Done.

6. L75. commonly not been studied --> not been thoroughly studied

Done.

7. L80. Delete "In this study"

Done.

8. L102 and L297. Probably it makes to sense to cite Komatitsch and Tromp (2002a) here, for your purposes, which are a 1D Earth model (plus source heterogeneities).

Done.

9. L105. completely lacking --> absent

Done.

10. Figure 2. What is the size of the pixels within the 1 km anomaly? It looks like it's about 30 x 30, which would imply pixels of 30 m x 30 m.

Yes, this is correct. See previous comments.

11. "Colors (black and light gray) represents random seismic velocity and density variations of +/- 25% at each GLL point"

Done

12. Random in the sense of what? So are there approximately half the pixels as +25% and half as -25%? It would be good to be more explicit about this, whether or not the details matter to the simulation results.

The random is the location of either +25% or -25%. We have updated the text and Figure 2 caption.

13. L115. Probably N-m would make more sense than dyne-cm, especially since you list N-m in L132. We computational seismologists are collectively still getting plagued by 10^7 coding factor issues for things like this. But in the long run, we'll all be using N-m.

The change in units is mainly due to SPECFEM2D and SPECFEM3D_GLOBE requiring different units. We have added the equivalent Nm when dyne-cm are used so that units are consistent for reproducibility. See info from manual here:

From SPECFEM Manual:

`Mxx,Mzz,Mxz` Moment tensor components (valid only for moment tensor sources, `source_type = 2`). Note that the units for the components of a moment tensor source are different in SPECFEM2D and in SPECFEM3D:

SPECFEM3D: in SPECFEM3D the moment tensor components are in dyne*cm

SPECFEM2D: in SPECFEM2D the moment tensor components are in N*m

14. L115. "To measure the S-wave polarization in subsequent sections, we calculate the 2D PCA (Principal Component Analysis) for the windowed radial and transverse components, similar to the approaches in other studies (e.g., Li et al., 2021). "How does PCA differ from SVD in this application?

Either application can be used, but PCA is a subset of SVD, where we are only interested in the polarization (the principal components). In our case, we are using PCA from the scikit Python library, which centers the data. As a result, SVD is identical to PCA.

15. Figure 3ab, Figure 9ab. Why not plot a solid beachball at the upper left?

The explosion beachballs have been added to Figures 3 and 9.

16. L123. "...can produced an S-Wave, in agreement with the findings of Burgos et al. (2016)."It's good to identify where you're corroborating previous work versus demonstrating altogether new findings.

Done.

17. L131. "...a double couple earthquake source that..."

Done

18. L153. need commas around "specifically changes in rigidity"

Done

19. L176. "The transverse component is the component orthogonal to the radial component." Not precise. It might help to specify a righthanded coordinate convention, like R-T-Z (or whatever it is), to reinforce the direction of T.

We have corrected the sentence to: The transverse component is the component orthogonal to the radial component (ZRT coordinates, right-handed coordinate convention).

20. L300. "could greatly increase" --> "is expected to increase"

Done

21. L453. "our anisotropic heterogeneities are on similar to single-crystal"

Done

END

<u>Reviewer 2:</u>

Review: Shear-Wave Radiation Patterns from Explosive and Earthquake Sources in Scattering, Heterogenous Media

Summary: This paper presents synthetic simulations looking at the modification of waveforms from isotropic sources in the presence of near-source heterogeneity. Such work is relevant in a number of areas of seismology, most pertinently nuclear monitoring. Overall comments: The idea behind this paper is sound. The authors have made an effort to be systematic and thorough. However, I have many concerns about the validity/ correctness of the simulations. In some cases, this may be down to unclear presentation or discussion which could be worded better, but I am left unconvinced by the end of the manuscript and there are lots of things which seem incorrect. I suggest that this manuscript be re-submitted once the authors have had a chance to re-structure their simulations to address the points made here.

Other comments applicable to the entire paper:

1. Many of the figures are very challenging to understand, with many overlapping curves and lines plotted in such a way that I cannot see the data properly. In particular, Figures 7, 8, 11, and S3 all contain features that are not resolvable. I assume that the colours have also been chosen from a colour cycler, but I would encourage the authors to also run the plots through a colourblindness filter for accessibility reasons as there are lots of reds/greens/browns.

We switched to a different color palette and added more line variants to be safe.

2. There are many typos which are significant enough to affect the readability of the paper. I have highlighted some of the major ones below before I stopped keeping track, a thorough proofread is needed

We have addressed your suggestions below and have reviewed the paper again for clarity.

Specific major comments:

Introduction:

1. The problem of source discrimination based on P/S ratios from supposedly isotropic sources is also relevant to impact processes. The authors may like to consider adding references to this effect (by no means required, but could be useful to highlight to new audiences). For example, I would suggest Daubar et al 2020, A new crater near InSight: implications for seismic impact detectability on Mars and Fernando et al 2021, Listening for the Landing: seismic detections of Perseverance's arrival at Mars using InSight. There may be others more relevant to a terrestrial audience, too.

We agree with these suggestions. We have added this additional source type at line 48.

2. L 33: "does not generate SH (transverse component seismic energy)". I would be clearer about what an explosion does and does not produce, and the ways in which this happens. By this I mean specifying that a pure, idealised explosion is isotropic (with no deviatoric component to the moment tensor and no S wave), and that in a 1D model all the energy is confined to the source-receiver plane. The conversion of P to S phases at far-field interfaces should also be mentioned.

We have expanded the definition at lines 36-38.

We have added the following sentences:

"These idealized explosions contain energy only within the source-receiver plane. The only S-waves that are generated are from the conversion of P due to scattering and discontinuities within the Earth."

3. L 48: My understanding is that the near-field effects of non-linear wave propagation through a solid medium can also produce S waves (e.g. Vorobiev, 2022 On various mechanisms of shear wave generation from underground chemical explosions in hard rocks). This should be mentioned, as the near-field effects are not only due to "smallscale heterogeneity" - especially as the authors use only a linear source.

This paper is a great resource. We have expanded our list at line 56.

4. L 76: “The advantage using [sic]” - the authors imply that ‘speculative corrections’ are required to make P/S ratios useful as discriminators, but these corrections are not mentioned anywhere else in the paper (nor do they seem to apply them anywhere in their own results?). Also, on the previous page (L 52) they indicate that they are going to use P/S ratios, but this is undermined as a useful exercise by L76. Some resolution to this conundrum is required.

Since we are using a homogenous model (constant attenuation), we do not need to apply a correction for path effects when calculating P/S ratios. We added a sentence at lines 50-52 to clarify this point.

5. There are a number of other studies which have attempted to account for local-scale heterogeneities in simulations for nuclear monitoring (or related) purposes using simulations. These should be at the very least described, and ideally the authors should demonstrate an improvement/comparison/difference to work already conducted. For example, Pienkowska et al 2020 (High-frequency global wavefields for local 3D structures by wavefield injection and extrapolation) looked specifically at this issue. I would also suggest Leng et al 2020 (3-D scattering of elastic waves by small-scale heterogeneities in the Earth’s mantle) and references/papers citing this for greater context on the computational side of things. These are only two, there are many more.

While these papers are useful, we are focused on heterogeneities and scattering in the near-source field, while these examples are more relevant for the far-field. We included these references in the introduction and emphasize how our study is different.

6. Similarly, the authors might like to consider referencing work on microseism modelling which is related (showing how transverse components can be excited by isotropic pressure fields). Such methods are not unrelated to those discussed in this paper (e.g. Gualtieri et al, 2020; The origin of secondary microseism Love waves).

We have added the citation at line 60.

Numerical methods:

7. L 96-97: The figures quoted for material parameters are given to vastly different degrees of significance, which is confusing. Similarly, I believe the units of the density are incorrect.

At the top of the mantle, density is slightly higher (based on PREM) that what we used, ranging from 3.36 – 4 g/cm3. They are just approximate values for the mantle, and we have adjusted the text to emphasize this. Mantle values allow us to save on computational time for the given frequencies.

8. I believe that the units for mesh dimensions should be consistent and comparable, i.e. either all in degrees or all in kilometres - ideally the latter unless the latitude is also stated.

We have added equivalent distances in km (line 123).

9. The switching between coordinate frames and units is confusing (and does not seem necessary anyhow)? In some places earthquake moment tensors are given in (x,y,z) and Nm and elsewhere in (r,t,p) and dyne-cm.

We switch because SPECFEM2D is in cartesian coordinates, while SPECFEM_GLOBE is in spherical coordinates. The source files also have different units. We provide these units, so it is reproducible since these are the input units for the waveform solvers.

10. L 111: “colours (black and light gray) represents [sic]…” - does this mean that at each point, each cell must be either + or - 25% to vp/vs/rho all together; i.e. do all the changes have the same sign? Why was the value of 25% chosen, rather than a spectrum? I can see advantages to doing this, but it should be discussed, as it is much stronger than the degree of perturbations we would expect in an average underground site (assuming no cavity, etc).

We found in our work that using a spectrum (such as van Karman or Gaussian) does not significantly change out results, where using a checkerboard pattern is the simplest approach to demonstrate the main points. We have added how a Gaussian spectrum results in similar results. See new additions to Figure 3 and a new Figure S1.

Also, 25% was just chosen somewhat arbitrarily since Burgos et al., explored the strength of anomalies. Additionally, we found 25% to be reasonable based on natural variation in crustal rocks (https://gpg.geosci.xyz/content/physical_properties/tables/seismic_velocity.html) but we do agree that this value is larger on average that the average RMS values reported for crustal rocks (10%) (Sato et al., 2019). Therefore, we focused on a singular value. However, the value we use of 25% is not equivalent to a 25% RMS value.

We have added these comments to our paper at lines 144-150.

11. The bounding domain is 50 x 50 km (i.e. 25km from source to the nearest edge). With vp > 7 km/s, and synthetics filtered to include energy out to 100s, there is barely a fraction of the longest seismic wavelength fitted into the simulation domain. In general, I think this should be avoided as then the long wavelengths are sampling the edge PML. I suggest re-filtering that the longest periods are contained purely within the box.

We have changed the filter to 0.14 Hz – 20 Hz (equivalent to a 7s [50 km wavelength] – 0.05 s period filter). There is no large difference in our waveforms.

12. Fig 2: I assume that each cell in the “anomaly” has a random change in properties which is unrelated to the cells around it? That should be specified - in reality, these sorts of things will have some characteristic scale of course.

In this case, each cell is plus or minus 25% but each GLL points is randomly assigned a +25% or -25%. We updated Figure 2 caption.

Results:

13. Fig 3: I am afraid that I do not really understand what is going on here. If I follow correctly, Fig 3a and 3b are identical other than that the P wave has propagated through some randomly scattering medium in the near-source region. I agree that this should produce an S wave, but I am struggling to understand the radiation pattern which you have shown. For an isotropic source, I see no preferred orientation axis. Why does random scatter produce a displacement pattern which has such strong symmetries (as in Fig 3b)? Should the P wave not end up being stronger and weaker randomly across different azimuths if the scatter is random? Why does the second lobe of the P wave

disappear or get thinner (rather than broader) at some azimuths entirely? Surely some energy should remain there? Perhaps I am missing something fundamental?

Yes, the left images are wavefield snapshots through homogenous material. The right images are wavefield snapshots where there is a 1km region in around the source that is heterogeneous. Heterogeneities in the near-field produces strong symmetries (different from far-field heterogeneities) was explained by the work of Leavy, 1993 and Ben-Menahem, 1997. As Leavy (1993) described, when scattering occurs within a single wavelength near the seismic source, the dominate scattered wave is of the quadrantal type, where the frequency dependence is the same as the source (see equations 50-52 and 15 16 spherical harmonics solutions to see angle dependance). Burgos et al., (2016) also found the same result in Figure 1.

The P-wave changes because the near-field heterogeneities change the shear modulus, which changes the P-wave radiation pattern as well.

14. Fig 3: Additionally, without a colourscale which is normalised the same amount across both simulations, it is hard to compare how much energy has been scattered into the S wave as the scale has saturated. Why not plot the displacement norm (is that what's plotted?) on the same scale and give the scale a larger range?

In all snapshots, the displacement norm is plotted. Amplitudes are muted below 20% of the max amplitude, so there will be variations between different snapshots for different set ups. We added the corresponding waveforms in a new Figure S1.

15. Fig 3: I can see some reasons for focussing on pure strike-slip earthquakes (e.g. in a Korean context) but this is not discussed. At the very least, the impact of more complex source mechanisms on these results in general should be discussed.

We decided not to consider other source types as we don't think it would influence the results significantly, but we can demonstrate other sources if the reviewer recommends. We chose a strike-slip event since the resulting radiation pattern of an explosion with heterogeneities resembled a strike-slip event.

16. Fig 4: For some reason, the S wave in Figure 4c seems to have adopted a three-lobed radiation pattern? Is this a quirk of the "anomaly" velocity contrast? If so, these simulations should be run again with a series of anomaly contrasts to demonstrate that the results are robust and not dependent on the scattering model generated.

Due to the random heterogeneities, the lobes can still be influenced by the heterogeneity once the wavefield leaves the source region. Therefore, the lobe that lowers in amplitude passed through the region of heterogeneities over a longer path-length compared to the other azimuths, leading to deconstructive interference.

17. Fig 5: Maybe I am misunderstanding, but what is the "no anomaly" P/S ratio referring to?

The no anomaly means the background medium is homogenous. The legends have been updated to homogenous material as it is confusing.

18. Where is the shear wave energy coming from in an isotropic medium with an isotropic source that has no deviatoric component?

Is this in reference to Figure 5 (now Figure S4)? The region of small-scale heterogeneities in the source region causes the shear wave.

19. What is breaking the symmetry in such a way that any S wave arrives?

There is no S-wave energy in the homogenous case, so there is no symmetry. In the homogenous case, overlapping energy between the time windows and numerical noise (minimal) contributes to the S energy.

20. Fig 5: The P/S ratio with azimuth is in effect quantifying the strength of scattering at different azimuths. Why is this symmetric? I don't understand.

There are certain planes where the S-wave is not present, which is why P/S begins to increase at certain azimuths.

21. Eqn 2: This equation is confusing and the subscripts are not defined. I think what you mean is that you are squaring the amplitude at each point in the time series to get the power, and then averaging that across the length of the P wave. What does P_Ri represent? If it's just the instantaneous power, then what does it mean to take its magnitude as it is already positive?

This equation is summing the windowed waveforms. To make it simpler, we changed the inside part to be "d" = waveform.

22. L 190: "normalised amplitudes" I think (but do not quite follow your logic) that this is wrong, as the power in the seismic wave is proportional to the square of the amplitude? The normalised amplitude is still a signed quantity and the energy is never negative.

They are not normalized; it was a typo. Line is fixed at L175.

23. Fig 7b, ironically the S wavefront in this figure looks a lot more like I would expect. I am still confused by why the P wavefront is narrower, rather than broader though - there's always a small amount of back-conversion which I would expect to broaden things. Is the colour scale different between 7a and 7b? Either way, the seismograms are unreadable.

The P-wavefront is narrower in 7b because the source frequency is higher at 8 Hz.

24. L 279: "adding anisotropic anomalies near the source does not…" Perhaps I have misunderstood what these anomalies are, but going off Fig 2 I envisage anomalies which are either heterogeneous and isotropic, or heterogeneous and isotropic. How does making the anomalies anisotropic reduce the effect on the P/S ratio? If we imagine reducing the degree of anisotropy to zero, surely the two should be the same?

The anisotropic percentage of the anomalies are 20%. If anisotropy were zero, the region of heterogeneities would be homogenous and would not result in an S-wave. The anisotropy heterogeneities are where we vary the elastic tensors and add transverse isotropy (see Table S1 in the SI).

25. L 318-319: I am afraid I do not understand. Surely most of this paper has been about how P to S ratios are affected by near-source heterogeneity. Doesn't this directly contradict saying "The near-source heterogeneity does not impact the vertical or radial

component arrivals for a surface event"? How can an S wave be produced unless some energy is lost from the P wave which has no T component?

That is a good point made by the reviewer that we need to clarify. The energy for the T component does come from the P wave. The T component amplitude is much smaller than the R and Z components, so by eye it looks like the R and Z components are unchanged. That is why we said it does not impact them, but we have changed the wording to clarify that they do slightly change.

Discussion:

26. L 349: "energy equivalent to an earthquake". This might be true but I am not sure that it is really a valid conclusion of this work? The simulation used unfeasibly high velocity contrasts to achieve this. If you just mean that you can cross-compare the energy in the S wavefield to that in an earthquake then sure, that is true, but it is also meaningless unless you specify the magnitude.

We removed this phrase to improve clarity.

27. L 365 - 368: "However, for low magnitude… treated as constant" - I'm afraid this seems nonsensical to me. Do you have a reference for this? Surely, for a small earthquake, with a shorter rupture length, all that changes is the size of the inhomogeneities which have an impact on the far-field wavefield. There is a power law spectrum of inhomogeneities in the real Earth, not only those at long wavelengths that only big earthquakes can feel. Additionally, there are nuclear tests with magnitudes equivalent to kilometre-scale ruptures, which are close to the most significant inhomogeneity scales in the crust.

This is for a given frequency wave. 1 Hz wave propagating along a 10 km fault will encounter more inhomogeneity at the right wavelength than a 1 Hz wave travelling along a 1 km fault. We have changed the text to mention a given frequency wave. Also, the rupture length equivalent for nuclear tests is cavity size. Cavity size scales to the cube root with magnitude because it a volume while rupture length scales to the square root because it is a surface area. This means that a nuclear test with a magnitude equivalent to a kilometer-scale rupture will still have a small source region comparable to a much smaller magnitude earthquake even though it released much more energy.

28. L 369 onward: I am afraid that I see little point to this section at present, from here onward and suggest removing it. There is no data shown (either here or in the supplement) so the reader is left to find and interpret unspecified figures in other papers. If the authors would like to mention evidence for S-waves brought about by induced small-scale heterogeneity in actual data they could do that in the conclusion in a much more brief manner - maybe focussing on Salmon/Sterling as that seems to be the pertinent point.

We have reworded this section and reduced this section. We think comparing our results to previous explosions is important because it's where our results can be directly applied to observations. We do not have the seismograms for Salmon/Sterling and other events ourselves, but we think it is worth mentioning to the readers that these near source heterogeneity experiments do exist.

29. L 433 - 438: this seems to be the main thrust of the conclusions, which is reasonable based on the simulations done (if the above points can be addressed). I would emphasis this set of summary points and leave it there without overstating it. From L 440 onward, I am again unconvinced - a general conclusion about how inhomogeneity affects P/S ratios of earthquakes is really a much more nuanced topic, depending on source mechanism, the distributions of inhomogeneity and their power spectra, etc. I am not convinced that it is true for all real-world earthquakes that the P/S ratio goes down for larger regions of inhomogeneity, for example.

We have reworded L435 onward to be less conclusive, but more of what we observed in our work and the nuances of this work.

30. L 445: "S wavefield… looks similar to an earthquake" - again, I don't think is really true in the sense of looking like a real earthquake at all (for starters, there are no surface waves). I'm also not sure it really looks like the earthquake simulation shown - I suspect the S wave polarisation and coda don't really look the same?

We reworded for clarity. We only meant the S-wave field can appear to look like a strike-slip event, but this is correct that all waves should be considered.

31. L 447: "which we tested in 2D" - you go on to say that anisotropy requires 3D - so how did you test this in 2D? Also, L 453 "on similar"?

In 2D, we used a simplified symmetry, such as hexagonal symmetry, which has symmetry over a single axis, which can be modeled in 2D. See Section 3.3. We have corrected L453.

32. L 460: "As a result of this study… may not always be a reliable metric" I don't think the literature seriously suggests that this can ever be a reliable metric in isolation. Similarly in L 483 - 484.

We updated the text to reflect that P/S is used in combination with other metrics. Line 527.

33. L 462: Similarly, what do you mean "S wave generation could only occur under certain conditions"? Surely in every test you did with an inhomogeneity, there were S waves generated?

We updated the text to be clearer. Now line 531.

34. This entire section is missing references to practical methods used by the USGS, CTBTO, and others - they have a list of proposed criteria which is not even mentioned.

We have added additional references, line 529.

Conclusions:

35. L 489-90: You state that you will "outline the key conditions needed to produce an S wavefield with explosive sources", but then do not go on to actually list any conditions (just a summary set of points of the paper which are not conditions".

We modified the conclusion to be clearer. Lines 509-522.

Specific minor comments:

36. VTI and HTI should be defined prior to their first use in the abstract.

It has been removed from the abstract.

37. L 43: “this method” - which method? P-to-S discrimination?

Done.

38. L 59: (fig 1 caption) “normalised” - normalised with respect to what? The maximum absolute value in the SV normal trace?

The amplitudes are all equally adjusted by the same multiplier for visualization.

39. L 72-74: “However, …” appears to be a repeat of several sentences from earlier in the introduction.

We have reworded this sentence to be less repetitive (line 79).

40. L 114: Units should be properly formatted, i.e. not as “1e25 dyne*cm”

Done.

41. L 127: the S-wave wavelength is only 4.5 km at 1Hz in your simulation.

We have clarified the text.

42. L 265: “asphericity” - asphericity of what?

This has been removed.

43. L 361: plane not “plain” (and others - stopped keeping track).

Done.

Reviewer 3:

Review for Seismica

Review for “Shear-Wave Radiation Patterns from Explosive and Earthquake Sources in Scattering, Heterogeneous Media” by Peter Nelson and Neala Creasy

This paper presents a numerical study of explosive sources seismic waveforms when embedded within a scattering medium in the source vicinity. The paper focuses on the polarization of S waves generated by small-scale isotropic and anisotropic heterogeneities. The paper is interesting, and the small-scale isotropic heterogeneity part is (mostly) in agreement with previous works on the subject. The small-scale anisotropic part is new and interesting. One of the main issues of the presented tests is the exact location of the point source with respect to the heterogeneities, particularly with respect to the nearest one. In the case of a single heterogeneity, for example, placing the source at the center of the heterogeneity or near one of its corners will produce very different results. My impression is that the exact location of the source relative to the nearest heterogeneities should always be very clear, and that some cases probably need to be re-run to avoid some particular locations, such as the center of a heterogeneous cube, being the only tested case. A few other things could be improved before considering publications:

1. The definition of small-scale is not rigorous (see my first remark).
2. Figures: The subplot of the geometry should be provided each time, and large enough to clearly see the relative position of the source within an element (it is provided a few times, but it is very small). In particular, the exact position of the source relative to the immediate surrounding heterogeneities should be visible as it is one of the critical parameters to understand the studied effect (Burgos et al., 2016). The geometry of the heterogeneities is not discussed. As it can be seen in Capdeville (2021), Figs 10 and 11, more complex geometry can lead to different types of radiation patterns.

Some references could be added (see below).

Remarks:

3. Line 82: The size of “heterogeneities are at least 1/6 of the…” This is not a very rigorous definition of small scales. Indeed, a very large square heterogeneity with a source near one of its corners would produce a large effect. A similar size heterogeneity with smooth border would produce no effect, instead of size, one should talk of heterogeneity spectrum. Then, we can argue the spectrum about what (velocity, slowness, elastic tensor or else). This leads to homogenization as discussed in Capdeville et al. (2020).

Heterogeneity spectrum has been used in most studies, but we wanted to show even with the simplest representation, we can produce S-waves. By conducting this approach, we can explore more specifically the effects of source frequency and anisotropy. We added Gaussian distributions to Figure 1 and the resulting radiation pattern to show that our results are similar.

4. It is probably difficult to introduce such a notion (homogenisation) in the present paper; nevertheless, I suggest the authors to be more precise about heterogeneity size or scale.

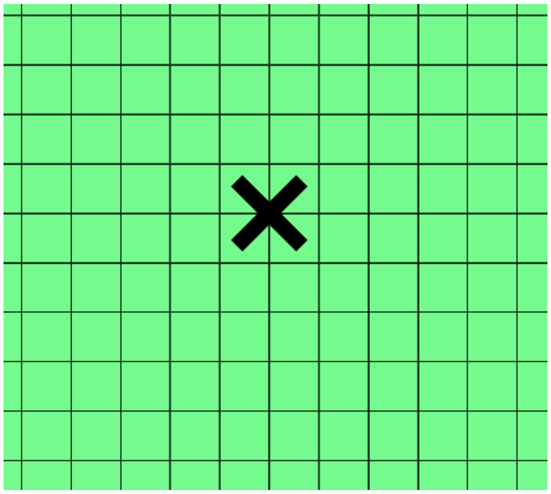
We include a summary of homogenization in the Discussion (line 563). We also explored a gaussian spectrum with RMS value of 25% and 10% and correlation length of 35 meters, similar to our checkerboard type heterogeneity (line 162). See new Figure 3 and Figure S1.

5. Line 134, Figure 3: An early very similar modeling can be found in Capdeville et al. (2010), Fig. 5.

We added the citation to Figure 3 caption.

6. Line 138: Moreover, as shown in Burgos et al. (2016), the exact location of the source relative to the heterogeneities strongly matters. Within an element surrounded by other elements with different mechanical properties, if the source is located in the center of the element or within 1 meter (for example) of a corner, makes a difference. In general, the relative position of the source relative to the nearest small heterogeneity is hard to guess from the figures. A better explanation of figures should be provided for each case.

Here is a zoom in on our mesh for all cases in this study. We place the source at the vertices of the mesh elements. In the figure below, our source is the black “X”, and the mesh is displayed as green squares, where an element is represented by a green square.



7. Line 149: “1 GLL away”: gives a distance. Putting it that way gives the impression that the source needs to be located on a collocation point, which is not the case.

In the paper, we added more specificity on the distance between our GLL points (see line 142). Also, this is correct. We placed the source 50 meters away from the edge of the perturbations and updated the text (Figure 4 caption). Leavy, 1993 equations 48-52 show that as long as the rigidity perturbation is a sub length distance away (wavenumber*radius<<1), it has a strong effect on the scattered wave field.

8. Lines 158-163 and Fig 5: It would be nice to add a zoom on the mesh + material properties and source location of each (or at least one) of the cases. It would help to visualize.

We have included the figure above for visualization and added text in the document to explain the location of our source (lines 111-114).

9. Lines 158-161: I’m not sure of this conclusion. It also depends on the exact distance of the source to the heterogeneities. Reducing the size of the heterogeneity while making the source closer and closer to one corner of the heterogeneities, you would probably obtain different results.

We modified the text at Lines 187-198. The particular text this comment refers to has the source in the middle of the region of heterogeneities. We do agree the exact distance matters. See comment 7.

10. Fig 6: The relative source position to the nearest heterogeneity corner is not clear here. As mentioned already, in my experience, the relative distance is also important. It would be interesting to see if the observed pattern is reproduced if the source gets closer to this corner at the same time as the patches get smaller.

The source remains constant in its location for each scenario (in the middle of the domain). The region of heterogeneities just decreases (the mesh stays constant). Figure 6 is now Figure 5. We are confused on what the reviewer means for, “It would be interesting to see if the observed pattern is reproduced if the source gets closer to this corner at the same time as the patches get smaller.”

11. Fig 7: This is unexpected, and I would be surprised if this were true. Where is the source relative to the homogeneous inner box? If the source is at the center of the area, I would suggest moving it toward the corner of the homogeneous box and see what happens. Make a bigger sub-figure of the geometry with source locations, etc.

The source is in the middle and along the vertices of mesh elements. Figure S5 shows insets of the simulation. The source is placed within a homogenous material but that material is surrounded by heterogeneous material with a gap set by Figure 7c (now Figure 6c).

12. Line 305: Choosing the center of the heterogeneous inclusion is not a good choice. The center of symmetry is an exceptional location for the studied effects. I suggest picking an excentric source location (near one corner, again) and redoing the test.

All the simulations are on the corner or vertex of four elements. The source is in the center of the mesh domain, not the middle of an element.

13. Lines 365-370: This discussion seems incomplete as it doesn't mention the heterogeneity spectrum. Is a small source really more likely to meet a heterogeneity change than a large one is dependent on the heterogeneity spectrum, and the answer doesn't seem obvious to me.

We included a comparison to a heterogeneity spectrum to add a comparison to our checkerboard set up. We do find similar results, but the strength of the S-wavefield changes, similar to what Burgos et al., 2016 found. See previous comments.

All the best.

References

Capdeville, Y., Guillot, L., & Marigo, J. J. (2010). 2-D non-periodic homogenization to upscale elastic media for P–SV waves. Geophysical Journal International, 182(2), 903-922.

Capdeville, Y., Cupillard, P., & Singh, S. (2020). An introduction to the two-scale homogenization method for seismology. In Advances in Geophysics (Vol. 61, pp. 217-306). Elsevier.

Capdeville, Y. (2021). Homogenization of seismic point and extended sources. Geophysical Journal International, 226(2), 1390-1416.

Rebuttal letter round 2

This paper is an improved version of a manuscript that I previously reviewed. It looks at the generation of shear waves from sources which would not necessarily be expected to produce intrinsic shear phases (i.e. explosions) and compares them to earthquakes. The authors place some constraints on the scattering parameters that give rise to strong shear waves.

This manuscript is an improvement on the previous version in terms of clarity and explanation. Many of my original comments have been addressed which is great (or the relevant figures have been reworked/removed so they no longer apply), but these clarifications have also highlighted areas where further improvement is needed. I suggest it is returned to the authors for major revisions. There are also some errors and contradictions scattered throughout the paper which need addressing.

In general, the scientific content of this paper would be enhanced by discussing the physical origins of some of the conclusions, rather than just stating results. In particular, when discussing the effects that (an)isotropy has on earthquake versus explosion wavefields, there are a few places where I wonder if the results are generalised or just a quirk of the authors' setup, because no plausible physical explanation is provided.

We thank this reviewer for their very constructive feedback and comments that have improved our manuscript.

Major comments:

Introduction:

1. I think that this could be laid out more clearly in sub-sections corresponding to past work, ongoing areas of research, and scope of the article. "in this study", "in this article", and "we explore" appear throughout the introduction which makes it hard to follow what is novel about this work.

We reorganized the introduction and made the last paragraph to focus on the contributions of our study (lines 111-120).

2. L83 (and elsewhere): I don't think that it is meaningful to say that "…large S-waves when heterogeneities are at least 1/6…". What does "large" mean in this context? Comparable to the amplitude of the P-wave? Is 1/6 the average size of heterogeneities, or does this result only hold for uniform distributions? I think Reviewer 3 also picked up on this but it does not seem to have been addressed.

We see now how using just the word large could be vague. What we do mean by large is that the S-wave field has comparable amplitude to the P-wavefield. We have changed the text wherever we have described the S-wavefield as "large" to also state comparable to the P-wavefield.

For the 1/6 comment, we made a math error. The 1/6 came from our patch tests in figure 5 where we mistook those values as half widths, but they are actually full widths. So, the real value is 1/12. The 1/12 refers to the size of the heterogeneity region not the size of each individual anomaly. We have modified the text to explicitly state we are referring to size of the region of heterogeneity, not the individual heterogeneities. We did not perform any simulations to give insight on how the size of the individual heterogeneities impact the wavefield. This comment also made us realize that we probably did not prove the 1/12 relationship as rigorously as needed so we have also added the caveat that is for our simulation setups. (Lines 116-117).

Numerical methods:

3. L92: I don't think that the words "small" and "minimal" have any meaning here without some comparison. Minimal on a laptop? Cluster? Supercomputer?

We meant minimal on a single computer with multiple processors, a supercomputer or HPC is not needed for these simulations. We changed the section to be clearer that HPC is not needed for these simulations (line 127-128).

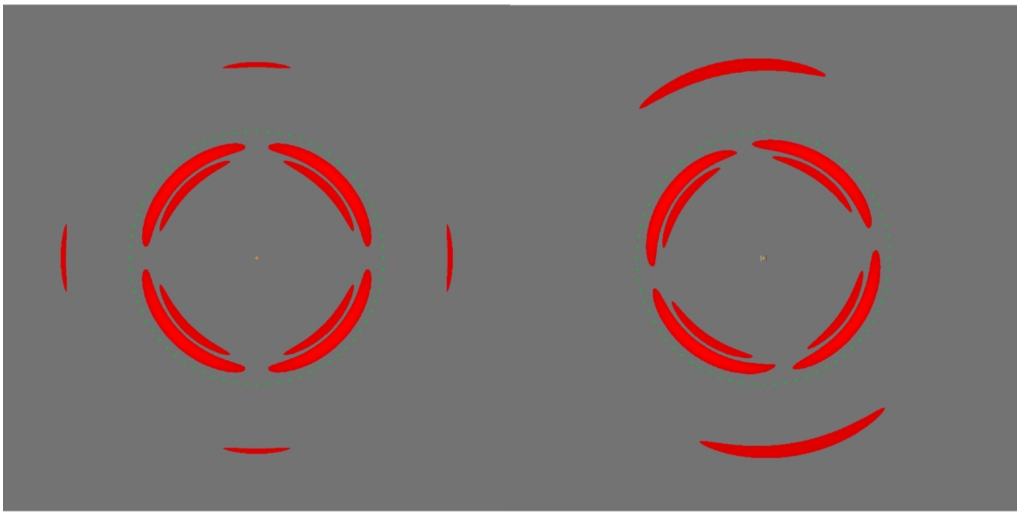
4. L 96 & 101: the use of "most" is inappropriate here. These results need to be reproducible so it is important that authors know which ones were done on a uniform grid with a gaussian STF, and which ones are not.

We agree that we should clarify "most." The only simulations where we changed the mesh from 200m was when we did the mesh convergence test. We changed this sentence to make that clearer (lines 133-135)

5. L103-104: I think it is important to note that most real earthquakes do not have all three diagonal entries being zero, rather are generally assumed to have a total zero volumetric component. In reality, this means that the comparison is never really going to be between a diagonal moment tensor and one where the diagonals are zero. This needs to be at the very least discussed and acknowledged, and ideally the authors should test their results on a more realistic moment tensor.

We only selected this tensor as an example and to emphasize end member scenarios. It's certainly true that there would be diagonal moment tensors, but it would not necessarily change our results, only the azimuthal pattern observed. We are showing another example of a dip slip event, showing a similar result to the strike slip event. We emphasized that the P/S ratio pattern will change with different source type (lines 451-452).

Dip Slip (homogenous [left] vs. heterogenous [right]):



6. Additionally, at some point in 3D the authors introduce a pure strike-slip earthquake as a comparison. I can see some reasons for doing this but they need to be expanded upon, at the very least making clear how things would change with fault type. This was the same comment as made in the previous review.

With different fault types, the radiation pattern would change which would influence the P/S ratios. We added lines emphasizing that our P/S results would change if we selected a different earthquake at lines 451-452. We mainly selected a strike-slip because it's simple, has the most azimuthal change in its radiation pattern and the largest off diagonal terms (Figure 1 and line 142).

7. L104-107: I am concerned to find that the 'placement of the source matters significantly" within an element. Of course, all numerical methods have some issues such as this, but the authors have not explained why they chose the vertices of the elements, or why they believe that this is meaningful/correct as compared to the real world.

The placement of the source matter significantly because of the strong dependence source to heterogeneity with distance away. You can see in Burgos figure 5 moving the source less than a 0.1 of a km into one of the homogenous blocks has a significant impact on the deviatoric component. We found similar results, and this is also supported by the equations in Leavy, 1993.

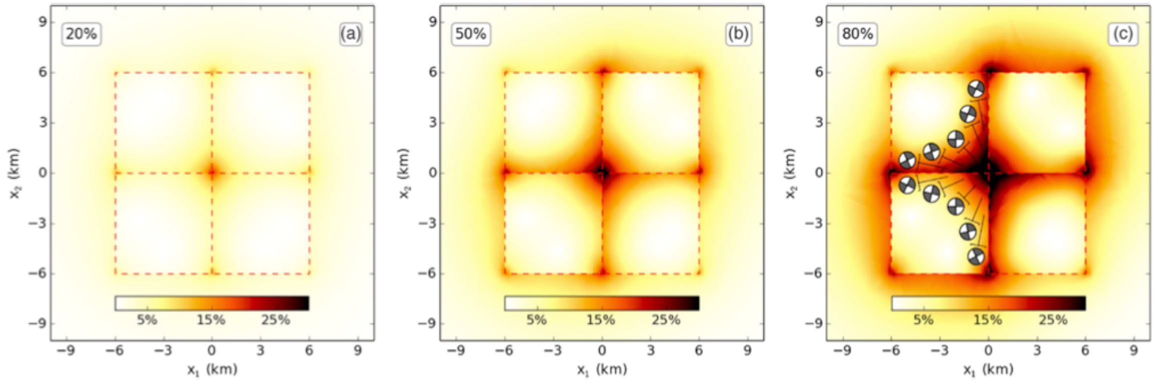
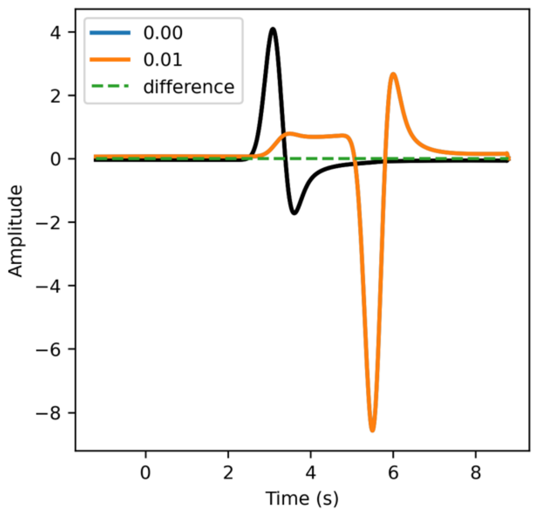


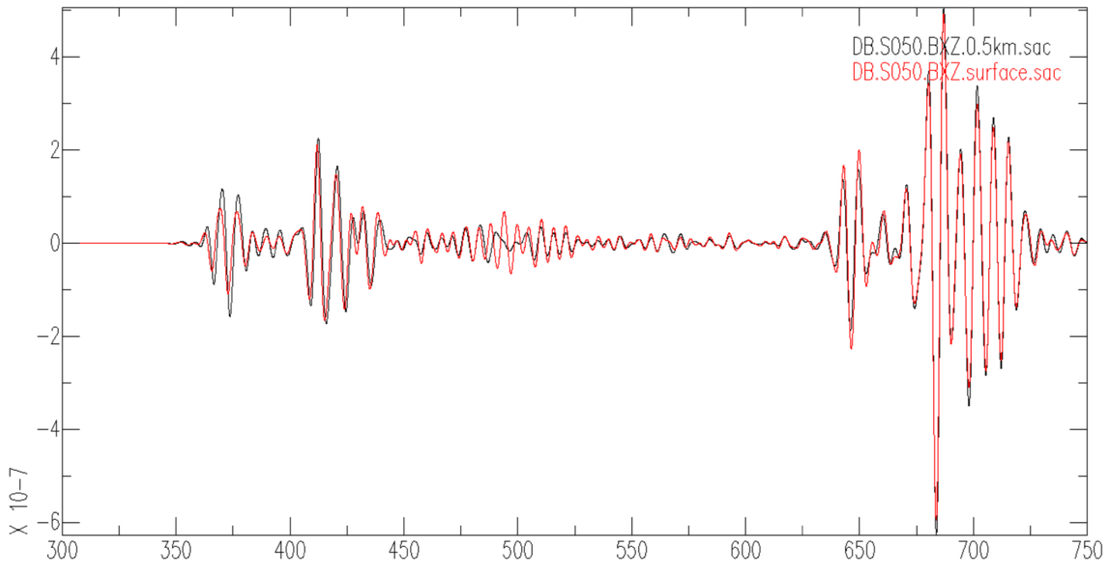
Figure 5. Deviatoric component *p*DEV distribution for three 2-D heterogeneous media with various amplitude contrast values: (a) 20%, (b) 50%, and (c) 80%. Focal mechanisms of the deviatoric component are shown for some specific source locations on Figure 5.

We placed the source at 0,0 for convenience so it is in the middle of our model. To show that placing the source directly on the vertices do not have an impact on the results, here a comparison of the results at which the source at (0,0) meters and other at (0.01,0.01) meters and as you can see the seismograms are practically identical and plot on top of each other.. The next figure shows each of these simulations, where the black lines are radial, the coloured lines are transverse components. The waveforms are just slightly offset from each other. Labels represent if the source is at (0,0) or (0.01,0.01). Numerically, SPECFEM can accommodate a point source placed anywhere, including at edges or vertices between elements. If a point source falls on an edge between two elements, it gets injected over both, i.e. interpolated over the Lagrange polynomials of both elements.



8. L112: does SPECFEM support sources that are identically at the free surface? I am aware that this can cause issues in some codes where the traction-free boundary condition can in some way mask part of the moment tensor.

We are unaware of this issue, and we could not find any publications that reference this issue, so we are happy to fix it if we know more details. We have run simulations with events at the surface and compared them to events at 0.5 km (see figure below). All we find is minor differences in the amplitude. Below are the results of the same setup for SPECFEM recorded at a station 30 degrees away with one event at the surface(red) and the other 0.5km deep for an event with Mrr=1 and rest of the moment tensor elements = 0. We find similar results when the other moment tensor elements =1.



9. L114: "the transverse component is absent in 2D simulations" is in direct contradiction to "the "transverse" component is perpendicular" above. I assume that the authors mean to refer to the third component of the wavefield in 2D, but the language should be clearer.

Yes, we did. We have fixed it. Line 156.

10. L122: "filtered from 0.14 to 20Hz" – why is this frequency range chosen? The number of 8Hz was mentioned on the previous page for 2D, does this mean that 3D simulations were at higher frequencies than 2D? Why was 0.14 chosen?

We selected 0.14 Hz as the minimum because the duration of our seismograms were 14 seconds long. We selected the lower end period of the bandpass to be ~½*duration of the seismogram. Therefore, 1/7s = .14 Hz. For 3D synthetics, we do use a different filter because the resolution is lower (1.75 Hz). We added that information about 3D filtering in line 164-165. Sometimes numerical noise can be present above the resolution of the simulation, so we filter the waveforms to exclude any frequencies above the resolution.

11. I believe that the authors have made a mistake in their conversion from dyne-centimetres to Newton-metres...

Yes, we multiplied by 100 instead of dividing for cm – meter (line 161).

12. The authors have stated that they use M_rt = M_rp = 1e25 d-cm for the earthquake, and and M_ii = 1e22 d-cm for the explosion, but does this not produce two sources with different seismic moments, which is no doubt affecting the results?

We checked and M_ii = 10^18. This was a conversion mistake.

Results:

13. L130-131: the statement that "variations of 25% are reasonable" requires some referencing and justification.

We fixed this statement that 25% is rare and 10% is more common in the crust. Lines 177-178.

14. The statement "RMS variations of crustal rocks can exceed 10%" requires some clarification as to what units are being talked about

We corrected the sentence that is reference to seismic velocities in crustal rocks (line 178).

15. There does not appear to be any discussion of effective medium theory in this section, which I think is required for the explanation of wavefield behaviour when the length scale is significantly shorter than the wavelength

You are right. We have added some sentences about effective medium theory. We think this is a good idea for future research (lines 507-516).

16. The explanation of coordinate systems in 2D is confusing.

We tried to rewrite the coordinate system. See lines 207-208. We mistakenly wrote right-handed system, but it is left-handed.

17. L191: "a 350m hetereogeneous region will produce similar P/S ratios as a 60km region" – I do not know what this sentence means?

We rewrote the phrase to the following: "For example, the P/S ratios for the 350 m and 60 km tests are nearly identical (Figure 5). Therefore, only the near-field heterogeneities influence the overall wavefield, while heterogeneities further away from the source (e.g., 60 km test) do not seem to change the P/S ratios at all." We hope this is clearer. We just wanted to point out that far-field heterogeneities do not influence the P/S ratios at this source frequency, but the near-field heterogeneities do. Lines 243-245.

18. Throughout this section, there is alternation in discussion between units of wavelength and units of physical distance. I suggest moving all of the physical

distances into units of wavelength, I cannot really interpret what '175-200m' means in a seismic context, but 'half a wavelength' is easy to understand.

This is a good idea. We added the relative wavelengths with respect to 1 Hz in parathesis next to where we had physical units where it makes sense to.

19. L218-219: "higher source frequencies may generate more S-wave energy through scattering" – it is not clear to me that this is generally true in all cases?

This is a fair point. We updated the sentence to be more accurate (now line 272-274).

20. L220: "at certain frequencies and scales of heterogeneity" – I don't think that this has any meaning without some quantification

We quantified the frequencies and scales in this sentence (now lines 274-276).

21. L236-238: This is not really an explanation of how SKS splitting works, I think some more detail is needed to make the analogy works, or it can be removed.

We just removed the reference to SKS, as splitting is partitioning energy onto the other horizontal component. We just rephrased the sentence and added additional context (lines 311-314).

22. L241: "we alternate" – again, does this mean that half the simulations are one way and half are the other, etc?

No, we designed the mesh in the near field for heterogenous anisotropy, where each element alternates between VTI and HTI, similar to a checkerboard but with the axis of symmetry. We rewrote the lines (301-302).

23. L244: 'for the earthquake source' – can the authors provide some justification or explanation for this? Are they sure that it is not just a numerical issue? It seems a bit odd to me, given that we know that anisotropic heterogeneities can have a substantial influence on wavefields.

This is an area of active research. We also find this interesting, but we don't think it's a numerical issue given all the other tests we have done. Explosions are purely P-waves while earthquakes are a mix but mainly consist of S-waves, therefore, it could be a fundamental difference between how P and S-waves scatter off anisotropic anomalies. We would need to conduct further experiments and tests which is beyond what we can do for this paper. We did not want to speculate about it in the paper, so we left it only as an observational remark.

24. L246: Similarly, is there some plausible physical justification for why the same heterogeneity has a bigger impact on the wavefield when it is isotropic rather than anisotropic? This seems counterintuitive to me, though I am happy to be convinced. I would imagine that isotropy is a special case of anisotropy, and as

anisotropy increases the impact on the wavefield also increases? If that is the case, then the isotropic case should not be the most affected?

We intend to explore anisotropy more in a future study. Our current hypothesis is that the anisotropic variations are not as impactful because each small heterogeneity is only changes the axis of symmetry. In the isotropic case, each grid point has a strong change in isotropic velocities (+/- 25%). However, in the anisotropic case, each grid point only has a strong change in anisotropy with the same isotropic value, so if you consider only the velocity change that is occurring for a certain wave direction, the actual change in velocity relative to the background is minimal. In short, because the anisotropic variations are constrained to have the same isotropic velocity values as the background it results in a smaller effective perturbation strength than the isotropic case so that is why the impact is smaller.

Discussion

25. L314-316: what is meant by "has a four-lobed pattern"? what does? The whole wavefield?

We have corrected the phrase (line 381-382).

26. L329-331: "as a result… because perhaps" – I am really not sure about this sentence. It is true that very, very small earthquakes have rupture lengths that are comparable to cavity sizes, but these are very unlikely to be detected at regional distances. I imagine that the main issue of discrimination for small earthquakes is rather the limited data…

We expanded that limited data is another reason. Line 398-399.

27. L332-339: I am not sure what the main take-away from this section is, other than that the results do not really agree with observations? I think that warrants further explanation?

The point of this section is describing that in 3D there is transverse component energy produced by the explosion but it's small relative to earthquakes. Earthquakes can have polarization angles near +/- 90 degrees, but the explosions within small-scale heterogeneities cannot reproduce these polarization angles, so it's possible this may help with discrimination. We added a sentence to emphasize this point in line 368-370.

28. There are some interesting points made in this section about the various tests and damage explosions, but I think that they might be better placed in the introduction so that they frame the topic, as they do not really relate to the specific work undertaken in this paper.

We moved most of this discussion into the introduction. See lines 52-76.

29. L369-371: is it not immediately obvious to me why an aspherical geometry can be treated as a rigidity perturbation, nor is it clear to me why this is relevant to this paper.

We rephrased the sentence that there would be strong perturbations in seismic wave speed and density, but changes in rigidity is what causes the S-wavefield to form. In an aspherical vs sphere cavity geometry there will be some area that goes from rock to air. The transition from rock to air can be treated as a 100% reduction in S-wave velocity/rigidity (Line 439).

30. L433: I am unsure that moving to an unstructured mesh would necessarily be quicker, because lots of implementations and libraries might well end up being slower with a more complex mesh connectivity. Unless the authors can show that this is the case, I suggest that they remove this discussion.

We deleted the sentence.

Conclusions:

31. Overall, I feel that the conclusions of this work are overly-generalised without justification or clarity. For example, "under some circumstances, mainly in 2D" – I do not think that these peculiarities (2D, fully non-diagonal moment tensors) are really applicable to real-world circumstances. That doesn't mean that this work isn't useful, indeed it is, but I think more caveats on the conclusion are needed.

We have added more text to the conclusions to be more specific and add more caveats. See lines 522-532.

32. I re-iterate my concerns about the use of thresholds such as "1/6" without any explanation or clarity. I imagine that any resolved scale will produce some S-wavefield, the question is how big/significant it is.
    See comment 2

Figures:

33. In general, the figures in this version of the paper are significantly better than in the previous version. Fig. 5: I do not see a light purple line, and if it is present the lighest shade of green is almost invisible

The lines all overlap, which is why it's hard to see. After 350 meters, they all overlap, so we just removed the lines for the larger areas and added a note in the caption.

34. Fig. 10: the lines in the seismograms are overlapping to the point that it is hard to make anything out. If this time range is not relevant to the paper, I suggest trimming the figure. At least one figure in the supplement seems to have the same issue.

We increased the font size and just showed the comparison of the two explosion simulations to demonstrate our point.

Minor comments:

- L42 I think this should be a ‘meteoroid’ impact rather than a ‘meteorite’

Changed.

- L53 typo

Fixed

- L55-57 “introduces several influences on the seismic wavefield…” not sure that this sentence makes sense, maybe it should be “introduces several features into”

We modified the sentence (line 83) to be clearer.

- L62-63 “and under which conditions must be satisfied” is poor grammar

We broke up the sentences to be clearer (line 111-113).

- L64 I am unfamiliar with the term ‘quadrantal type’

Quadrantal type is when the wavefield has a pattern where there are two symmetry axes 90 degrees apart, thus creating four equal sections, similar to a double couple earthquake source with radiation distributed over four quadrants. We added a definition to the text (line 92-93).

- L67 ‘azimuth of the wave’ – do you mean position on the wavefront?

Yes. The azimuth of the wave from the source (line 96).

- L74-77 this appears to be a partial repeat of something on the previous page

I am not sure what the reviewer is referring to as being repeated. Please clarify.

- L91 “SPECFEM2D can do” is not particularly scientific language, I suggest rewording this

We have rephrased this section (line 125).

- Please stick consistently to one set of units, e.g. 1e22 or 10^{22}.

Done.

- L163: j and k are not indices but rather limits for the index i.
  Fixed
- L235: what is meant by “coordinate axes” here?

We changed the phrase to axes of symmetry because we meant the axes of the anisotropic symmetry in this case (line 295-296).

- L259: what does ‘common regional distance stations’ mean?

We changed to sentence to include regional distances as stations <15 degrees.

Reviewer B:

I have examined the revised manuscript and the detailed responses from the authors, included the numerical tests. I have no additional comments, and I would be happy to see the paper published as it stands.

Thank you; we appreciate your responses in improving our paper.

Reviewer C:

I still cannot find the correspondence between the line numbers given in the rebuttal letter and the other two documents, which is very frustrating and annoying
I just put the first example I can find:
-"We include a summary of homogenization in the Discussion (line 496)"
It happens that the discussion finishes on line 470. Moreover, reading the Section "Discussion", I cannot find the mentioned summary. The only thing I can see is "we think this work will help improve homogenization studies in the future. Future work on this study could include how anisotropy effects homogenization." This doesn't make sense. Homogenization is a general method that applies to any elastic tensor, including anisotropic ones. I'm curious to read what this claim means.

We thank Reviewer C for their time. We apologize for the confusion regarding the line numbers. We tried to mitigate that by submitting a non-editable PDF in addition to a Word document.

For the homogenization sentences we added, we want to emphasize that homogenization is a useful approach that can be used instead of HPC (lines 530-535) to reduce computational costs. However, the computations in this work were minimal. We view homogenization as a helpful tool to reduce computational costs, so we reduced our discussion about homogenization to focus on that.