Reviewer Comments

For author and editor

In this paper, the authors present a new Vsv model of the lower crust and mantle lithosphere in the neighbourhood of the Gawler Craton and Curnamona Province based on surface wave tomography. From these results, and by drawing on other information from the literature, including isotope data, xenolith data and resistivity imaging, they make a series of findings on the nature of the cratonic lithosphere, the effect of Rodinian breakup, and the implications of IOGC prospectivity. Overall, I found the manuscript to be well written, illustrated and the underlying science robust. As such, my comments below correspond to minor-moderate revisions.

1. I'm not sure you need to state your main conclusions at the end of the Introduction.

2. Line 104: If you're going to mention the corner period of the Trillium Compact, you might as well do the same for the LE-3Dlite (1 Hz I believe).

3. Line 111: Do you think it is correct to characterise these methods as modelling surface waves as plane waves? In any case, you should include a few references at the end of this sentence to back up your claim.

4. Lines 114-116: While I largely agree with you, the two-plane wave method has limitations in terms of array aperture, so it would be worth mentioning this (you do touch on it later).

5. Line 123: It would be useful to have a bit more detail on the approach used to distill the event catalogue down to 142 events.

6. Lines 149-152: It may be worth emphasizing that the uncertainty has more meaning in a relative rather than absolute sense, since your knowledge of data uncertainty and prior model uncertainty is limited (i.e. not much constraint on absolute values).

7. Figure 4: Is any noise added to the synthetic dataset used for checkerboard reconstruction? This can be useful to apply, since it provides a dose of realism to the recovery.

8. Figure 6: I don’t see the plot numbers (i), (ii) etc in the figure. Also, on lines 177-178, it suggests that there is an explicit Moho (?) included in each inversion, but I don’t see much evidence of it here, but perhaps it’s averaged out in these plots? I guess one caveat is that since the minimum period is around 30s, there is little constraint on the crust, which is why velocity is almost constant in the upper 25 km (which in reality it won’t be), then there is a strong gradient to mantle Vs values between 25-50+ km depth. This is not really suggestive of an explicit Moho.

9. Figure 7 Moho map – it would be useful to see a comparison with AusMoho, perhaps in the supplementary information. Also, only part of the colour bar range is used, so I would suggest recallibrating to use all colours available.

10. Lines 208-210: Given the uncertainties in the Vsv models, which should be quantifiable via the transD inversion approach, is such a tight classification really justified? What happens if you choose a slightly different range of numbers?

11. Paragraph stating with line 306: The exact nature of the lithosphere between the Gawler Craton and Curnamona Province is an interesting question. As the authors point out, given the rifting and associated processes that occurred, one might expect it to be lower velocity or otherwise more distinct from the adjoining regions. It is a fairly narrow region on the surface however, so if it in fact did feature lower velocities, would the tomography used in this study be able to resolve it, particularly at greater depths? At crustal scale, Young et al (2013) did find that the Gawler and Curnamona were characterised by distinct low velocities – see

https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/grl.50878

while Rawlinson et al. (2016) found that the mantle lithosphere beneath the cores of the craton and province were characterised by higher velocities -see

https://www.sciencedirect.com/science/article/pii/S004019511500671X

Reviewer Comments

For author and editor

Here's the review of the manuscript entitled "The deep lithospheric structure of the South Australian Craton revealed by teleseismic Rayleigh wave tomography." The manuscript is well organized, internally consistent, and flows logically from tectonic context to seismic modeling and lithospheric interpretation. The text structure within sections is generally strong, but some paragraphs are excessively long, especially in the Discussion, which reduces readability. In the following, I give some comments and refer directly to the corresponding lines in the manuscript. Overall, my recommendation is a Minor revision for this manuscript.

1) In the Abstract section: The flow from the cratonic lithosphere imaging to its metallogenic implications is logical, but the transition between these two themes is abrupt. A brief linking sentence could help bridge the shift from geophysical to mineral exploration implications, provided word count limits permit.

2) In the Introduction Section:
2-a) The transition between the first paragraph (geological background/tectnic framework) and the second (seismic velocity context) could be smoother. Consider adding a linking sentence such as: "To investigate how these long-lived processes are reflected in present-day lithospheric structure, seismic velocity models provide a key diagnostic tool."
2-b) The final paragraph is effective, but mixing the methodological details (arrays, periods, etc.) with the conceptual outcome diminishes its impact. Consider separating the paragraph's content into two distinct sentences to clearly present the data/method first, followed by the application/outcome. This structural change will significantly enhance readability and clarity.
2-c) Use commas more sparingly; several long sentences can be split for easier reading.

3) The Tectonic Framework section:
3-a) The first paragraph effectively defines the cratonic units, but the description of margins is lengthy and could be condensed. Hence, summarize margin characteristics before moving to mineralization history.
3-b) The second and third paragraphs shift to IOCG deposits and geodynamic context; this transition is slightly abrupt. A linking statement such as "Understanding the tectonic and metallogenic evolution of the Gawler and Curnamona regions provides essential context for interpreting lithospheric structure" would improve coherence.
3-c) This section intermixes tectonics and mineralization discussion. Since mineralization is later related to lithospheric architecture, consider highlighting the last paragraph explicitly such as "Implications for metallogeny", to improve internal organization.

4) Rayleigh Wave Phase Velocity Maps section: The explanation of the resolution matrix results is clear but somewhat repetitive. Consider condensing the sentences on decreasing resolution with the period into one concise statement.

5) Shear Wave Velocity Model section: The section opens well but could benefit from an introductory sentence summarizing purpose: "We next inverted the phase velocity maps for shear-wave velocity structure to derive a pseudo-3D model of southern Australia."

6) Discussion section:
6-a) sub-section 7.1: The Moho: Clear and concise; well-positioned. However, it could end with a short linking phrase to the next subsection (“Having established the Moho depth pattern, we next examine the lithospheric mantle structure”).
6-b) sub-section 7.2: Lithospheric Mantle Structure: Excellent synthesis but overlong; consider subdividing into thematic paragraphs (core features, margins, anisotropy correlation) for easier reading. The argument is strong but buried in dense prose.
6-c) sub-section 7.3: Comparison with Mantle Xenolith, Resistivity, and Isotopes: This is well argued but could benefit from clearer paragraphing — currently, some transitions (e.g., from Gawler Craton to Curnamona Province) are abrupt. Adding subtopic openers (“In the Curnamona Province…”) improves readability.
6-d) subsection 7.4: Lithospheric Signature of IOCG Deposits: A strong and coherent conclusion. The logical progression (previous models, then new findings, next explain implications) works well. Only minor condensation suggested (the long summary of past MT studies could be reduced).

7) Minor comments:
Line 14: edit "recorded on three recent broadband" to "recorded by three recently deployed broadband seismic arrays."

Line 36: edit "so velocity variations can be used to map lithospheric architecture" to "thus, velocity variations can be used to map lithospheric architecture."

Good luck
Shirzad

Dear Chiara,

Thanks for your efforts on this. I've worked through the reviewer comments, responded, and uploaded a revised manuscript for consideration (including a formal response letter, clean manuscript version, highlighted changes manuscript version, latex source files, figures, etc.).

Best wishes,

JP

October 30, 2025
Dear Dr. Civiero,
Detailed below are our responses to the reviews of “The deep lithospheric structure of the South Australian
Craton revealed by teleseismic Rayleigh wave tomography”. We include a pdf version with highlighted changes, a “clean” counterpart, and the various latex source files. We hope that the manuscript
is now deemed suitable for publication.
Sincerely,
J. P. O’Donnell on behalf of all authors

Editor's comments
Dear John Paul O'Donnell, Kate Selway, Claire Wade, Stephan Thiel, Caroline Eakin, Robert Pickle, Shubham
Agrawal, Bruce Goleby, Alexei Gorbatov:
I hope this email finds you well. I have reached a decision regarding your submission to Seismica,"The deep
lithospheric structure of the South Australian Craton revealed by teleseismic Rayleigh wave tomography".
Thank you once again for submitting your work to Seismica.
First of all, please accept my apologies for the delay in sending the reviews. Based on the comments I
have received, your manuscript may be suitable for publication after minor revisions. Both the reviewers and
I agree that it is a good and well-written paper, and that the revisions mainly aim to improve the structure
and clarity of the text, without requiring any significant changes to the methodology or interpretation.
When you are ready to resubmit the revised version of your manuscript, please upload:
• A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.
• A pdf version of the revised manuscript clearly highlighting changes/markup/edits.
• A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a
summary of the resulting changes made to the manuscript.
Once I have read your revised manuscript and rebuttal, I will then decide whether the manuscript either
needs to be sent to reviewers again, requires further minor changes, or can be accepted.
If you deem it appropriate, please check that the revised version of your manuscript recognises the work of
the reviewers in the Acknowledgements section. Please note that Seismica does not have any strict deadlines
for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly,
and please let me know of any expected delays. I wish you the best with working on the revisions. Please
don't hesitate to contact me with any questions or comments about your submission, or if you have any
feedback about your experience with Seismica. Kind regards, Chiara Civiero
1
Reviewer D
In this paper, the authors present a new Vsv model of the lower crust and mantle lithosphere in the neighbourhood
of the Gawler Craton and Curnamona Province based on surface wave tomography. From these results, and by drawing on other information from the literature, including isotope data, xenolith data and
resistivity imaging, they make a series of findings on the nature of the cratonic lithosphere, the effect of
Rodinian breakup, and the implications of IOGC prospectivity. Overall, I found the manuscript to be well
written, illustrated and the underlying science robust. As such, my comments below correspond to minormoderate

revisions.
I'm not sure you need to state your main conclusions at the end of the Introduction.
#Response#: The sentence in question has been removed.
Line 104: If you're going to mention the corner period of the Trillium Compact, you might as well do the same for the LE-3Dlite (1 Hz I believe).
#Response#: Added, as suggested.
Line 111: Do you think it is correct to characterise these methods as modelling surface waves as plane waves? In any case, you should include a few references at the end of this sentence to back up your claim.
#Response#: No, as rightly pointed out, it's an incorrect characterisation, ignoring, for instance, spherical
wave and normal mode approaches. What we meant to convey was that off-great-circle path propagation and
multipathing should be accounted for in analysis of teleseismic surface waves recorded on regional seismic
arrays. The amended statement now reads:
Accounting for off-great-circle path propagation and multipathing of surface waves is necessary in developing
high-fidelity seismological Earth models (e.g., Evernden, 1953; Capon, 1970; Lay and Kanamori, 1985;
Woodhouse and Wong, 1986; Friederich et al., 1994; Laske, 1995; Spetzler et al., 2001; Forysth and Li,
2005). Motivated by observations reminiscent of wave interference across regional seismic arrays, Forsyth et
al. (1998) cast the wavefield as the superposition of two interfering plane waves. With the caveat that the
validity of the plane wave assumption depends on the array aperture, this modelling approach demonstrably
improved data fitting relative to single plane wave representations (e.g., Li et al., 2003). So-called two-planewave
tomography has since usefully illuminated lithospheric structure across diverse tectonic settings (e.g.,
Weeraratne et al., 2003; Yang and Forsyth, 2006b; Harmon et al., 2009; O'Donnell et al., 2019).
Lines 114-116: While I largely agree with you, the two-plane wave method has limitations in terms of array aperture, so it would be worth mentioning this (you do touch on it later).
#Response#: The above amended statement now includes a reference to this limitation, which we describe
in more detail – including a mitigating strategy – in the Two-Plane-Wave Tomography section.
2
Line 123: It would be useful to have a bit more detail on the approach used to distill the event catalogue down to 142 events.
#Response#: We've updated the text in the relevant section to describe (i) the visual perusal of record
sections in four broad frequency bands as a means of assessing Rayleigh wave signal quality; and (ii) the
omission of aftershocks from consideration due to redundancy:
We applied the two-plane-wave method to fundamental mode Rayleigh waves recorded on the Lake Eyre

Basin, AusArray SA, SNAKEY, Australian National Seismic Network and Australian Seismometers in
Schools arrays over the period August 2019 to September 2023. To garner good quality waveforms for analysis,
we screened earthquakes with magnitudes ≥6.0 occurring within the epicentral distance range $25^\circ < \Delta < 60^\circ$
of the composite seismic array. If multiple earthquakes clustered in space and time (i.e., a mainshock and aftershocks),
we screened only the mainshock. Following instrument response deconvolution, a visual perusal of
vertical-component record sections in four filter bands (full bandwidth; 25-50 s; 50-100 s; 100-125 s) was used
to identify candidate earthquakes yielding high signal-to-noise ratio Rayleigh waves. The lower magnitude
threshold was subsequently iteratively reduced to 5.1 to secure improved azimuthal earthquake illumination
from the spreading ridges west and south of Australia. Of many hundreds of earthquakes screened, 142 were
ultimately earmarked for tomography (Figure 2a).
Ultimately, the screening is nothing more sophisticated than manually trawling through the many candidate
earthquakes in the stated epicentral distance range and retaining those with decent signals.
Lines 149-152: It may be worth emphasizing that the uncertainty has more meaning in a relative
rather than absolute sense, since your knowledge of data uncertainty and prior model
uncertainty is limited (i.e. not much constraint on absolute values).
#Response#: We agree wholeheartedly and have added the following statement at line XX:
Given the difficulty in quantifying the uncertainty of the antecedent seismograms, coupled with our adoption
of a particular starting model and regularisation in the tomography, we emphasise that the inferred
uncertainty has more meaning in a relative rather than absolute sense.
Figure 4: Is any noise added to the synthetic dataset used for checkerboard reconstruction?
This can be useful to apply, since it provides a dose of realism to the recovery.
#Response#: We have added noise to the checkerboard recovery tests; see updated Figure 4 and caption.
Figure 6: I don't see the plot numbers (i), (ii) etc in the figure. Also, on lines 177-178, it
suggests that there is an explicit Moho (?) included in each inversion, but I don't see much
evidence of it here, but perhaps it's averaged out in these plots? I guess one caveat is that
since the minimum period is around 30s, there is little constraint on the crust, which is why
velocity is almost constant in the upper 25 km (which in reality it won't be), then there is a
strong gradient to mantle Vs values between 25-50+ km depth. This is not really suggestive
3
of an explicit Moho.
#Response#: We've removed the reference to plot numbers (i), (ii), etc; these were leftovers from an
earlier iteration of the plot.
The original Moho parameterisation statement perhaps misleading implied that the inferred Moho had
to fall within the AusMoho °æ5km window, whereas Bayhunter in fact uses a prior Moho value as a soft

initialisation rather than a hard constraint. We've revised the statement accordingly:
Moho depths were initialised to be within ±5 km of the Australian Moho model (Kennett et al., 2023),
but the algorithm was free to subsequently explore values outside this range.
Incidentally, I (JPOD) have modified Bayhunter to implement a harder Moho constraint, but this introduces
additional parameters (e.g., to implement a sharp or gradational Moho), and either way, some degree
of modelling artefact is unavoidable.
Yes, our period range means that we have little constraint on upper-to-mid crustal structure. We've
added the following sentence to the Figure 6 caption to make this clear to readers:
Upper-to-mid crustal structure is not well constrained by our phase velocity period range and should not
be interpreted.
Figure 7 Moho map – it would be useful to see a comparison with AusMoho, perhaps in the supplementary information. Also, only part of the colour bar range is used, so I would suggest recallibrating to use all colours available.
#Response#: The colour bar range was in fact designed to facilitate the comparison with AusMoho: the
superimposed colour-coded symbols on the Moho depth map (squares for passive seismic stations; triangles
for active seismic survey locations) are the "raw" data used to construct AusMoho, and the colour bar range
is necessarily wide to capture the AusMoho variation. Granted, the colour-coded symbols are small, but
that is to prevent symbol overlap. We've amended the Figure caption to emphasise that the superimposed
symbols reflect AusMoho, and believe that this presents a reasonable comparison as is.
Lines 208-210: Given the uncertainties in the Vsv models, which should be quantifiable via the
transD inversion approach, is such a tight classification really justified? What happens if you choose a slightly different range of numbers?
#Response#: We'd again emphasise that lateral velocity variations are better determined than are absolute
values. Had we "tuned" the tomography with a slightly different regularisation, the absolute velocity
ranges proffered here would have commensurately shifted slightly. As is, the absolute values selected are (i)
in the ballpark of what might be expected for geological units of these ages; and (ii) reflective of anticipated
lateral geological patterns (Archean cores transitioning to bounding Proterzoic units). We have amended the
original text to this:

Given the suite of parameterisation and regularisation choices that cumulatively influence the two-planewave
tomography and subsequent VSV model development (described as all the little choices by Fichtner et

al., 2025), we emphasise that relative velocity variations are better constrained than are absolute values. Accordingly,
while we here interpret VSV of ~4.6 km/s (equivalent to ~2% above isotropic AK135 values) and
higher as characterising cratonic core lithosphere, and VSV of ~4.55–4.6 km/s (equivalent to ~AK135+1%
< VSV < ~AK135 +2%) as characterising cratonic margin lithosphere, it is the relative VSV variations and
spatial patterns that primarily inform our interpretation.

Paragraph stating with line 306: The exact nature of the lithosphere between the Gawler Craton and Curnamona Province is an interesting question. As the authors point out, given the rifting and associated processes that occurred, one might expect it to be lower velocity or otherwise more distinct from the adjoining regions. It is a fairly narrow region on the surface however, so if it in fact did feature lower velocities, would the tomography used in this study be able to resolve it, particularly at greater depths? At crustal scale, Young et al (2013) did find that the Gawler and Curnamona were characterised by distinct low velocities – see https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/grl.50878
while Rawlinson et al. (2016) found that the mantle lithosphere beneath the cores of the craton and province were characterised by higher velocities - see https://www.sciencedirect.com/science/article/pii/S004019511500671X

#Response#: Granted it's a narrow region, but we'd argue that our phase velocity resolution tests indicate
that structure of that dimension at upper lithospheric mantle depths is resolvable. We certainly acknowledge
the inferences from the studies mentioned, but it's also worth pointing out the inferred deeper (100-200 km-ish
depth) anomalously seismically-fast structure in the same region in the Rawlinson et al. (2016) model, i.e.,
structure that is largely indistinguishable from the Gawler Craton and Curnamona Province (see our red box
superimposed on Figure 12 from Rawlinson et al. (2016) below). We already state that the resistivity and
xenolith data at least loosely support out contention. Future seismic deployments may well prove us wrong,
but for now, we'd argue that the jury is out. We have amended the original text to this:
The section of lithosphere between the Gawler Craton and Curnamona Province is seismically fast with
characteristics indistinguishable from the cores of the Gawler Craton and Curnamona Province. Although
a narrow region, our phase velocity resolution analysis at 50 s (primarily sensitive to upper lithospheric
mantle depths) indicates that structure of wavelength comparable to the dimension of the separation can be
resolved (Figure 4). The electrical resistivities are also moderate to high, and mantle model ages are generally
>2500 Ma, although the latter are not well constrained in this region (Wade et al., 2024). On the other
hand, Rawlinson et al. (2016) suggest that there is no evidence in the lithospheric mantle that the Gawler

Craton and Curnamona Province are currently connected based on a teleseismic relative arrival time body
wave tomography developed using the WOMBAT data...
5
Recommendation: Revisions Required
6
Reviewer E
Here's the review of the manuscript entitled "The deep lithospheric structure of the South Australian Craton
revealed by teleseismic Rayleigh wave tomography." The manuscript is well organized, internally consistent,
and flows logically from tectonic context to seismic modeling and lithospheric interpretation. The text structure
within sections is generally strong, but some paragraphs are excessively long, especially in the Discussion,
which reduces readability. In the following, I give some comments and refer directly to the corresponding
lines in the manuscript. Overall, my recommendation is a Minor revision for this manuscript.
In the Abstract section: The flow from the cratonic lithosphere imaging to its metallogenic
implications is logical, but the transition between these two themes is abrupt. A brief linking
sentence could help bridge the shift from geophysical to mineral exploration implications, provided
word count limits permit.
#Response#: We appreciate where the Reviewer is coming from, but the stipulated Abstract word limit
is 200 words, and we're at 200 words as is.
2) In the Introduction Section:
2-a) The transition between the first paragraph (geological background/tectonic framework)
and the second (seismic velocity context) could be smoother. Consider adding a linking sentence
such as: "To investigate how these long-lived processes are reflected in present-day
lithospheric structure, seismic velocity models provide a key diagnostic tool."
#Response#: We respectfully disagree on the need for this as we feel that it'd more or less duplicate
information in the existing sentence:
Seismic velocity is sensitive to temperature and bulk composition, so velocity variations can be used to
map lithospheric architecture (e.g., older, depleted or younger, fertile domains in the lithospheric mantle).
2-b) The final paragraph is effective, but mixing the methodological details (arrays, periods,
etc.) with the conceptual outcome diminishes its impact. Consider separating the paragraph's
content into two distinct sentences to clearly present the data/method first, followed by the
application/outcome. This structural change will significantly enhance readability and clarity.
#Response#: The concluding sentence of this paragraph has been removed in line with Reviewer D's
suggestion, so this methodological/outcome juxtaposition has been lessened.
2-c) Use commas more sparingly; several long sentences can be split for easier reading.
3) The Tectonic Framework section:
3-a) The first paragraph effectively defines the cratonic units, but the description of margins
is lengthy and could be condensed. Hence, summarize margin characteristics before moving

to mineralization history.
#Response#: We describe the Gawler Craton's southern, western, northern and eastern margins, and
abutting units in one (admittedly long) sentence. We, respectfully, don't think it can be meaningfully condensed
beyond what it already is:
The southern edge of the Gawler Craton is defined by the continental margin developed during the Jurassic-
Cretaceous separation of Australia and Antarctica, but the craton's western margin, which abuts the mid-
Neoproterozoic - Early Paleozoic Officer Basin, northern margin, which abuts the Meso-Neoproterozoic Musgrave
Province, and eastern margin, which abuts the Neoproterozoic-Middle-Cambrian Adelaide Superbasin,
are obscured by sedimentary cover and are less well defined (e.g., Preiss et al., 1993; Preiss, 2000;Wade et
al., 2008; Reid and Hand, 2012; Lloyd et al., 2020b; Agrawal et al., 2022).
3-b) The second and third paragraphs shift to IOCG deposits and geodynamic context; this transition is slightly abrupt. A linking statement such as "Understanding the tectonic and metallogenic evolution of the Gawler and Curnamona regions provides essential context for interpreting lithospheric structure" would improve coherence.
#Response#: Although perhaps semantics, we'd contend that our goal is the opposite: mapping lithospheric
structure as a means of understanding the tectonic and metallogenic evolution of the Gawler and
Curnamona regions. We have recast the sentence and use it as a useful concluding statement at the section
end:
"Constraining the lithospheric structure of the South Australian Craton provides essential context for interpreting
the tectonic and metallogenic evolution of the Gawler Craton and Curnamona Province."
3-c) This section intermixes tectonics and mineralization discussion. Since mineralization is later related to lithospheric architecture, consider highlighting the last paragraph explicitly such as "Implications for metallogeny", to improve internal organization.
#Response#: The section paragraph now constitutes a "Mineralisation" subsection and the third paragraph
has been promoted to a separate "Lithospheric architecture and IOCG formation" section.
4) Rayleigh Wave Phase Velocity Maps section: The explanation of the resolution matrix results is clear but somewhat repetitive. Consider condensing the sentences on decreasing resolution with the period into one concise statement.
#Response#: The modified section has been condensed to:
Companion Figure 4 shows the resolving capability of the inversion. As expected, the patterns mirror
that of the phase velocity uncertainty maps: resolution is best where the concentration of seismic stations
is greatest, and naturally degrades with increasing period as wavelengths lengthen. The latter is hastened by
the progressively decreasing number of seismograms constraining the two-plane-wave tomography at periods

>100 s (Figure 2d). Within the region enclosed by the white polygon, the resolution matrix indicates that
the morphology and amplitude of velocity anomalies of length scale 1.5◦ and greater are faithfully imaged at
25 s period. At 125 s period, meanwhile, structure of length scale >2◦ is resolved, albeit with more anomaly
amplitude diminution and smearing relative to shorter periods.
5) Shear Wave Velocity Model section: The section opens well but could benefit from an introductory
sentence summarizing purpose: “We next inverted the phase velocity maps for
shear-wave velocity structure to derive a pseudo-3D model of southern Australia.”
#Response#: Added:
“We next inverted the phase velocity maps to derive a pseudo 3D shear wave velocity model of southern
Australia”.
6) Discussion section:
6-a) sub-section 7.1: The Moho: Clear and concise; well-positioned. However, it could end
with a short linking phrase to the next subsection “Having established the Moho depth pattern,
we next examine the lithospheric mantle structure”).
#Response#: Added:
We next examine lithospheric mantle structure.
6-b) sub-section 7.2: Lithospheric Mantle Structure: Excellent synthesis but overlong; consider
subdividing into thematic paragraphs (core features, margins, anisotropy correlation) for
easier reading. The argument is strong but buried in dense prose.
#Response#: At three reasonably short paragraphs that are already thematic (1. introducing our seismic
proxies for interpreting cratonic core and cratonic margin lithosphere; 2. describing the model based on
these proxies; 3. anisotropy correlation), we respectfully disagree that rearrangement or explicit subsection
headings are actually needed.
6-c) sub-section 7.3: Comparison with Mantle Xenolith, Resistivity, and Isotopes: This is well
argued but could benefit from clearer paragraphing — currently, some transitions (e.g., from
Gawler Craton to Curnamona Province) are abrupt. Adding subtopic openers (“In the Curnamona
Province. . . ”) improves readability.
#Response#: We have added subsection headers to signpost the section.
6-d) subsection 7.4: Lithospheric Signature of IOCG Deposits: A strong and coherent conclusion.
The logical progression (previous models, then new findings, next explain implications)
works well. Only minor condensation suggested (the long summary of past MT studies could
be reduced).

#Response#: MT models are referred to in only two sentences in this section, so we’d prefer to keep as is.
7) Minor comments:
Line 14: edit “recorded on three recent broadband” to “recorded by three recently deployed
broadband seismic arrays.”
#Response#: Changed as suggested.

Line 36: edit “so velocity variations can be used to map lithospheric architecture” to “thus, velocity variations can be used to map lithospheric architecture.”
#Response#: Changed as suggested.
Good luck
Shirzad
Recommendation: Revisions Required

Completed: 2025-12-15 11:19 AM
Recommendation: Accept Submission

Completed: 2025-11-10 01:17 PM
Recommendation: Accept Submission