Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

4th Sep 20

Dear Dr Isse,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "Seismic evidence for residual mantle beneath the Ontong Java Plateau". It has now been seen by 3 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

In addition, please ensure that the revised manuscript addresses the following editorial concerns:

Provide a compelling and robust characterisation of the subsurface structure beneath the Ontong Java Plateau

Fully justify your use of surface waves to derive insights on the depth of the lithosphere-asthenosphere boundary, or adapt your approach accordingly

Discuss the limitations, caveats and challenges associated with your support of a plume-related formation hypothesis for the Ontong Java Plateau (including alternative interpretations), and soften your conclusion in this regard if necessary

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Joe Aslin

Associate Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: Policy requirements

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

In the event that your manuscript is accepted, we will provide detailed guidance on our journal policies and formatting. You may however wish to ensure that the manuscript broadly complies with our house style at this stage. See our style and formatting guide Communications Earth & Environment formatting guide for reference. The main points are as follows:

* ABSTRACT: less than 150 words and accessible. It should include the background and context of the work, the phrase ‘Here we’ (present/show/suggest) to indicate where the description of your own work starts, and then the methods/data, main results and conclusions of the paper.

*The text must be split into:

- INTRODUCTION (<1000 words): includes the background and rationale for the work. The final paragraph should be a brief summary of the main results and conclusions. The results of the current study should only be discussed in this final paragraph.
- RESULTS: split into subheaded sections; ensure that the subheadings are no longer than 60 characters including spaces.
- DISCUSSION: without subheadings.
- METHODS: split into subheaded sections; ensure that the subheadings are no longer than 60 characters including spaces

* SUPPLEMENTARY INFORMATION should be organised logically, with all items labelled as one of the following item types, and cited in the main article:
- Supplementary Figures, labelled and referred to as i.e. Supplementary Figure 1 throughout both the Supplementary Information and the main text
- Supplementary Tables, labelled as above
- Supplementary Notes, labelled as above
- Supplementary Discussion
- Supplementary Methods
- Supplementary References

Data: All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, and a list of examples, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.
In addition, Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).
In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This paper presents new seismic tomographic results of the upper mantle in the OJP region, using surface waves collected at the "OJP array" and other stations. The main finding was that the LAB beneath the OJP is deeper than the surrounding region, and they explain the thickened lithosphere as residue material from a thermochemical plume that formed the OJP. The work itself is solid and interesting but I found results not convincing as presented in the manuscript.

The authors claim that the most remarkable feature is high velocity only in the central part of OJP down to 150km (Figure 2) but inspection of the Figure shows that was not the case – a much broader region northwest of OJP has the same velocity (blue at 50, 75 and 100 km and then also becomes red at 150 km). This is not consistent with LAB depth calculations in Figure 3 – the definition of the LAB using depth gradient may not be meaningful, it does not represent the most dominant feature in tomographic velocity structure. This is also confirmed in Extended Data Figure 4 where the depth of the LAB (circles) determined using the vertical gradient method do not recover the input structure, and, the calculated depths are most likely very sensitive to noises at different periods, unless you force certain curvatures in depth parameterization.

Another issue is radial anisotropy. Love and Rayleigh waves have different depth resolutions, depth smearing is usually more severe for Love waves than Rayleigh waves. The preferred SH and SV models depend a lot on data coverage and regularization chosen for Love and Rayleigh. Simply taking the difference between the preferred models as anisotropy is not reliable. The results and respective resolutions need to be discussed.

The trade-off between crust and mantle and how it is handled in the inversion should be discussed. For example, a slower crust in the model may lead to a faster/thicker mantle lid.

The main disadvantage of using surface waves for LAB studies is that they are not particularly sensitive to the depth of the LAB. Unlike body waves reflected or converted at the boundary, the depth resolutions of surface waves are not good enough to determine the depth of the LAB. If the lithosphere is indeed substantially thicker, the proposed explanation (melt residue) is a possibility, but I do not understand how that rules out passive upwellings at the ridge, the reasoning was very vague (line 114-118). There are several regions in the Pacific where thicker lithosphere have been reported (e.g. Rychert et al 2012), some comparisons and discussions will be needed.

Reviewer #2 (Remarks to the Author):

Seeing the results of a much more thorough examination of the mantle beneath the Ontong Java Plateau than the tantalizing work of the Northwestern group two decades ago is most welcome and fascinating. In general, the manuscript is well-written, logical, and lucid, although a full-length paper describing the data, methodology, and results in much greater detail should follow this manuscript.

Implications of a thermomechanical plume, which has been proposed by many in the past, need to be addressed in the manuscript. For example, it is well documented that OJP's minimal uplift and subsidence history does not agree with either observations or models from other LIPs and hotspots – how do the authors explain this? Also, in contrast to other LIPs and hotspots, the geochemistry of OJP basalts are extraordinarily homogeneous – do the authors have an explanation for this? Other challenges for a thermomechanical plume model for OJP include the absence of an obvious hotspot source or track, high degrees of melting at shallow upper mantle depths, low water contents of basalts, and enrichment of platinum group elements in basalts – how do the authors reconcile these observations with their plume model?

Line-by-line and figure comments

Lines 2-3: The title doesn't do justice to the main conclusion of the manuscript; something like 'Seismic evidence for thermomechanical mantle plume underplating of the Ontong Java Plateau' would seem more apt.

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Lines 37-40: How strong is this evidence, e.g., could Jurassic oceanic crust have been entrained by melt flowing toward the center of an impact, and how much do we know about extraterrestrial impact signatures from deep-ocean impacts? Regarding the latter, to paraphrase an old adage, absence of evidence does not constitute evidence of absence, and why hasn't anything more than an AGU abstract been published on this subject in the past decade?

Line 60 onward: Klosko et al, EPSL, 2001 reported shear wave splitting results from OJP; these should be addressed in the manuscript.

Figure 1. What is the red line? Does OJP actually encompass some of the much younger Caroline Islands chain? The OJP outline defined in the ODP Leg 192 volumes and subsequent publications

(e.g., reference 10) is the best outline/definition of the feature to date. What is the reference for the location of the Caroline hotspot?

Figure 2. The OJP outline should be replaced with the published outline as in Figure 1.

Figure 3. The locations of plate boundaries (green and red dashed lines) need to be updated, as they're much more complex than shown in the figure. See DeMets et al, GJI, 2010 and Argus et al, G3, 2011 (the latter's Figure 2).

Figure 4. Inserting key parameters on the figure itself would make it a better summary.

Extended Data Figure 1. See comments on OJP outline for Figures 1 and 2.

Mike Coffin

Reviewer #3 (Remarks to the Author):

Overall, this is an interesting and well-written paper. Ontong Java is a very important location to study, image and understand because its formation is enigmatic, and there are broad implications for our understanding of continent formation and the evolution of the planet.

Ontong Java is relatively remote, located beneath the oceans, and therefore tight constraints have proven challenging up until this point. This is a unique and important result from a large ocean bottom deployment, providing high resolution imaging of the lithosphere and the asthenosphere beneath the plateau for the first time.

I have several questions detailed below regarding resolution of both isotropic velocities and also radial anisotropy, in particular regarding depth resolution, and also the possibility of mapping the slab into thickened lithosphere. However, once these are answered I think this is a nice result that should be published.

Line 30 - the first seismic evidence of a plume? It is best to avoid claims of first, as there is almost always someone who thinks they were first. Stern et al., 2020 proposed a similar idea for Hirurungi, and by default OJP. Maybe just change tone.

Line 66 - 69 - It would be useful to compare 1-D velocity profiles with those from 'normal' ocean lithosphere.

Could absolute velocity be somehow affected by anisotropy?

Why is there strong radial anisotropy down to 250/300 km depth in north (section b)? Is there a tectonic reason?

Extended data figure 4 - it seems like the spline/smoothing applied introduces a bias to the depth of the negative gradient. It is good that the authors acknowledge this, but this test does not fully address uncertainty in depth owing the broad depth sensitivity kernels of the waveforms. Another way to say this is - there are lots of other shapes that could fit the data, and the estimates for error in depth are much larger than the 5 and 15 km reported by the authors. Some of this could probably be alleviated by looking at phase velocities - which are robust in comparison to the semi-arbitrary shape of shear velocities in depth.

Why are the petrological velocities for many peridotites higher than those here, and garnet-pyroxenes so much higher? Are the garnet pyroxenes not thought to be in abundance? Could it be a smoothing/damping problem? What would you need to do to match the petrology and could that still fit your data? Also, could it be related to anisotropy - horizontal alignment? I agree that horizontal alignment does not seem strong in the plateau in your result, but the depth resolution of radial anisotropy is typically poor (Rychert & Harmon, 2017). The different depth sensitivity of Love and Rayleigh waves impart artefacts. This is a problem in particular when interpreting deep positive radial anisotropy - the kernels push the anisotropy there artificially. So, this part of the argument is tenuous without more information/testing.

Fig 3 - do you mean red dashed line for the plate boundary? Where is the slab? Are you sure you are not mapping the slab into a thickened lithosphere? The slab location would be more evident by plotting earthquakes in the X-sections and also by doing a structural resolution test where the thicker lithosphere is not included, but a nearby slab is included. Then you would be able to see the impact of the slab on the tomography.

The authors correct for crust using CRUST1.0. What does it look like in OJP? What if it is not correct? Could you incorporate more recent information. There was a P-to-S receiver function study from similar authors (Tonegawa et al., 2019).

The Stern 2020 science advances paper suggests extreme radial anisotropy in the lithosphere of Hikurungi Plateau. They interpret the finding in terms of formation via a plume. The event that formed Hikurungi is often thought to be the same event that formed Ontong Java. Why would Hirurungi have high radial anisotropy shallow and OJP have it deep? Were they not formed in the same event?

It might be helpful to show the phase velocities that the result is based on somewhere.

There are probably some good Phipps-Morgan papers modelling the formation of viscous roots that should be included. The result is broadly consistent with relatively thick lithosphere beneath hotspot locations, regardless of the age of the seafloor (Rychert et al., 2018).

The lead up to the plume hypothesis is good. I believe that the event was not purely thermal. Are there any other hypothesized mechanisms of formation? Crater impact, etc.? Also, out of curiosity how did the plume form this massive plateau without making an ocean island?

Rychert, C. A., & Harmon, N. (2017). Constraints on the anisotropic contributions to velocity discontinuities at approximate to 60 km depth beneath the Pacific. Geochemistry Geophysics Geosystems, 18(8), 2855-2871. https://doi.org/10.1002/2017gc006850

Tonegawa, T., Miura, S., Ishikawa, A., Sano, T., Suetsugu, D., Isse, T., et al. (2019). Characterization of crustal and uppermost-mantle seismic discontinuities in the Ontong Java Plateau. Journal of Geophysical Research: Solid Earth, 124, 7155–7170.

https://doi.org/10.1029/2018JB016970

Stern, T., Lamb, S., Moore, J. D. P., Okaya, D., & Hochmuth, K. (2020, May). High mantle seismic P-wave speeds as a signature for gravitational spreading of superplumes. Science Advances, 6(22). https://doi.org/ARTN eaba7118

Rychert, C. A., Harmon, N., & Tharimena, S. (2018). Seismic Imaging of the Base of the Ocean Plates. In H. Yuan (Ed.), Lithospheric Discontinuities (pp. 71-87). AGU Monographs.

Rychert, C. A., Harmon, N., & Armitage, J. (2018). Seismic imaging of thickened lithosphere resulting from plume pulsing beneath Iceland. Geochemistry Geophysics Geosystems. https://doi.org/10.1029/2018GC007501

31st October 2020

We have revised the manuscript by the following the reviewers' comments as mentioned below and changed the title to “Seismic evidence for thermochemical mantle plume underplating beneath the lithosphere of the Ontong Java Plateau,” based on comments from reviewer #2. We divided the Extended Data Figs. 1–4 in the main manuscript into Supplementary Figures to conform to the journal formatting requirements.

For our responses to the reviewers, their comments are shown in italics and our replies are in bold. Line numbers in responses correspond to those in the revised manuscripts.

Response to reviewers:

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This paper presents new seismic tomographic results of the upper mantle in the OJP region, using surface waves collected at the “OJP array” and other stations. The main finding was that the LAB beneath the OJP is deeper than the surrounding region, and they explain the thickened lithosphere as residue material from a thermochemical plume that formed the OJP. The work itself is solid and interesting but I found results not convincing as presented in the manuscript.

The authors claim that the most remarkable feature is high velocity only in the central part of OJP down to 150km (Figure 2) but inspection of the Figure shows that was not the case – a much broader region northwest of OJP has the same velocity (blue at 50, 75 and 100 km and then also becomes red at 150 km).
This is not consistent with LAB depth calculations in Figure 3 – the definition of the LAB using depth gradient may not be meaningful, it does not represent the most dominant feature in tomographic velocity structure.

We assume that we should interpret 'much broader region northwest of OJP has the same velocity' in the reviewer's comment as 'much broader region northeast of OJP has the same velocity.'

The blue region at depth of 50–100 km represents normal oceanic lithosphere and faster than the asthenosphere beneath both the OJP and Nauru basin (northeast of the OJP), where fast velocities in this depth range are not anomalous (note that the colour scale of Fig. 2 is the absolute velocity, not velocity anomalies). To avoid confusion, we clearly describe that the blue region at depths shallower than 100 km is normal lithosphere; only beneath the central OJP does the fast velocity is extend to a depth of 150 km. Please see L67–L74.

Reliability and possible bias in the LAB depth determined by vertical velocity gradient are discussed in Supplementary Note 1. The LAB depths can be estimated within an error of 10 km for the LAB shallower than 100 km and underestimated for greater LAB depths by 20 km or more. As shown in Supplementary Fig. 2, the estimated LAB depths are 130 km beneath the OJP and 70–90 km (close to the average LAB depth for similar ages in the Pacific) for the region surrounding the OJP. The LAB depth (130 km) beneath the OJP could be underestimated, which indicates that the anomalously thick lithosphere beneath the OJP is robust. The topography of the LAB thus determined represents the characteristics of the lithosphere–asthenosphere system in the Pacific Ocean fairly well (cross sections in Fig. 3) for most of the study region: fast velocity with weak radial anisotropy in the lithosphere and slow velocity with strong radial anisotropy in the asthenosphere. The exception is the anomalously thick lithosphere beneath the OJP.

This is also confirmed in Extended Data Figure 4 where the depth of the LAB (circles) determined using the vertical gradient method do not recover the input structure, and, the calculated depths are most likely very sensitive to noises at different periods, unless you force certain curvatures in depth parameterization.

In this synthetic test, we attempted to use a sharp discontinuity model to show the worst case, which is an unfavourable condition for surface wave analysis with respect to recovery due to a limited vertical resolution.

We carefully examined the resolution and accuracy of the LAB depths estimated from the vertical gradient of the seismic velocity (Supplementary Note 1) using synthetic dispersion data to which random noise was added. The figure shows LAB depths of 70–100 km determined within 10 km from the true values, whereas those deeper than 110 km are shallower than the true depths by more than 10–20 km. The OJP LAB depth of 130 km estimated in this study is robustly deeper than that beneath the Nauru basin (and average Pacific). We also performed experiments by changing the Moho depth in the inversion, but our conclusions on the deep LAB beneath the OJP did not change (Supplementary Note 3).

Another issue is radial anisotropy. Love and Rayleigh waves have different depth resolutions, depth smearing is usually more severe for Love waves than Rayleigh waves. **Using up to 4th mode of the Love and Rayleigh waves, we achieved improvements to the depth resolution for anisotropy. See Supplementary Fig. 6.**

The preferred SH and SV models depend a lot on data coverage and regularization chosen for Love and Rayleigh. Simply taking the difference between the preferred models as anisotropy is not reliable. The results and respective resolutions need to be discussed.

As you have mentioned, the lateral resolution of SH and SV is different (see Supplementary Fig. 8). To compensate for the difference in the tomographic map, we applied a 4º and 7º spatial filter to the isotropic and anisotropic structure, respectively in the map. We added information on this on L235–238. We forgot to apply the 7º spatial filter for radial anisotropy in Fig.3; we therefore corrected this in the revision.

The trade-off between crust and mantle and how it is handled in the inversion should be discussed. For example, a slower crust in the model may lead to a faster/thicker mantle lid.

In this inversion, the velocity of the crust is also inverted, although the Moho depth is fixed to the initial model. We added information on this on L232−233. To assess the mis-location of the Moho depth, we added synthetic tests with a changing Moho depth in the initial model (L233–234, Supplementary Note 3, and Supplementary Fig.

9). While there is a trade-off between the crustal and mantle structures as the reviewer suggests, it does not change our conclusion that the LAB depth between the OJP is significantly greater than that beneath the surrounding region.

The main disadvantage of using surface waves for LAB studies is that they are not particularly sensitive to the depth of the LAB. Unlike body waves reflected or converted at the boundary, the depth resolutions of surface waves are not good enough to determine the depth of the LAB.

We failed to image the LAB using S-receiver function analysis because of the high noise level in OBS horizontal-component records. An SS-precursor analysis is not applicable to most of the OJP region because of the availability of earthquakes and stations(see L241–244). While we agree with the reviewer that the body wave reflection/conversion analysis has a better resolution for a discontinuity depth than surface wave analysis, the surface wave approach has sufficient depth resolution to obtain the deep LAB (thick lithosphere) beneath the OJP, as presented in Supplementary Note 1.

If the lithosphere is indeed substantially thicker, the proposed explanation (melt residue) is a possibility, but I do not understand how that rules out passive upwellings at the ridge, the reasoning was very vague (line 114-118).

In the passive model, there is no reason to thicken the lithosphere as OJP magma should have been generated and completely outpoured via decompression melting of eclogite with a low solidus temperature. Substantial partial-melting of mantle peridotite does not occur in the passive model due to an absence of high temperature anomaly sufficient to melt the peridotites. As a result, no melt residues should remain after OJP emplacement. The lithosphere should remain as normal oceanic lithosphere after OJP emplacement. See L165–169.

There are several regions in the Pacific where thicker lithosphere have been reported (e.g. Rychert et al 2012), some comparisons and discussions will be needed.

We added comparisons on L80–82. Recent SS-precursor studies (e.g., Tharimena et al., 2017, JGR) found that a region in the north-western Pacific has a deep LAB depth of 120 km, which is shallower or similar to the estimated LAB depth beneath the OJP.

Reviewer #2 (Remarks to the Author):

Seeing the results of a much more thorough examination of the mantle beneath the Ontong Java Plateau than the tantalizing work of the Northwestern group two decades ago is most welcome and fascinating. In general, the manuscript is well-written, logical, and lucid, although a full-length paper describing the data, methodology, and results in much greater detail should follow this manuscript.

Implications of a thermomechanical plume, which has been proposed by many in the past, need to be addressed in the manuscript.
For example, it is well documented that OJP's minimal uplift and subsidence history does not agree with either observations or models from other LIPs and hotspots – how do the authors explain this? Also, in contrast to other LIPs and hotspots, the geochemistry of OJP basalts are extraordinarily homogeneous – do the authors have an explanation for this?

We added Kerr et al. (2007) as a reference for the thermochemical plume hypothesis. We also added sentences on L143–160 and L189–194.

Other challenges for a thermomechanical plume model for OJP include the absence of an obvious hotspot source or track, high degrees of melting at shallow upper mantle depths, low water contents of basalts, and enrichment of platinum group elements in basalts – how do the authors reconcile these observations with their plume model?

We agree that non-plume hypotheses, such as the passive upwelling of dense eclogite or a meteorite impact, stand due to the absence of an obvious hot spot track as a plume-tail trace. We left these possibilities open in the manuscript (L161–L163). Regarding constraints from OJP lava compositions, we added the sentence "the assumption of 20–30% melting of homogeneous anhydrous peridotite is not a robust assumption for modelling lava compositions" (L149–152), avoiding responses to individual geochemical characteristics (e.g., low-H2O, high-PGE contents, and a flat REE pattern), as these are beyond the scope of this manuscript. Instead, we point out

the fundamental question of whether the current geochemical data obtained from the uppermost 3–4 km section of entire >30 km crust are representative or not (L155–158).

Line-by-line and figure comments

Lines 2-3: The title doesn't do justice to the main conclusion of the manuscript; something like 'Seismic evidence for thermomechanical mantle plume underplating of the Ontong Java Plateau' would seem more apt.

Thank you for constructive suggestion. We have changed the title.

Line 17: delete 'mass extinction' with 'extinctions'; 'mass extinction' has a defined meaning of >75% of Earth's estimated species becoming extinct in a geologically short interval, and OJP is not associated with a mass extinction.

We replaced 'mass extinction' with 'extinctions.'

Lines 18, 34: replace 'volcanic' and 'volcanism' with 'magmatic' and 'magmatism', respectively; the bulk of OJP's crust appears to be plutonic, not volcanic.

We replaced 'volcanic' and 'volcanism' with 'magmatic' and 'magmatism.'

Lines 37-40: How strong is this evidence, e.g., could Jurassic oceanic crust have been entrained by melt flowing toward the center of an impact, and how much do we know about extraterrestrial impact signatures from deep-ocean impacts? Regarding the latter, to paraphrase an old adage, absence of evidence does not constitute evidence of absence, and why hasn't anything more than an AGU abstract been published on this subject in the past decade?

Following your suggestion, we introduced the impact hypothesis as a third hypothesis. See L38–41. In the discussion, we discuss this hypothesis, L143–176. While it is difficult to reject the impact hypothesis based only on the seismic image from this study, we consider that the impact hypothesis is less plausible than the thermochemical plume based on recent results from geological/geochemical studies, as mentioned in the text. See L170–176.

Line 60 onward: Klosko et al, EPSL, 2001 reported shear wave splitting results from OJP; these should be addressed in the manuscript.

We added this to the manuscript. See L52–53

Figure 1. What is the red line? Does OJP actually encompass some of the much younger Caroline Islands chain? The OJP outline defined in the ODP Leg 192 volumes and subsequent publications (e.g., reference 10) is the best outline/definition of the feature to date. What is the reference for the location of the Caroline hotspot?

We changed OJP to ODP Leg 192.

The location of the Caroline hotspot is from Steinberger, B JGR 2000.

Figure 2. The OJP outline should be replaced with the published outline as in Figure 1.

We changed this as you have suggested.

Figure 3. The locations of plate boundaries (green and red dashed lines) need to be updated, as they're much more complex than shown in the figure. See DeMets et al, GJI, 2010 and Argus et al, G3, 2011 (the latter's Figure 2).

We changed the plate boundary in the maps to that reported in Birds (2013).

Figure 4. Inserting key parameters on the figure itself would make it a better summary.

We added the shear velocities and anisotropy with a depth axis to the figure.

Extended Data Figure 1. See comments on OJP outline for Figures 1 and 2.

We changed this as you have suggested.

Mike Coffin

Reviewer #3 (Remarks to the Author):

Overall, this is an interesting and well-written paper. Ontong Java is a very important location to study, image and understand because its formation is enigmatic, and there are broad implications for our understanding of continent formation and the evolution of the planet.

Ontong Java is relatively remote, located beneath the oceans, and therefore tight constraints have proven challenging up until this point. This is a unique and important result *from a large ocean bottom deployment, providing high resolution imaging of the lithosphere and the asthenosphere beneath the plateau for the first time.*

I have several questions detailed below regarding resolution of both isotropic velocities and also radial anisotropy, in particular regarding depth resolution, and also the possibility of mapping the slab into thickened lithosphere. However, once these are answered I think this is a nice result that should be published.

Line 30 - the first seismic evidence of a plume? It is best to avoid claims of first, as there is almost always someone who thinks they were first. Stern et al., 2020 proposed a similar idea for Hirurungi, and by default OJP. Maybe just change tone.

Stern et al. (2020) propose a superplume hypothesis. They did not show it as a thermal or thermochemical superplume. While we clearly show that this is a thermochemical plume based on seismic and petrological results, we removed this expression.

Line 66 - 69 - It would be useful to compare 1-D velocity profiles with those from 'normal' ocean lithosphere.

We added the 1-D profiles and dispersion curves of the OJP, Caroline Islands, and Nauru basin in Supplementary Note 2 and Supplementary Figs. 3–4.

Could absolute velocity be somehow affected by anisotropy?

The inversion parameter is Vsv and Vsh; both parameters are well resolved as we used not only the fundamental mode, but also up to 4^{th} higher modes (see Supplementary Fig. 6).Therefore, anisotropy has little effect on the absolute velocity.

Why is there strong radial anisotropy down to 250/300 km depth in north (section b)? Is there a tectonic reason?

Unfortunately, we have no reasonable interpretation yet. This region is beneath the Caroline hotspot and Caroline Islands. Therefore, it may be associated with hotspot activity and/or the construction of the Caroline Islands and Caroline plate. This remains to be solved in future studies.

Extended data figure 4 - it seems like the spline/smoothing applied introduces a bias to the depth of the negative gradient.

We used the inversion method of Tarantola and Vallete, where correlation length may affect the result. We added this information on L220–L228.

It is good that the authors acknowledge this, but this test does not fully address uncertainty in depth owing the broad depth sensitivity kernels of the waveforms. Another way to say this is - there are lots of other shapes that could fit the data, and the estimates for error in depth are much larger than the 5 and 15 km reported by the authors.

We agree that this synthetic test does not fully address the uncertainty. To improve the estimation of uncertainty, we performed a synthetic test. We added a random error, at most, of 0.4% to a synthetic model. The recovered model suggested that one standard deviation is ~15 km. In addition, there should be systematic bias due to the method based on the vertical velocity gradient. For the deep LAB, the depth should be underestimated by more than 20 km, which does not change our conclusion on the deep LAB beneath the OJP. See supplementary Note 1 and Supplementary Fig. 2.

Some of this could probably be alleviated by looking at phase velocities - which are robust in comparison to the semi-arbitrary shape of shear velocities in depth.

We added Supplementary Note 2 and Supplementary Figs. 3–4 to show the 1-D velocity profiles and dispersion curves of the OJP, Nauru Basin, and Caroline Islands.

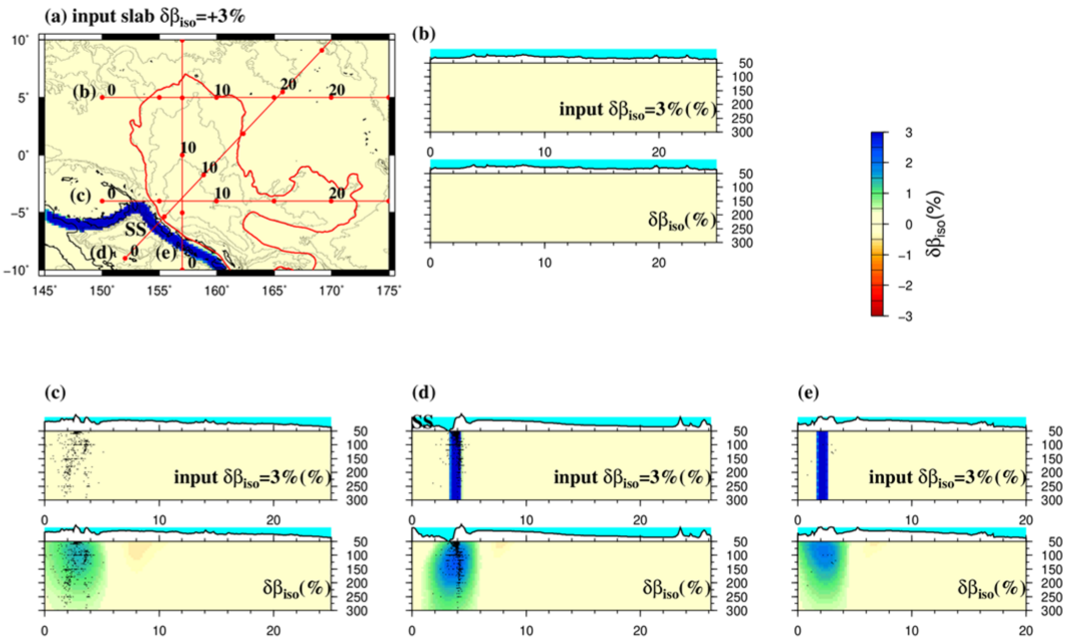
Why are the petrological velocities for many peridotites higher than those here, and garnet-pyroxenes so much higher? Are the garnet pyroxenes not thought to be in abundance? Could it be a smoothing/damping problem?

The petrological velocity of peridotites at depths shallower than 80 km are faster than our seismic results, which is due to the vertical resolution of the inversion. Supplementary Fig. 2c clearly shows that the recovered velocity is slower than the input model in the shallow part.

We think that the amount of garnet pyroxenes is small, such that bulk velocity of the mantle is similar to the peridotite velocity.

What would you need to do to match the petrology and could that still fit your data?
To improve the fit of the shallow part (<60km), shorter period phase velocity measurements are necessary.
Also, could it be related to anisotropy - horizontal alignment? I agree that horizontal alignment does not seem strong in the plateau in your result, but the depth resolution of radial anisotropy is typically poor (Rychert & Harmon, 2017).
We show the depth resolution of the radial anisotropy is sufficient because of the use of higher modes. See Supplementary Fig. 6.

The different depth sensitivity of Love and Rayleigh waves impart artefacts. This is a problem in particular when interpreting deep positive radial anisotropy - the kernels push the anisotropy there artificially. So, this part of the argument is tenuous without more information/testing.
If you use only the fundamental mode of the Love and Rayleigh waves, such artefacts may occur. However, adding up to the 4th higher modes allows the sufficient recovery of the anisotropy. This is because the sensitivity of the higher modes is not only in the deeper part, but also in the shallow part, such that the depth resolution improves (Supplementary Fig. 6).
Fig 3 - do you mean red dashed line for the plate boundary?
Where is the slab?
We removed the red dashed line and plotted the earthquakes that have occurred around the cross-section to show the location of the slab.
Are you sure you are not mapping the slab into a thickened lithosphere?
The slab location would be more evident by plotting earthquakes in the X-sections and also by doing a structural resolution test where the thicker lithosphere is not included, but a nearby slab is included. Then you would be able to see the impact of the slab on the tomography.
We performed a synthetic test assuming a 5% fast velocity slab following your suggestion. The result shows that the slab anomalies are not smeared into the OJP structure.



The authors correct for crust using CRUST1.0. What does it look like in OJP?

The central part of the OJP has a thick crust (~30 km). See Supplementary Fig. 7.

What if it is not correct? Could you incorporate more recent information. There was a P-to-S receiver function study from similar authors (Tonegawa et al., 2019).

We changed the crust model, which does not change the tomographic image in the mantle. See L228–231 and Supplementary Fig. 7.

The Stern 2020 science advances paper suggests extreme radial anisotropy in the lithosphere of Hikurungi Plateau. They interpret the finding in terms of formation via a plume. The event that formed Hikurungi is often thought to be the same event that formed Ontong Java. Why would Hirurungi have high radial anisotropy shallow and OJP have it deep? Were they not formed in the same event?

We mentioned Stern et al. (2020) on L45–47. First, we are not yet sure whether the OJP and Hikurangi (and Manihiki) plateaus were emplaced by a single event. Assuming that the single event hypothesis is correct, the upper mantle structures obtained by Stern et al. (2020) and this study are not inconsistent with each other, as the two studies focus on different depths. The depth of the strong anisotropy (Stern et al., 2020) is immediately beneath the Moho where our model has no resolution. Therefore, strong radial anisotropy just beneath Hikulangi is not inconsistent with the

seismic image presented in this study. On the other hand, the method in Stern et al. (2020) has no resolution for anisotropy in the asthenosphere (well below the uppermost mantle), such that Stern et al. (2020) should not see the strong anisotropy that we found in the asthenosphere.

It might be helpful to show the phase velocities that the result is based on somewhere.

We added this in Supplementary Note 2. Phase velocities for the fundamental mode traveling through the OJP are slower than those outside the OJP because the fundamental mode is sensitive to both crustal and upper mantle structures and dominantly affected by the extremely thick (and slow) OJP crust. On the other hand, faster and thicker lithosphere beneath the OJP is apparent because of the faster velocities of the first higher mode traveling through the OJP than those outside the OJP. This is because the first higher mode is mainly affected by upper mantle velocities, but negligibly affected by crustal velocities.

There are probably some good Phipps-Morgan papers modelling the formation of viscous roots that should be included. The result is broadly consistent with relatively thick lithosphere beneath hotspot locations, regardless of the age of the seafloor (Rychert et al., 2018).

We mention the Phipps Morgan model on L180–182.

We also mention the thick lithosphere beneath the hotspot island on L183–184.

The lead up to the plume hypothesis is good. I believe that the event was not purely thermal. Are there any other hypothesized mechanisms of formation? Crater impact, etc.?

As mentioned in the text, there are three major hypotheses. Our seismological and petrological results indicate that the plume hypothesis, particularly the thermochemical plume hypothesis, is more plausible than the other two hypotheses. We discuss this on L143–176.

Also, out of curiosity how did the plume form this massive plateau without making an ocean island?

We have added discussion on this on L143–149.

14th Jan 21

Dear Dr Isse,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "Seismic evidence for thermochemical mantle plume underplating beneath the lithosphere of the Ontong Java Plateau". It has now been seen by our reviewers, and I include their comments at the end of this message. Reviewers #2 and #3 are the same as in the previous round, reviewer #4 is a new reviewer who was brought in to ensure that the full breadth of required expertise was covered. You will see that some important concerns remain.

We are still interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these remaining concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to again revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

In addition, we will need to see that your manuscript meets the below editorial thresholds in order to consider it further.

Justify the depth resolution of radial anisotropy in light of the concerns raised by Reviewer #3

Further discuss the evidence contrary to the thermomechanical plume model for the formation of the Ontong Java Plateau, in order to place your findings clearly in the context of current understanding

Ensure all statements and assumptions you make are fully justified, or soften or remove them accordingly

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Joe Aslin

Associate Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

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If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

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REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

The authors have addressed the majority of issues raised in my original review. Attached please find an edited/commented version of the revised submission for consideration. As in my review of the original version of the manuscript, the revised manuscript is well-written, logical, and lucid, although a full-length paper describing the data, methodology, and results in much greater detail should follow this manuscript.

Worrisome in the revised manuscript, however, is that the authors selectively use evidence supporting a thermomechanical plume model, but treat contrary evidence (e.g., high degrees of melting at shallow upper mantle depths, low water contents of basalts, enrichment of platinum group elements in basalts) as “beyond the scope of this manuscript.” Thus the revised mansucript would potentially lead readers to the conclusion that a thermomechanical plume model is unequivocally correct. To my mind, the authors need to address all evidence for and against a thermomechanical plume model in order to do justice both to their great new data and to the scientific community, the latter especially in terms of fairness and balance. Review by an independent petrologist/geochemist familiar with the Ontong Java Plateau, but not an author, would likely improve the manuscript considerably.

Mike Coffin

Reviewer #3 (Remarks to the Author):

Overall, the authors have done a good job responding to the reviews.

The depth resolution of radial anisotropy still needs to be better presented and supported.
In Fig. S6 I agree that higher modes push the radial anisotropy a bit deeper, with a better fit to the input than that recovered by the fundamental mode. However, that is not the classic problem with radial anisotropy depth resolution. The problem is that the difference in SH vs. SV sensitivity pushes anisotropy artificially deep. If the real (input) structure has shallow radial anisotropy (say 0 - 100 km) , it is typically recovered much deeper (> 150 km). Using an input model with deep anisotropy (~200 km), as done in the current revision, is not very useful to investigate this problem. What does the recovery test look like if radial anisotropy is shallow (< 100 km)? To be convincing, the recovery test should be shown for an input model with strong, shallow radial anisotropy (< 100 km), also testing a variety of starting models, for fundamental and higher modes.

Also, the manuscript would be improved if more of the responses were reflected as changes to the text, rather than a response to review. As an example, the reasons for not matching petrology - lack of shallow resolution could be added to manuscript to aid the reader. There are several other examples.

Reviewer #4 (Remarks to the Author):

The paper presents robust evidence for the existence of thicker lithosphere beneath the Ontong Java Plateau (OJP); an important conclusion in light of the geodynamic significance of the OJP. I have, however, some reservations about the petrological aspects of the paper, and have noted a few minor editorial corrections.

Lines 35-36: Emplacement of the OJP was not entirely submarine. Drilling at ODP Site 1184 penetrated a thick sequence of pyroclastic rocks that formed through subaerial phreatomagmatic eruptions (Thordarson, GSL Special Publication 229, 275-306, 2004).

Figure 1 is not a geological map.

Lines 110-111: Would a 40 Myr difference in lithosphere age between the OJP and Nauru Basin make any difference to the temperature, given that both are >120 Myr old?

Lines 123-127: Has all this been deduced from xenoliths? Add references (given together on line 120) for the various conclusions.

Line 134: delete “that”.

Line 143: “comprising”, not “comprised of”.

Line 145: OJP emplacement was not entirely submarine (see above).

Lines 149-152: High mantle potential temperature is required by the geochemical data. Melting to 20-30% is a conclusion based on the composition of primitive OJP basalt and is not an assumption. Primitive OJP basalt has extremely low concentrations of moderately incompatible elements that require either very high melt fractions or an extraordinarily depleted source. The mixed peridotite/eclogite source proposed here would have higher concentrations of incompatible elements than would a peridotite source and would require even higher melt fractions to account for the composition of primitive OJP basalt.

Line 152: …is not robust… (delete “a”). This statement needs to be justified. The value of 20-30% melting is based on phase equilibria (Herzberg, 2004) and incompatible element concentrations (Fitton & Godard, 2004) and is robust.

Lines 155-160: Restriction of sampling to the upper 3-4 km is a fair point, but the sampling is from several sites covering a large part of the surface area (an area the size of Alaska or Western Europe). Similar coverage of any continental LIP would give a true picture of variability. The assertion that the apparent homogeneity is due to mixing in large magma chambers is not valid because the sampled basalts range in composition from primitive to evolved. The surface layers of the OJP would have been emplaced during the waning stages of LIP formation when mantle temperatures would have been lower. Deeper sampling is therefore likely to provide evidence for even higher mantle temperatures. Even with the data we have, we can be confident in the estimated temperature.

Line 163: It has been argued that the Louisville seamount trail is the OJP plume-tail trace (e.g. Mahoney, J.J. and Spencer, K.J. (1991). Isotopic evidence for the origin of the Manihiki and Ontong Java oceanic plateaus, Earth and Planetary Science Letters 104, 196–210).

Lines 189-192: This argument implies that the melting responsible for the formation of the OJP was essentially a static process. The melt zone will have been fed continuously during OJP emplacement, and depleted, residual mantle would have moved sideways. The dry residue forming the thickened lithosphere may represent the last residue formed during the waning phase of activity, thus removing the volume problem.

Godfrey Fitton
16 December 2020

For our responses to the reviewers, their comments are shown in italics and our replies are in bold. Line numbers in responses correspond to those in the revised manuscripts.

We also made some minor modifications based on comments by reviewer #2 in his attachment and the English editing by professional company (Editage by CACTUS).

REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

The authors have addressed the majority of issues raised in my original review. Attached please find an edited/commented version of the revised submission for consideration. As in my review of the original version of the manuscript, the revised manuscript is well-written, logical, and lucid, although a full-length paper describing the data, methodology, and results in much greater detail should follow this manuscript.

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Following this comment, we added several statements in order to avoid the phrase "beyond the scope of this manuscript" and provide possible solution for apparently contrary evidence in L150-186.

In thermochemical plume model, we consider that the 'hybrid pyroxenite model' presented by a newly cited reference (Sobolev et al., 2007) is suitable for explaining the observed OJP lava compositions.

This model predicts that compatible elements such as Ni and PGE are enriched in the coexisting melt, whereas incompatible elements tend to reside in the residual pyroxenites.

Comments by reviewer #2 in attached file:

Figure 1: A white star would be much more visible. And the figure needs a key/color bar for the colors used to indicate various depths and elevations.

We changed the figure as you suggested.

L71 moves over: The original wording suggest that the lithosphere moves independently of the asthenosphere?

We changed the word as you suggested. See in L72.

Figure 4: Capitalize 'Basin'. Insert space between '4.6' and 'km/s'. Insert 'Vs=' before '4.5' and '<4.5'.

We changed the figure as you suggested.

L160-161: A reference is needed here.

We added Kerr et al (2007) as a reference in L184-186.

L171: See Ingle & Coffin (referenced) and Jones et al, EPSL, 2002 (not referenced).

We added words "uppermost 300 km" as you mentioned. See in L198.

L172: The entire crust would have been vaporized

We added a word "crust" as you mentioned. See in L198.

L173-175 : This doesn't strike me as a strong argument; see below.

L175: (may not have been able to survive) If these Jurassic rocks were >100 km from the center of the impact, they could have very well survived. See Fig. 4 of Ingle & Coffin.

Fig. 4 of Ingle & Coffin shows that the pre-existing lithosphere was destroyed within ~300 km from the center of an impact and disturbed ~1000 km away by a magma migration. Following this, we corrected the sentences as L199-202.

Reviewer #3 (Remarks to the Author):

Overall, the authors have done a good job responding to the reviews.

The depth resolution of radial anisotropy still needs to be better presented and supported.

In Fig. S6 I agree that higher modes push the radial anisotropy a bit deeper, with a better fit to the input than that recovered by the fundamental mode. However, that is not the classic problem with radial anisotropy depth resolution. The problem is that the difference in SH vs. SV sensitivity pushes anisotropy artificially deep. If the real (input) structure has shallow radial anisotropy (say 0 - 100 km) , it is typically recovered much deeper (> 150 km). Using an input model with deep anisotropy (~200 km), as done in the current revision, is not very useful to investigate this problem. What does the recovery test look like if radial anisotropy is shallow (< 100 km)? To be convincing, the recovery test should be shown for an input model with strong, shallow radial anisotropy (< 100 km), also testing a variety of starting models, for fundamental and higher modes.

We added synthetic tests using with an input model with shallow anisotropy (< 100 km) as you suggested and using with an isotropic model as an initial model. See Supplementary Figure 6. Using with isotropic and anisotropic initial model, recovered models are similar, which suggested that initial model dependence may not be seriously affected. In case of a shallow anisotropic model, only about 1% anisotropy is smeared at depths about 200 km.

Also, the manuscript would be improved if more of the responses were reflected as changes to the text, rather than a response to review. As an example, the reasons for not matching petrology - lack of shallow resolution could be added to manuscript to aid the reader. There are several other examples.

We added sentences about limited resolution in shallower part in L268-270. We also added about the influence of fast slab anomaly to the OJP structure in L276-279 and in Supplementary Figure 10, which is mentioned only in the response letters.

Reviewer #4 (Remarks to the Author):

The paper presents robust evidence for the existence of thicker lithosphere beneath the Ontong Java Plateau (OJP); an important conclusion in light of the geodynamic significance of the OJP. I have, however, some reservations about the petrological aspects of the paper, and have noted a few minor editorial corrections.

Lines 35-36: Emplacement of the OJP was not entirely submarine. Drilling at ODP Site 1184 *penetrated a thick sequence of pyroclastic rocks that formed through subaerial phreatomagmatic eruptions (Thordarson, GSL Special Publication 229, 275-306, 2004).*

We added a sentence in L37 about subaerial eruptions and the reference #7.

Figure 1 is not a geological map.

We changed the title to Bathymetric map.

Lines 110-111: Would a 40 Myr difference in lithosphere age between the OJP and Nauru Basin make any difference to the temperature, given that both are >120 Myr old?

We modified the sentence in L111-115 to clarify the seafloor ages.

Lines 123-127: Has all this been deduced from xenoliths? Add references (given together on line 120) for the various conclusions.

Yes, all are deduced from xenoliths. All conclusions in L125-129 are from reference #9. So, we added reference #9 for the sentence.

Line 134: delete "that".

We deleted it. See L135.

Line 143: "comprising", not "comprised of".

We changed it. See L143.

Line 145: OJP emplacement was not entirely submarine (see above).

We modified the sentences in L37 and L145.

Lines 149-152: High mantle potential temperature is required by the geochemical data. Melting to 20-30% is a conclusion based on the composition of primitive OJP basalt and is not an assumption. Primitive OJP basalt has extremely low concentrations of moderately incompatible elements that require either very high melt fractions or an extraordinarily depleted source. The mixed peridotite/eclogite source proposed here would have higher concentrations of incompatible elements than would a peridotite source and would require even higher melt fractions to account for the composition of primitive OJP basalt.

Following this comment, we modified the sentence in line 150-156. Despite this, we cannot agree most of these arguments because trace element compositions of mixed peridotite/eclogite source remain unknown. If recycled eclogites and hybrid pyroxenites are relatively depleted in trace elements (as much as OJP lavas), high-degree of melting of hybrid source (up to 100%) can create incompatible-element depleted melt. Instead of noting the detail of this arguments, we cited several articles (Korenaga 2005; Sobolev et

al., 2007) discussing the effect of melting peridotite/eclogite source and Ni problem for the OJP lavas in Line 158-163.

Line 152: …is not robust… (delete "a"). This statement needs to be justified. The value of 20-30% melting is based on phase equilibria (Herzberg, 2004) and incompatible element concentrations (Fitton & Godard, 2004) and is robust.

We agree that a value of 20-30% melting is robust if the assumption of anhydrous peridotite melting is correct. In order to avoid the confusion, we modified the sentence in line151-153

Lines 155-160: Restriction of sampling to the upper 3-4 km is a fair point, but the sampling is from several sites covering a large part of the surface area (an area the size of Alaska or Western Europe). Similar coverage of any continental LIP would give a true picture of variability. The assertion that the apparent homogeneity is due to mixing in large magma chambers is not valid because the sampled basalts range in composition from primitive to evolved. The surface layers of the OJP would have been emplaced during the waning stages of LIP formation when mantle temperatures would have been lower. Deeper sampling is therefore likely to provide evidence for even higher mantle temperatures. Even with the data we have, we can be confident in the estimated temperature.

We agree that apparent homogeneity is not only due to mixing in magma chamber, but it seems worth noting such process would help to erase the preexisting heterogeneity. Thus, we added "presumably" to the sentence in L178. Moreover, we added a statement that the compositionally and isotopically enriched lavas were actually recognized as the waning stages of heterogeneous plume in L181-183. We also modified the sentences about the future sampling. See L183-186.

Line 163: It has been argued that the Louisville seamount trail is the OJP plume-tail trace (e.g. Mahoney, J.J. and Spencer, K.J. (1991). Isotopic evidence for the origin of the Manihiki and Ontong Java oceanic plateaus, Earth and Planetary Science Letters 104, 196–210).

We added the sentence about Louisville seamount trail in L190-191.

Lines 189-192: This argument implies that the melting responsible for the formation of the OJP *was essentially a static process. The melt zone will have been fed continuously during OJP emplacement, and depleted, residual mantle would have moved sideways. The dry residue forming the thickened lithosphere may represent the last residue formed during the waning phase of activity, thus removing the volume problem.*

We agree it is also possible. We added sentences in L222-227. However, it may be difficult to explain minimal subsidence, so we prefer a delamination hypothesis.

25th Mar 21

Dear Dr Isse,

Your manuscript titled "Seismic evidence for a thermochemical mantle plume underplating the lithosphere of the Ontong Java Plateau" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers, particularly those of Reviewer #3 regarding further explanation of the differences in depth resolution relative to other studies. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Joe Aslin

Associate Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

The manuscript is greatly improved from the original submission, and I recommend publication with minor revisions. From my perspective, the authors have thoroughly addressed reviewers' concerns in the revised manuscript. Although the authors and the petrologist/geochemist reviewer disagree on certain matters, the revised manuscript takes these disagreements into account. I attach an edited/commented version of the manuscript, and encourage the authors to consider these suggestions as well as the additional references (Elkins-Tanton et al, EPSL, 2004; Jones et al, GSA Special Paper 388, 2005) in revising/finalizing the manuscript for publication.

Reviewer #3 (Remarks to the Author):

The authors now sufficiently demonstrate the resolution of radial anisotropy in figure S6. I admit that I am surprised by it, because it is different than that of Rychert & Harmon 2017 (fig. 3) even comparing only the 0th order. The only thing that is obviously different to me is that the model of R&H had the strongest anisotropy at 30 km rather than 50 km depth. This could potentially be the reason, unless the authors did something special related to damping, etc.? If that is the case it would be nice if the authors mentioned this in the text, explaining. If instead it is the first reason, I suppose

it says something very specific about depth resolution that would also be nice to mention, but the authors may be off the hook in this case because of the thicker OJP crust. Otherwise, it is a nice manuscript and I am happy for the paper to be published.
Kate Rychert

Rychert, C.A., Harmon, N., 2017. Constraints on the anisotropic contributions to velocity discontinuities at ~60 km depth beneath the Pacific. Geochem. Geophys. Geosyst. 18, doi:10.1002/2017gc006850.

Reviewer #4 (Remarks to the Author):

I've now read the revised manuscript and the authors' responses to reviewer criticism. The authors have made a conscientious attempt to rebut the criticism and the revised paper is a lot better for it. While I could continue to debate the nature of the OJP mantle source and the source of the pyroxenite that the authors suggest is a major constituent, I don't think this would get us anywhere. We simply don't know the answers and the revised paper acknowledges this. I'm happy to confirm that the points raised by the reviewers have been satisfactorily addressed.

Article

Seismic evidence for a thermochemical mantle plume underplating the lithosphere of the Ontong Java Plateau

Takehi Isse[1]*, Daisuke Suetsugu[2], Akira Ishikawa[3], Hajime Shiobara[1], Hiroko Sugioka[4], Aki Ito[2], Yuki Kawano[1], Kazunori Yoshizawa[5], Yasushi Ishihara[2], Satoru Tanaka[2], Masayuki Obayashi[2], Takashi Tonegawa[2], Junko Yoshimitsu[2]

[1] Earthquake Research Institute, ~~the~~ University of Tokyo, Tokyo, 113-0032, Japan
[3] School of Science, Tokyo Institute of Technology, Tokyo, 152-8550, Japan
[4] Graduate School of Science, Kobe University, Kobe, 657-8501, Japan
[5] Faculty of Science, Hokkaido University, Sapporo, 060-0810, Japan
*Corresponding author tisse@eri.u-tokyo.ac.jp

Abstract

The Ontong Java Plateau (OJP) in the western Pacific Ocean is the world's largest oceanic plateau~~s in the western Pacific Ocean~~, formed by a massive volcanic event 122 million years ago that significantly affected ~~the~~ Earth's environment. The cause of the magmatic event remains controversial because the ~~geological~~ upper mantle structure beneath the OJP is poorly understood due to limited geophysical and geochemical data. Using newly collected passive seismic data obtained through

seafloor observations alongside existing seismic data, we determined the three-dimensional radially anisotropic shear wave velocity ~~at~~ to depths as great as 300 km. The seismic structure shows that the lithosphere–asthenosphere boundary (LAB) is approximately 40 km deeper beneath the centre of the OJP than beneath the surrounding seafloor. Based on our results and petrological/rheological constraints, we propose that the LAB has deepened as a result of dehydrated residual material from a thermochemical mantle plume underplating pre-existing lithosphere during ~~the~~ formation of the OJP.

Introduction

The Ontong Java Plateau (OJP) in the western Pacific Ocean is the largest oceanic Large Igneous Province (LIP) on Earth[1], with crust 30–40 km thick[2,3,4]. While the entire OJP is thought to have been emplaced at approximately 122 Ma by a single massive magmatic event, influencing the global climate, as well as causing oceanic anoxic events and the extinction of marine species[1], the cause of the magmatism is still debated. The geochemistry of OJP basalts is extraordinarily homogeneous[5], which differs from those of other LIPs and hotspots. The main OJP emplacement was submarine at depths greater than 800 m below sea level[6], but partly subaerial[7], and post-emplacement subsidence has been minimal compared to normal seafloor[8].

Three major hypotheses have been proposed for the origin of the OJP to explain these characteristics: 1) the ascent of a hot mantle plume[1,9,10] ~~whose~~ with a potential temperature (T_p) ~~was~~ higher than 1500 °C; 2) passive mantle upwelling near a spreading ridge[11]; and 3) a meteorite impact resulting in extensive melting[12]; however, none of these hypothesis satisfy existing data or are conclusive. Identifying the origin of the OJP is not only essential to understand Cretaceous Earth geodynamics, but also to evaluate the magnitude and mechanism of a change in the climate that may be induced by a future massive ~~volcanic~~ magmatic event. The seismic structure beneath the OJP may be the key to resolving its origin. For example, a recent study[13] found ~~a~~ strong radial anisotropy just beneath the Moho at the Hikurangi Plateau, which may have been a part of the single Ontong -Java-Manihiki-Hikurangi Plateau[14], with interpretations of its formation via a plume. Current knowledge on the structure beneath the OJP is sparse, particularly for the upper mantle, and seismic studies to date have yielded no direct evidence of a plume. Until now only two studies have focused on the upper mantle beneath the OJP, and the resulting models differ markedly. One study suggests the presence of a low-velocity, rheologically strong mantle root extending as deep as 300 km beneath the entire OJP[15]. Shear wave splitting analysis around the OJP also supports this mantle root hypothesis[16]. More recently, a very high-velocity and compositionally different mantle has been proposed, extending to a depth of 100 km beneath the corner of the L-shaped OJP[17], with no noticeable low-velocity anomalies. These opposing results may arise from the data used, which ~~originate~~ are solely from land stations well ~~even though the~~beyond the

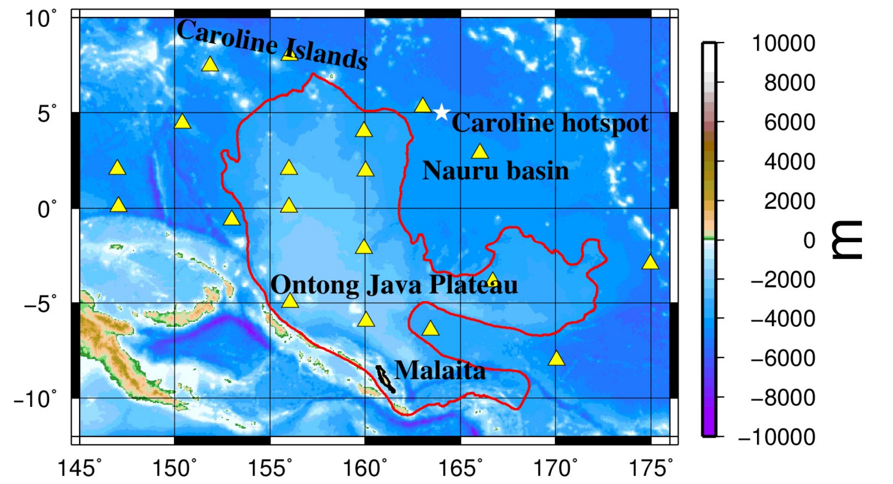

Figure 1. **Bathymetric map of the study region with the locations of the seismic stations indicated.** The maps show the OJP[6] (red line), ~~the~~ OJP array stations used in this study (yellow triangles), ~~the~~ Caroline hotspot[57] (white star), and ~~the~~ Malaita Island (black line).

boundaries of the submarine -OJP ~~is a submarine plateau~~. To ~~observe~~ investigate the OJP's upper mantle more reliably, in situ seismic seafloor observations are required.

From late 2014 to early 2017, we conducted the first broadband seismic and electromagnetic ~~observation campaign~~investigation of the OJP ~~on the seafloor and islands of the OJP~~ using the 'OJP array'[18] on OJP and adjacent seafloor and neighboring islands (Figure 1). Here, we determine the three-dimensional radially anisotropic shear wave structure of the upper mantle in and around the OJP via surface wave tomography using seismograms recorded by the OJP array and data from existing land and seafloor stations (Supplementary Figure 1).

Results

Seismic structure

The shear velocity model obtained in this study shows that a high-velocity lid (4.45–4.6 km s^{-1}) at a depth of ~100 km in most regions overlies low-velocity regions at ~150 km depth. This feature is seismic evidence of plate tectonics, i.e., the cold, rigid oceanic lithosphere overlies the hot, ~~soft~~ weak, viscous, and ductile asthenosphere. However, ~~in~~ under the central ~~part of the~~ OJP, the high shear velocity region (4.45–4.6 km s^{-1}, 2% high-velocity anomalies at a depth of 150 km) extends to 150 km, ~~which is~~ a remarkable

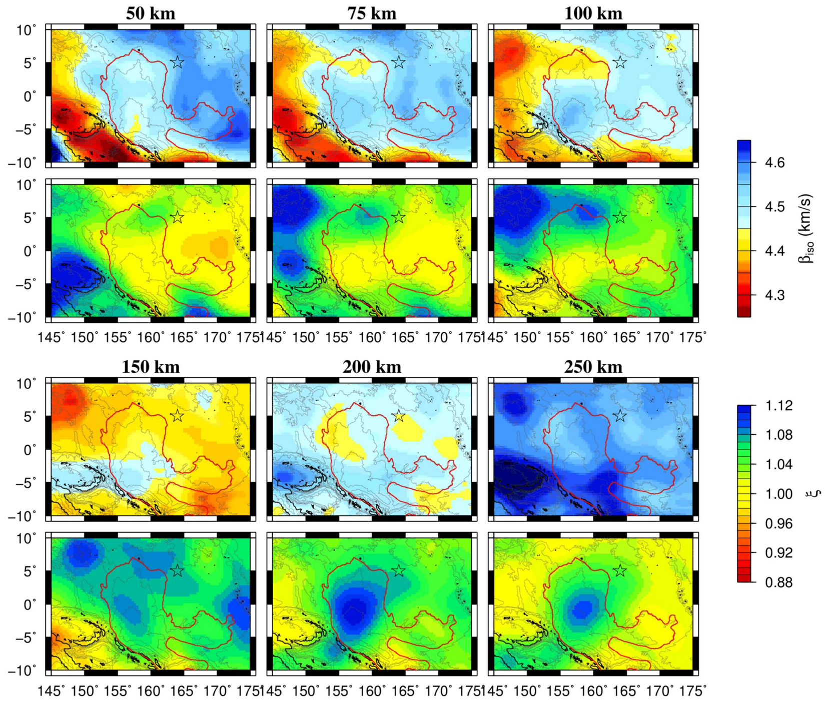

Figure 2. **Shear-wave tomography model.** The upper panels show the Voigt-averaged shear wave velocity (β_{iso}) structure and the lower panels show the radial anisotropy ($\xi = (\beta_H/\beta_V)^2$) at depths of 50, 75, 100, 150, 200, and 250 km. Open star shows the Caroline hotspot.

feature of the upper mantle~~ beneath the OJP~~. Radial anisotropy is weak ($\xi \equiv (\beta_H/\beta_V)^2 \approx 1.0$) and strong ($\xi > 1.1$) in the lithosphere and asthenosphere, respectively (Figures 2 and 3), which is similar to normal Pacific Ocean upper mantle[19,20], but the depth of the lithosphere–asthenosphere boundary (LAB) is significantly greater beneath the OJP. The LAB depth, defined here as the depth of the negative peak in the vertical velocity gradient (see Methods, Figure 3a), is ~130 km, which is ~~significantly~~ much deeper than the average

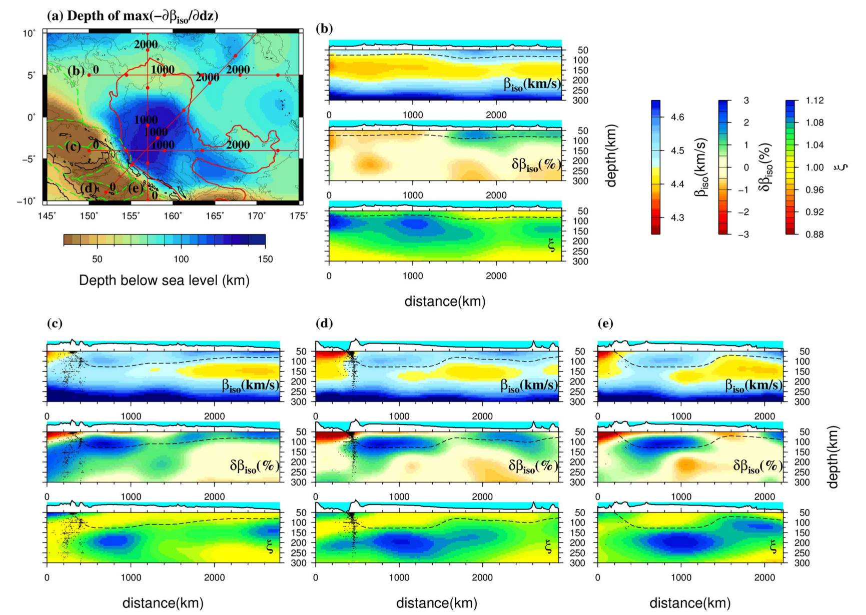

Figure 3 **Estimated depth of the LAB and vertical profiles of the OJP. a)** Map of LAB depths beneath the OJP and the adjacent region estimated using the negative peak depth of the vertical gradient of β_{iso}. The green dashed line shows the plate boundary. **b–e**) Vertical profiles of the shear ~~-~~wave velocity~~,~~ anomalies with respect to the average of the studied region, and radial anisotropy along the profiles indicated in **a**. Black dashed lines show the depth of the LAB. Black dots in **b–e** show earthquakes that ~~have~~ occurred during deployment of the OJP array adjacent to the cross-sections.

LAB depth (80–90 km) beneath Pacific Ocean seafloor of a similar age in the study region as estimated from surface wave analysis[20]. The LAB depth beneath the OJP is deeper than the deepest LAB previously reported in the Pacific Ocean, ~~e.g.~~ i.e., ~~that of~~ 120 km in the north-west~~ern~~ Pacific based on SS precursor analysis[21].

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The Nauru Basin, east of the OJP, has a LAB depth of 90 km, similar to the average depth of the LAB in the Pacific Ocean. North of the OJP is an east–west-trending low-velocity region (4.35–4.45 km s^{-1}, 0.5–1% low-velocity anomalies, Figure 3b) with depths of at least 250 km along the Caroline Islands. Radial anisotropy is strong ($\xi > 1.1$) at depths shallower than 150 km. The LAB depth is 70 km, which is consistent with the findings of a previous study that analysed SS precursors[22]. These features may be related to the Caroline ~~hotspot~~plume, which is thought to be present below the easternmost Caroline Islands. The depth of the LAB gradually thins to the east, while thinning to the north and southeast of the OJP is abrupt (Figure 3a, c, and e). As the crustal thickness of the central OJP is 30–40 km and that of the Nauru Basin is 10 km[23,24], the lithospheric mantle is ≥~~~~~20 km thicker beneath the OJP than beneath the Nauru Basin. However, owing to limited depth resolution and the definition of the LAB depth adopted in this study, the LAB depth beneath the OJP may be underestimated by 20 km or more (see Methods, Supplementary Note 1, and Supplementary Figure 2). The observation of thick lithosphere is possible with multi-mode surface wave tomography, as the first higher-mode Rayleigh wave traveling through the OJP exhibits high phase velocities, which is sensitive to shear wave velocities at a depth of ~100 km and insensitive to those at crustal

Commented [A2]: A hotspot is the surface volcanic expression, so doesn't exist at depth.

depths (see Supplementary Note 2 and Supplementary Figures 3 and 4). Thick lithosphere beneath the OJP is qualitatively consistent with weak seismic attenuation in the OJP mantle[25,26].

Discussion

Thermal versus compositional factors

What is the origin – thermal and/or compositional – of the thick lithosphere beneath the OJP? If thermal effects are the cause, the thick lithosphere, with high-velocity anomalies of ~1–2%, should be accompanied by lower temperature anomalies of approximately 100–200 K[20] compared to lithosphere beneath the Nauru Basin. One possibility is a difference in lithospheric ages between the OJP and Nauru Basin; according to a half-space cooling model[27], the age of the OJP lithosphere should be ~40 Ma older than that of the Nauru Basin (~130–160 Ma[23]) to account for the temperature difference. However, interpreted seafloor ages underlying the southernmost OJP are ~160 Ma[28] and of the OJP's uppermost crust~122 Ma[12], which are unlikely to have caused such a large temperature difference. Other cooling models[29] provide even smaller temperature differences. Another possibility is the existence of small-scale down-welling flow[30] in the asthenosphere beneath the OJP. However, it appears unrealistic that a localised down-welling flow has moved with the OJP and maintained a cool lithosphere. Strong radial anisotropy beneath the OJP lithosphere at depths >150 km indicates the existence of horizontal flow rather than vertical flow. Therefore, we propose that the cause of the thick lithosphere is compositional, which agrees with studies of xenoliths sampled from the Malaita Island where OJP crust has been obducted[9,28,31,37,38,43] (Figure 1). This~~ese~~ xenolith population comprises diverse suites of peridotites and garnet pyroxenites that represent virtually the entire section beneath the southern margin of OJP lithosphere, extending to depths of ~~as much as~~ ≤120 km. The upper part (<80 km) of OJP lithosphere is mainly composed of fertile lherzolites originating from pre-existing oceanic lithosphere with subordinate pyroxenitic dykes or sills, whereas the deeper part of the OJP lithosphere, at depths between 90 and 120 km, can be interpreted as melt residues of a mixed peridotite-pyroxenite source from a thermochemical mantle plume[9]. Based on petrological calculations, shear wave velocities of the peridotite beneath the OJP at depths of 60–120 km are estimated to range between 4.6 and 4.5 km s^{-1}, respectively[31], which is consistent with our seismological results of this study (Supplementary Figure 5). The petrological model does not involve anomalous velocity structures, as proposed by previous studies, such as a broad low-velocity anomaly (4.0–4.1 km s^{-1}) at depths from 80–150 km[15] or very high anomalies (4.75 km s^{-1}) extending to a depth of 100 km[17]. We consider the seismic structure described here to be more reliable than any structure previously published because our imaging is the first to be based on in situ ~~observation~~ data ~~of~~ from the OJP. Therefore, we conclude that the seismically determined velocity model presented here is consistent with the petrologically determined velocity model, such that the thickened lithosphere was most likely caused by underplating of ~~the~~ melt residues of a thermochemical mantle plume beneath pre-existing oceanic lithosphere at the time of OJP emplacement (Figure 4).

The arrival of a hot and buoyant mantle plume comprising anhydrous peridotite at the base of the lithosphere should result in ~~a~~ considerable ~~amount of~~ subaerial volcanism[11], which is inconsistent with the observation that the main OJP emplacement was submarine[6]. However, a thermochemical plume[32], assuming the incorporation of compositionally dense but fusible material such as eclogites or garnet pyroxenites, would

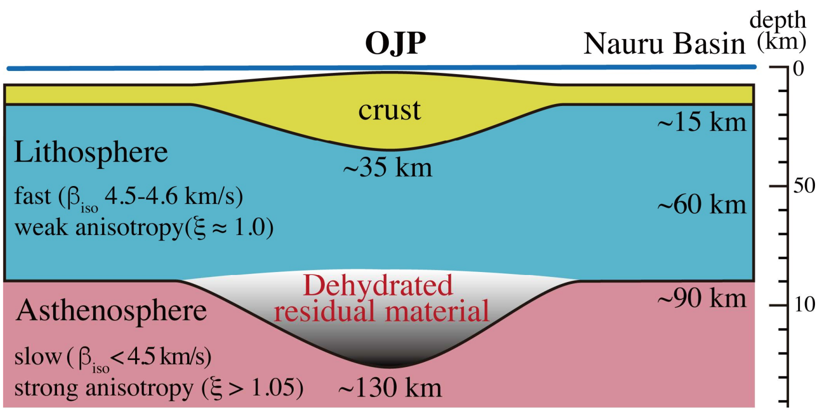

Figure 4 **Schematic illustration of ~~the~~ OJP upper mantle.** Thick lithosphere (~130 km) exists beneath the centre of the OJP. Dry melt residues underplate ~~the~~ pre-existing lithosphere beneath the OJP.

decrease both the solidus temperature and net buoyancy flux of the plume, such that a large outpouring of basaltic magma could occur with minor topographic uplift of the OJP.

We speculate that the high potential mantle temperature (T_p >-1,500 °C), derived from the geochemistry of OJP basalts[33,34] may be overestimated, as ~~the assumption of~~modelled 20–30% melting of homogeneous anhydrous peridotite is not a unique solution for ~~modelling~~ the observed lava compositions. Evidence to date shows that OJP basalts are extraordinarily homogeneous[5], depleted in incompatible trace elements including H_2O and CO_2[5, 32], and slightly enriched in platinum-group elements (PGEs)[35] compared with other LIPs and hotspots thought to be generated by melting of heterogeneous plume sources. However, such observations in favour of the OJP magma source being homogeneous anhydrous peridotite remain controversial and inconclusive. Indeed, previous studies[11,36] have pointed out that the high-Ni compositions of olivine from primitive OJP lavas are not compatible with the melting products of common peridotites. This led to the proposal of olivine-free lithology as a more appropriate source for the OJP lavas such as pure eclogite[11] or hybrid pyroxenite formed from reaction between eclogite-derived Si-rich melts and peridotite[36].

We ~~consider that~~favour the latter ~~is favourable~~ because a series of hybrid pyroxenites is the main constituent of deeply -derived xenoliths found in Malaita[9,37,38]. Moreover, it would be possible to reconcile the observed chemical and isotopic variations of OJP lavas if the hybridisation process during plume ascent created almost ~~-~~uniform pyroxenite as the main magma source[36]; the melting of hybrid pyroxenite could enhance the release of compatible elements such as Ni and PGEs into the melt, resulting in elevated concentrations of Ni and PGEs in OJP lavas; and the olivine-free but pyroxene-rich residues left behind after the melting of hybrid pyroxenite could accommodate substantial amounts of incompatible trace elements that may account for incompatible ~~-~~element depletion in the primitive OJP magma.

~~In its current state,~~Currently, geochemical modelling of trace element abundances and isotopic compositions of the OJP lavas is challenging because the compositional variability expected for recycled eclogites and resulting hybrid pyroxenites cannot be tightly constrained. This is partly because almost all OJP geochemistry is based on limited data obtained from the uppermost 3–4 km section of erupted lavas, presumably after effective mixing in large magma chambers in an already thickened crust. Nevertheless, a compositionally and isotopically enriched type of basalt was recognised as an overlying veneer of the main group of basalts at several localities, which seems consistent with the more fusible components in the heterogenous plume that was tapped during the waning phases of the OJP magmatism[33,39]. Future sampling of significantly deeper crustal sections[40] could clarify the extents of the heterogeneities in ~~the~~ OJP lavas, which is required for viable trace element modelling and T_p estimates for the thermochemical plume[32].

Other hypotheses, such as the passive upwelling of dense material[11] and a meteorite impact[12], which do not invoke deep mantle plume activity for the formation of the OJP, are consistent with the lack of convincing evidence of a post-OJP hotspot track (plume ~~-~~tail). The Louisville seamount trail is a potential plume trail[41] but remains ~~under discussion~~controversial. However, the thickened lithosphere observed in this study is difficult to reconcile with these non-plume scenarios. In the passive upwelling hypothesis, basaltic magma would have been produced by ~~the~~ almost ~~-~~complete melting of recycled crustal material that ascended via asthenospheric upwelling near a spreading ridge. This appears unlikely to have produced melt residues sufficient to thicken the OJP lithosphere as this essentially prevents the melting of anhydrous peridotite under high-pressure conditions. For impact-induced melting, basaltic magma would have been largely supplied first from the uppermost 300 ~150 km of pre-existing crust and mantle beneath the impact site where the most decompression and shock/release wave melting would have occurred (Jones et al, *GSA Special Paper 388*, 2005), although an extended secondary episode of additional melting would occur at greater depths (e.g., Elkins-Tanton et al, *EPSL*, 2004). If the melting region were sufficiently large to thicken the vast area of OJP lithosphere, Jurassic-aged oceanic crust and mantle identified in the Malaita xenolith population should have been disturbed[28]. A lack of extra-terrestrial signatures in coeval Pacific marine sediments would also suggest no impact event at the time of OJP formation[42]. We consider that impact hypothesis is to be unlikely,although absence of evidence does not equate to evidence of absence.

How can melt residues survive for over 120 Ma?

During the formation of OJP crust, the volatile components of the plume, including H_2O were incorporated into the basaltic melt, resulting in dry[43] and rheologically strong[44] residual plume material. The restite -root hypothesis[45], which assumes that a dry, hot, highly viscous restite root attached itself beneath the hotspot lithosphere, yielding a thick high-viscosity layer (lithosphere), was proposed to explain hotspot swell uplift. Recent receiver function analysis suggests that the LAB is as deep as 100 km beneath hotspot islands in the Pacific Ocean[46], which is similar to the deep LAB beneath the OJP. Recent rheological and geophysical studies[47,48] indicate that the asthenosphere is wet and thus rheologically weak compared to the lithosphere, which suggests that the dry and rigid melt residues could have moved with Pacific lithosphere over long time intervals. A similar mechanism has been proposed to explain the extended life of continental cratons (>2 Gyr)[49].

The area of >~~-~~20 ~~-~~km thicker lithosphere beneath the OJP is ~5 × 10^5 km^2, so that the volume of the dry, dense residue that we have identified is ~1–2 × 10^7 km^3, which is significantly less than the mantle source volume estimated to form OJP crust (1–3 × 10^8 km^3)[32]. One possible explanation is that most ~~of the~~ residue moved ~~sideway~~ laterally and did not underplate during ~~the~~ emplacement and the last residue formed thick lithosphere during the waning phase of activity. An alternative explanation is that most ~~of the~~ dense residue has delaminated while the OJP has moved with ~~the~~ Pacific lithosphere after ~~its~~ emplacement. The latter explanation may be more plausible given that this might explain the less-than-~~expected~~ predicted subsidence of ~~the~~ OJP ~~seafloor~~lithosphere.

> Commented [A3]: These numbers are for 25%-50% partial melting, which you've considered suspect above? If you believe lower degrees of partial melting, why not say so here and thus more closely align with your dry, dense residue volumes?

Methods

We employed a surface ~~-~~wave tomography method~~, which~~ consisting of three independent stages[20,50,51]. First, we measured the path-specific multimode phase-velocity dispersion curves of Love and Rayleigh waves from the fundamental to the 4th higher mode using a fully nonlinear waveform inversion method. The waveform-fitting process was fully automated, where the quality of the obtained dispersion curves was evaluated by a reliability parameter and determined by the degree of waveform fitting and relative power of each mode in the selected band~~-~~pass filtered time window. This empirical criterion is identical to that of our previous study[20]. We obtained, at most, 2,000–4,000 phase velocity dispersion curves for the fundamental Rayleigh waves~~,~~ and 500–1,500 for the higher modes in the 5–30 mHz frequency range. For the Love waves, the number of dispersion curves was approximately half the number obtained for the Rayleigh waves. Approximately 5–10% and 20–30% of the Love and Rayleigh wave dispersion curves, respectively, were obtained from broadband ocean bottom seismographs (BBOBSs). For the second stage, we inverted the path-specific multimode phase velocities for two-dimensional phase-velocity mapping at each frequency, incorporating finite-frequency and ray-bending effects. Phase-velocity maps were expanded in a set of B-spline functions with a rectangular grid, with the interval in each map varying from 2° to 6.5° depending on the number of paths and the size of the Fresnel zone for each mode and frequency. Finally, we inverted the multimode phase-velocity maps to generate a three-dimensional radially anisotropic shear wave velocity model, employing iterative least-squares inversion. The use of up to the 4^{th} higher modes of surface waves enables us to improve the depth resolution of the radial anisotropy (Supplementary Figure 6).

Following the protocol introduced in our previous study[20], we used β_V and β_H as independent parameters, showing β_{iso}, which is the Voigt-averaged isotropic shear wave velocity, where $\beta_{iso}^2 = (2/3)\beta_V^2+(1/3)\beta_H^2$ and $\xi = (\beta_H/\beta_V)^2$. In this inversion, two a priori parameters control the degree of perturbation and smoothness in the depth variations—the standard deviation, σ, and correlation length, L— which were respectively set to 0.01 km s^{-1} and 3 km in the crust, 0.04 km s^{-1} and 15 km between the Moho and a depth of 400 km, and 0.03 km s^{-1} and 15 km between ~~a~~ depths of 400 and 670 km. Here, σ decreases linearly from 0.03 km s^{-1} at 670 km to 0.02 km s^{-1} at 1,500 km, while L increases from 15 km at a depth of 670 km to 30 km at a depth of 1,500 km. We confirmed L and σ through a series of synthetic tests, in which L and σ were varied to observe whether the retrieved models were consistent with various input models. Our one-dimensional reference velocity model was modified from PREM by smoothing the 220 -km discontinuity with corrections for the crustal structure using the CRUST1.0 model and a recent result[4] from receiver function analysis beneath the OJP (Supplementary Figure 7). We corrected for the anelastic effect using the attenuation model of PREM with a reference frequency of 1 Hz. The Moho depth of an initial model was fixed during the inversion. The mis-location of the Moho depth affected the inverted structure (Supplementary Note 3 and Supplementary Figure 9); the obtained structure shallower than ~50 km was less resolved because the phase velocities we used were longer than 30 mHz, which is less sensitive in the shallow depths (See Supplementary Figure 4(d)). Checkerboard resolution tests suggested that the lateral resolution of the OJP is ~4° for the isotropic structure and ~7° for the anisotropic structure (Supplementary Figure 8). We applied a spatial two-dimensional FFT low-pass Gaussian filter at 4° and 7° (grdfft in GMT[52]). As the sensitivity of the surface waves decreases with depth, we only show a three-dimensional radially anisotropic shear wave velocity structure for depths shallower than 300 km. We also performed a synthetic test to assess the influence of the subducting slab in the south of the OJP, which is indicated as a fast velocity anomaly. The recovered model suggested that the fast subducting slab anomalies have little effect on the mantle structure beneath the OJP (Supplementary Figure 10)

A popular method for analysing the LAB is to use a reflected or converted seismic wave. In the northern part of the OJP, the depth of LAB is estimated to be ~70 km based on SS-precursor wave analysis[22]. However, SS-precursor waves do not sample the main part of the OJP due to the locations of the seismic stations and earthquakes. While S-wave receiver function analysis is also a common method for estimating the LAB depth[53], a high level of horizontal-component noise in the OJP array data prevents this type of analysis. Alternatively, we propose an approach that uses a negative gradient peak in the shear wave velocity as a proxy for the LAB depth. For this, we calculated the depth of the negative peak in the vertical gradient of the shear wave velocity obtained from the tomographic model to analyse the lateral variation of LAB depth. From a seismological perspective, the upper mantle structure can be described as a high-velocity lid (lithosphere) overlying the low-velocity zone (asthenosphere), such that the negative peak in the vertical gradient can be used as a proxy for the LAB[20,51,54]. Thus, we defined the depth of the proxy as the depth of the LAB. To assess the depth resolution of the surface waves and the reliability of this proxy, we performed synthetic tests using a synthetic shear ~~-~~wave velocity model and by changing the thickness of a 3% high-velocity lid (Supplementary Note 1 and Supplementary Figure 2). The recovered vertical profiles show a high-velocity lid at the top and a low-velocity body below. Although the sharp discontinuity was not recovered because ~~the~~ surface waves are insensitive to this, the peak depth represents the depth of the discontinuity (i.e., LAB) for input LAB depths of 70–110 km within an accuracy of 15 km. The peak depths are greater than the input discontinuity depths for depths of 60–80 km and shallower for depths of 90–160 km, indicating that the deep LAB beneath the OJP is likely to be underestimated by 20 km or more whereas this is not the case for the surrounding regions (e.g., Nauru and Caroline).

DATA

We collected seismic waveform data recorded by the OJP array for earthquakes with moment magnitudes (Mw) greater than 5.5 in the western Pacific Ocean to analyse the upper mantle shear wave structure beneath the OJP. As these data alone ~~are~~ do not suffic~~i~~e~~nt~~ for this purpose, we also collected seismic data recorded on land and on the seafloor in the western Pacific Ocean (Supplementary Figure 1). These data are distributed by the Ocean Hemisphere network Project (OHP) and the Incorporated Research Institutions for Seismology Data Management Center (IRIS DMC), which were collected for earthquakes between 1990 and 2016. The study region in the western Pacific Ocean is similar to that of another recent study[17] in which only seismic data from land stations were used. Here, we used seafloor observation data recorded by BBOBSs as well as land data from a total of 161 stations, of which 115 are BBOBS stations (Supplementary Figure 1). Vertical component seismograms recorded on the seafloor are contaminated by tilt noise caused by ~~leaks of a~~ high ~~degree of~~ noise levels in the horizontal components induced by seafloor currents[55] and compliance noise induced by the infragravity waves that are also recorded by seafloor ~~-~~pressure data[56]. We removed this noise from the BBOBS station data in the OJP array by applying a spectral transfer function between the vertical and horizontal components and between the vertical and pressure components, respectively.

Data Availability

The tomography model used in this study is available from the corresponding author upon reasonable request.

Code Availability

The analytical code for the tomography model is available from Dr. Kazunori Yoshizawa upon reasonable request.

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Author Contributions

TI performed the seafloor observation, analysed the data and wrote the manuscript. DS organised the OJP Array seafloor observation and wrote the manuscript. AIS interpreted and wrote the section on the petrological results. HSH, HSU, and AIT performed the seafloor observations. YK developed the code for noise reduction of the seismological data. KY developed the code for surface wave analysis. YI, ST, MO, TT, and JY organised and performed the island observations. All authors read and approved the final manuscript.

Competing interests

The authors declare that they have no competing interests.

For our responses to the reviewers, their comments are shown in italics and our replies are in bold. Line numbers in responses correspond to those in the revised manuscripts.

We also made some minor modifications based on comments by reviewer #2 in his attachment.

We revised abstract based on editorial request.

Reviewer #2

The manuscript is greatly improved from the original submission, and I recommend publication with minor revisions. From my perspective, the authors have thoroughly addressed reviewers' concerns in the revised manuscript. Although the authors and the petrologist/geochemist reviewer disagree on certain matters, the revised manuscript takes these disagreements into account. I attach an edited/commented version of the manuscript, and encourage the authors to consider these suggestions as well as the additional references (Elkins-Tanton et al, EPSL, 2004; Jones et al, GSA Special Paper 388, 2005) in revising/finalizing the manuscript for publication.

We changed the manuscript based on your attached commented manuscript.

We added these references as you suggested. See line 198-199. We also modified sentence in line 200-202 to clarify the sentence.

Figure 3

Are the earthquakes shown above only those that occurred during the OJP array deployment, or do they also include 1990-2016 earthquakes?

The location of earthquakes is from ISC Bulletin for the period 1964-2020 whose magnitudes are greater than 4.0. We added these information and reference in the caption.

L92

A hotspot is the surface volcanic expression, so doesn't exist at depth.

We changed it to plume as you suggested.

L220-222

These numbers are for 25%-50% partial melting, which you've considered suspect above? If you believe lower degrees of partial melting, why not say so here and thus more closely align with your dry, dense residue volumes?

We believe lower degrees of partial melting for peridotitic part of mantle, but here we have to consider bulk mantle (peridotite + eclogite) for estimating mantle source volume relative to the crustal volume. A plausible range could be between 25% (melting of peridotite and minor contribution of eclogite) and 50% (partial melting of eclogite and minor contribution of peridotite). Details are in Kerr and Mahoney (2007) which we cited in the main text.

Reviewer #3 (Remarks to the Author):

The authors now sufficiently demonstrate the resolution of radial anisotropy in figure S6. I admit that I am surprised by it, because it is different than that of Rychert & Harmon 2017 (fig. 3) even comparing only the 0th order. The only thing that is obviously different to me is that the model of R&H had the strongest anisotropy at 30 km rather than 50 km depth. This could potentially be the reason, unless the authors did something special related to damping, etc.? If that is the case it would be nice if the authors mentioned this in the text, explaining. If instead it is the first reason, I suppose it says something very specific about depth resolution that would also be nice to mention, but the authors may be off the hook in this case because of the thicker OJP crust. Otherwise, it is a nice manuscript and I am happy for the paper to be published.
Kate Rychert

Rychert, C.A., Harmon, N., 2017. Constraints on the anisotropic contributions to velocity discontinuities at ~60 km depth beneath the Pacific. Geochem. Geophys. Geosyst. 18, doi:10.1002/2017gc006850.

As the reviewer wrote, there is a discrepancy in results of synthetic experiments by Rychert & Halmon (2017) (RH2017) and the present study. The experiments (only using fundamental modes) for an input anisotropic model with single peak of ξ at a depth ~30 km (RH2017) and 50 km (present study) provide double peak of radial anisotropy at depths of 30 km and 150 km (Fig. 3 of RH2017) and single peak at a depth of 40 km (Supplementary Figure 6 of the present study), respectively, in their recovered models. We employ a common regularization method as described in the Method section, which should not be a cause of the discrepancy.

We suspect that the discrepancy could be caused by strongly non-linear nature of anisotropy sensitivity (model-dependency), as suggested in RH2017 (p. 2863), different Vs model used for the numerical experiments, and different period ranges used by the two studies (10s–200 s in RH2017; 30 s–200 s in the present study): Input model of Figure 3 of RH2017 is a model for a region near a spreading ridge with strong low velocity zone (LVZ)(~4.0km/s) and our synthetic model based on the OJP structure (120–160Ma) has not such strong LVZ (~4.4km/s). A sensitivity kernel for radial anisotropy at a period around 30 seconds is strongly model-dependent and has a deeper sensitivity (~150km) in case for the strong LVZ than that for the weak LVZ (~50 km).

To examine the effect of the LVZ, we performed a synthetic test at periods from 30 s-200 s with a strong LVZ model like RH2017, where the anisotropy has single peak at a depth of 30 km (Figure A(a, b)). A sensitivity kernel of ξ for fundamental mode at a period of 30 s is high up to ~ 100km depth, which is substantially deeper than that of the OJP model (Figure A(c)), though it is still shallow as compared to RH2017. The recovered models of the synthetic test is shown in lower panels of Figure A. We used an initial model (blue) with no anisotropy. A peak of ξ in the recovered model is deeper than the input model when we used only fundamental mode data (Figure A(f), green line), whereas it agrees well with the peak in the input model using up to 4th mode phase velocity data (Figure A(f), red line). We do not have double peak of ξ (Figure 3 of RH2017). The artificial deeper peak in RH2017 may be caused by short period data (10-30s) used in RH2017, which have the peak sensitivity of ξ at depths around 100 km-150 km. In the present study, we did not use phase velocities at 10-30 seconds, which may also the cause of a good recovery in our synthetic test (Supplementary Figure 6), in addition to the use of the higher model data. Since the full discussion above is too lengthy, complicated, and out of main scope (thermochemical plume) of the present study, we just briefly mentioned it in Supplementary Figure 6 caption.

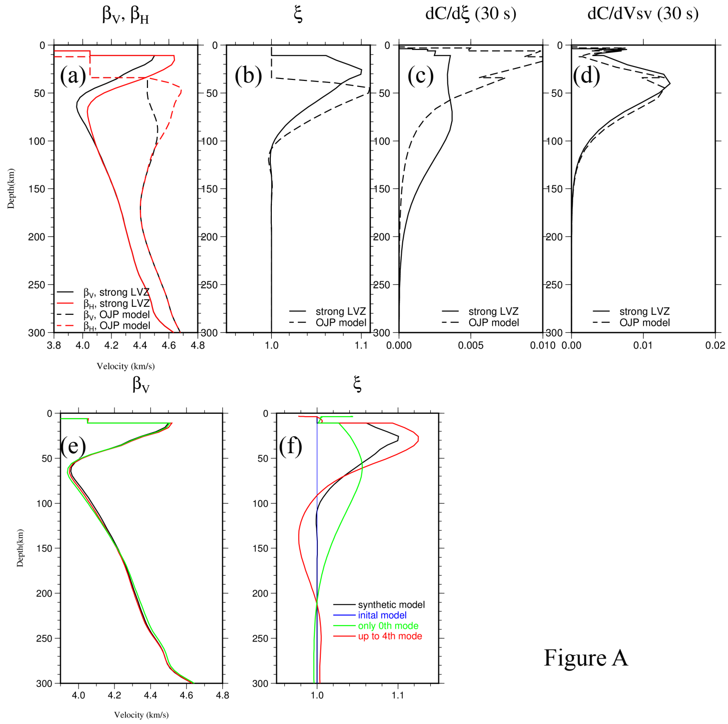

Figure A

Figure A. Sensitivity kernel and synthetic result of strong low velocity zone (LVZ) model. (a),(b) Input models; (c),(d) sensitivity kernels at a period of 30 s; (e),(f) Recovered models. Solid line indicates strong LVZ model, and broken line is OJP model used in Supplementary Figure 6. (a) Input depth profile of β_V (black) and β_H(red); (b) that of radial anisotropy(ξ); (c) sensitivity kernel of ξ and (d) that of β_V; (e) recovered depth profile of β_V; (f) that of ξ. Black lines indicate an input model (strong LVZ model). Green and red lines denote recovered models by using only fundamental mode and up to 4th mode, respectively. Blue line indicates initial model.