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Peer Review File

Slab stagnation sustained by wet harzburgite and dry basaltic slab assemblages

Corresponding Author: Professor Danian Shi

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
This manuscript presents high-resolution seismic imaging and mineral physics modeling of stagnant slabs in the mantle transition zone (MTZ) beneath the northeastern margin of the Eurasian continent. The authors identify a deep discontinuity (~695 km) consistent with the akimotoite-bridgmanite transition in a dry basaltic assemblage and observe significant excess depression (~10-15 km) of the post-spinel phase boundary, likely induced by water. The main finding is that slab stagnation is primarily initiated and sustained by buoyancy generated from these water-induced excess depressions, with an additional contribution from dry basaltic assemblages localized at the center of the stagnation zone.
The work integrates advanced seismological methods with mineral physics modeling to provide important new insights into the long-standing geodynamic problem of slab stagnation, and the findings are significant for understanding mantle dynamics, water cycling, and phase transformations. Therefore the study is principle suitable for publication in NC. However, several major points require clarification and strengthening to make the argument more compelling prior to final acceptance. Please check the Attachments for the details.

Reviewer #2

(Remarks to the Author)

Reviewer #3

(Remarks to the Author)
Comments on the manuscript “Slab stagnation sustained by wet harzburgite and dry basaltic slab assemblages” by Shi et al.

This study presents a seismic image of the mantle transition zone beneath East Asia, revealing two notable structural features: (1) a 700-km-deep, 400-km-wide discontinuity accompanied by double seismic phases beneath the Pacific stagnant slab, and (2) a significant 10–15 km depression of the 660-km discontinuity. The authors interpret these observations using mineral physics modeling.

In my opinion, a critical issue is the robustness of the imaging results. As I know, the absolute depths of interfaces depend on the reference velocity model. The authors used ‘TX2019slab’, a global tomographic model, for upper-mantle velocity corrections. How can they be certain that imaged discontinuities, such as the one at 700 km depth, are real and not a bias due to the chosen model? This is crucial because these absolute depths and depressions directly determine the subsequent mineralogical modeling and interpretations. Therefore, I suggest that the authors try to demonstrate the success of the 3-D corrections using 'TX2019slab', and further validate their findings by testing with other 3-D velocity models mentioned in the manuscript, such as 'FWEA23' and/or 'UU-P07', to verify that the outputs yield consistent features.

Furthermore, the reliability of the 'double seismic phases' is questionable. While the authors indicate their presence in the non-stacked receiver functions (Extended Data Fig. 3), I contend that their visibility is likely influenced by the specific strategy employed for selecting the receiver functions. This potential bias raises concerns about their robustness. It would also be valuable to know whether similar features are observable in other parts of the study region, beyond the two cross-sections presented in Fig. 2a and c.

In summary, to evaluate the robustness and reliability of the imaging results is paramount, as it is fundamental to all subsequent mineralogical modeling and interpretations.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The authors have satisfactorily addressed all my concerns regarding the uniqueness of the water interpretation, the impact of velocity model uncertainties, details of buoyancy calculations, and precision of wording. The sensitivity tests represent the most substantial improvement to the manuscript.
The revised manuscript now meets the high standards of scientific rigor, completeness, and clarity required for publication in Nature Communications.
Therefore, I recommend acceptance of this manuscript for publication.

Reviewer #3

(Remarks to the Author)
All of my questions have been thoroughly answered. I have no further concerns and recommend acceptance.

Reviewer #4

(Remarks to the Author)
The manuscript provides an additional explanation to the mechanism of stagnant slab in the mantle transition zone in the western Pacific subduction zone. The authors highlight the buoyancy from the excess depressions in the pyrolite and harzburgite mantle and aided by buoyancy of the dry basaltic assemblage in the uppermost lower mantle. The leading evidence is the double positive phases beneath a local area in the eastern China.

This is a revision so that the authors have made considerable changes to the manuscript according to previous comments. Here I provide only some further concerns on the confidence of the results from seismological view.

The authors have proved that the double phases are clear in both individual and stacked images. My comment here is whether the second phase may be caused by a anisotropic layer right beneath the 660 km discontinuity. Previous results have shown that the top of the lower mantle may be strongly anisotropic. Therefore, in specific directions, the velocity in the anisotropic layer may be significantly low. Figure S3 is good, but from an event of specific direction. I suggest the authors to provide such examples of more events from different directions so that the influence of potential anisotropic structure may be excluded.

A related issue here, I would suggest the authors to conducted the bootstrapping procedures to estimate the uncertainties in the stacking receiver functions. It would help to convince the readers that the double phases are robust above the noises and uneven data samples. By the way, the authors should show the distribution of events used in this study.

Another major concern is the subjective selection of the cross-sections. In fact, the similar double phases are also notable in other regions, such as the left-most regions of Profile A and Label 4 in this profile. With the data set shown in Figure 1, it is possible to provide a comprehensive view of the distribution of the double phases, such as under the Hainan plume and Changbaishan volcano. It is necessary to confirm whether the double phases only exist under eastern China shown in this study or they are also visible in other regions. In fact many stations, particularly in NE China, are not used at all in the study. The data sets also provide nice coverage of the mantle transition zone in SE China, including the Hainan plume.

Version 2:

Reviewer comments:

Reviewer #4

(Remarks to the Author)
The authors have carefully revised the manuscript according my comments. Now I think it is ready for publication.

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Danian Shi
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Feb 8, 2026

Editor, *Nature Communications*

Dear Editor,

We have revised our manuscript according to the comments we received. To address the first concern raised by the reviewers, we have replaced Supplementary Fig. 3 with sample receiver functions for the double phases plotted in a time-distance format, clearly distinguishing the double phases from major known body wave arrivals from earthquake sources. In addition, we updated Supplementary Fig. 4 with the results corrected with three different tomographic models as suggested by the reviewers, which show that the maximum depression of the post-spinel discontinuity in the center of the stagnation zone remain unchanged, although the depression positions vary somewhat if different models are used. Therefore, using of different models does not change our core conclusions. We also added a new section about the method we used to validate the excess depressions, in order to address the second concern raised by a reviewer. Other modifications were made to better explain the interpretations and methods and follow the formatting instructions. We hope our modifications have met the requirements of the journal.

We are grateful to you and reviewers for the comments that have greatly improved our manuscript. The following are our point-by-point responses, marked in dark green.

Sincerely yours,

Danian Shi, Yang Shen, Rongtao Zhao, Jianyu Shi, Changxin Chen, Xiaoming Xu, Yongqian Zhang, Yao Xu, Hua Xiang, Yan Liu & Yan Yang

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

This manuscript presents high-resolution seismic imaging and mineral physics modeling of stagnant slabs in the mantle transition zone (MTZ) beneath the northeastern margin of the Eurasian continent. The authors identify a deep discontinuity (~695 km) consistent with the akimotoite-bridgmanite transition in a dry basaltic assemblage and observe significant excess depression (~10-15 km) of the post-spinel phase boundary, likely induced by water. The main finding is that slab stagnation is primarily initiated and sustained by buoyancy generated from these water-induced excess depressions, with an additional contribution from dry basaltic assemblages localized at the center of the stagnation zone.

The work integrates advanced seismological methods with mineral physics modeling to provide important new insights into the long-standing geodynamic problem of slab stagnation, and the findings are significant for understanding mantle dynamics, water cycling, and phase transformations. Therefore the study is principle suitable for publication in NC. However, several major points require clarification and strengthening to make the argument more compelling prior to final acceptance. Please check the Attachments for the details.

Thank you. We have revised the manuscript according to your suggestions and hope you find we have made the argument more compelling now.

Attachments for the details:

Major Concerns and Recommendations:

1) The paper's key argument heavily relies on the interpretation that the "excess depression" of the post-spinel transition is caused by water. While this is a plausible and cited hypothesis, the authors have not fully addressed alternative explanations or the potential for systematic errors from seismic velocity models.

a) The discussion should more explicitly acknowledge that the attribution to water is the preferred but not the only interpretation. Briefly discuss whether other minor elements (e.g., Al, Fe) could similarly affect the transition depth.

In the revised manuscript, we discussed the effects of the minor elements Al, Fe on the depression of the post-spinel transition boundary (Page 10, lines 1-7). Fe may shift the post-spinel transition boundary to lower pressure (Ito & Takahashi,1989), and Al may shift the boundary to higher pressure (Weidner & Wang, 1998). However, we did not observe significant deviation of the post-spinel transition boundary from the normal phase transition position at location '1', where the basaltic slab assemblage is enriched with Fe and Al. This makes us speculate that the depression effects of Al may have been largely counteracted by Fe, as suggested by Ghosh et al., 2013.

b) Although the manuscript states that under-correction of velocity would cause "apparent shallowing," a more quantitative assessment of the impact of velocity model uncertainties on the interface depth estimates would be beneficial. Could an error range or sensitivity tests be provided to demonstrate the robustness of the depth results?

We have addressed more clearly the under-correction and over-correction effects (see Page 9, lines 7-9, new Supplementary Fig. 4, new section for the validation in

Methods/Pages 15-16). Our test results with different tomographic models show that although the depression center may change with different models, due to the differences in the tomographic models, the maximum depression magnitude remain unchanged.

2) The authors propose two mechanisms, crustal detachment/peeling, and trapped subducted ridge, to explain the high basalt fraction observed at the stagnation center but do not evaluate which mechanism is more likely or provide evidence to distinguish between them. The discussion should be extended to argue which mechanism better fits the observed seismic geometry and images. Consider discussing how these two mechanisms might produce different thermo-chemical structures in geodynamic models and how future work (e.g., even igher-resolution tomography) could differentiate them.

Good suggestions, and we have updated Supplementary Fig. 9. Distinguishing these two mechanisms will require detailed and sophisticated geodynamic modeling, which is beyond the scope of our study. We are unable to distinguish them with our current profiles.

3) Figure 4 and the associated buoyancy calculations are central to the main conclusion. However, the model assumptions are not described in sufficient detail, which affects the reproducibility and strength of the argument.

We added the details in the caption of Fig.4

a) Key parameters for the buoyancy calculation, such as slab thickness, temperature anomaly (ΔT), and the basalt/harzburgite ratio, should be clearly stated in the main text or methods section.

We added the key parameters in the caption of Fig.4

b) It must be unequivocally stated whether the buoyancy from the "excess depression" calculation considers only the density change due to the phase shift, or if it also includes effects from adiabatic thermal expansion due to the downward displacement. The text mentions the latter is "ignored," but the justification for this and its potential impact should be clarified.

OK, added in the caption of Fig.4

c) The description of the brown and cyan shaded areas in Figure 4 ("approximately proportional") could be more precise. The methods section should detail how buoyancy is integrated or estimated from these density contrast profiles.

We changed “approximately proportional” to “proportional” in the caption of Fig.4 to correct the imprecise use of words. The buoyancy is simply obtained by trapezoidal integration from the density contrast profiles, we did not describe it in the methods because it is straight-forward itself.

Minors:

1) When describing phases '1' and '2' in Figure 2, it would be helpful to more directly state which phase transition each is interpreted to represent.

We now added “ri->br” and “ak->br” nearby '1' and '2' in Figure 2, respectively.

2) The statement dismissing the role of a garnetite layer (lines 262-263) could be phrased more diplomatically, e.g., "Our observations do not support the presence of a widely distributed garnetite layer that plays a major role in slab stagnation in this region."

Thank you for the suggestion. The sentence is revised (Page 13, lines19-20)

3) Suggest adding a simple schematic panel to Figure 3 or 4 that visually compares the buoyancy generation mechanism for a "normal" phase transition versus one with a "water-induced excess depression."

We think that our Fig.4c and Fig.4d have showed your ideas. We added “normal phase transition” in Fig.4c for clarity.

Reviewer #2 (Remarks to the Author):

The author uses P-to-S receiver functions to image the topography variations of the 410 and 660 km discontinuities beneath the northeast margin of the Eurasian continent. The resulting CCP images demonstrates double peaks near P660s arrivals, centered at ~117.5° and ~32° within a localized region of ~400 km. The deeper one of these peaks is likely caused by the akimotoite-to-bridgmanite phase transition at adiabatic temperature of ~1000 K estimated from mineral physics modeling. Beneath the southern margin of the North China Craton (NCC), the 660-km discontinuity is moderately depressed by ~10-15 km, which can be explained by the elevated water content at the bottom of the mantle transition zone (MTZ).

This manuscript presents interesting results. However, there are significant work to be done before acceptance. I would like to suggest a major revision.

OK. Thank you.

My main concerns are:

1. In the CCP stacking, it is difficult to determine whether the conversions at depths of ~670 km and ~695 km are caused by diffractions. The double peaks are visible at the center part of profile C. Such peaks (phases 1 and 2) are observed only at the western edge of the prominent depression in the A profile. Along these two files, the anomalous regions exhibiting the double peaks have relatively poor resolution due to sparse data coverage. The arrays of the NECsaids, NECESSArray and ChinArray-Ⅱ does not provide enough coverage of the eastern NCC and the Trans-North China Orogen (TNCO). Consequently, the image may contain artifacts due to insufficient data, and the double peaks should be interpreted with caution.

In the old Supplementary Fig. 3, we plotted the sample receiver functions for the double phases trace by trace, limited page size only allows us to plot a small portion of them without too much overlapping. In the new Supplementary Fig. 3, we plotted them in a time-distance format, which can allow us to plot all of them. We think our new figure can help to distinguish the double peaks from known major body wave arrivals from earthquake sources and artifacts (Page 6, line 21 to page 7, line 4).

2. According to the previous studies (Liu et al., 2015; Zhang et al., 2016; Zhang et al., 2022), a broad depression of the 660-km discontinuity can be seen at the extension of the deepest seismicity (region of 130°E, 40°-43°N). Is this feature also observed in the present study?

Thank you for pointing out the previous studies. Their profiles are hundred(s) kilometers north of our profiles B and C.
Yes, our phase ‘4’ is a southern extension of the feature in the previous studies. We have clarified this in the manuscript (Page 9, lines 12-14).

3. Compared with the depth variations of the corrected 410-km and 660-km discontinuities, lateral variations in the transition zone (TZ) thickness can offer key insights into thermal and compositional heterogeneity. The estimates on the transition zone thickness are suggested to be presented.

Thank you for the suggestion. We now showed them in new Supplementary Fig. 4b.

4. The seismic data section does not describe the waveforms recorded by

temporary stations from the ChinArray-Ⅱ.

Ok, added. We merged with others for limited citation in the old version of our manuscript.

References
Liu, Z., Niu, F., Chen, Y., Grand, S., Kawakatsu, H., Ning, J., Tanaka, S., Obayashi, M., Ni, J., 2015. Receiver function images of the mantle transition zone beneath NE China: new constraints on intraplate volcanism, deep subduction and their potential link. Earth Planet. Sci. Lett. 412, 101-111.
Zhang, R., Gao, Z., Wu, Q., Xie, Z. and Zhang, G., 2016. Seismic images of the mantle transition zone beneath Northeast China and the Sino-Korean craton from P-wave receiver functions. Tectonophysics, 675, 159-167.
Zhang Y., Niu F., Ning J., 2022. Mantle transition zone beneath northeast China imaged by receiver function data using fast marching eikonal solver based 3-D migration. Chinese Journal of Geophysics, 65(8), 2945-2959. doi: 10.6038/cjg2022P0596 (in Chinese with English abstract).

Reviewer #3 (Remarks to the Author):

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This study presents a seismic image of the mantle transition zone beneath East Asia, revealing two notable structural features: (1) a 700-km-deep, 400-km-wide discontinuity accompanied by double seismic phases beneath the Pacific stagnant slab, and (2) a significant 10–15 km depression of the 660-km discontinuity. The authors interpret these observations using mineral physics modeling.

In my opinion, a critical issue is the robustness of the imaging results. As I know, the absolute depths of interfaces depend on the reference velocity model. The authors used ‘TX2019slab’, a global tomographic model, for upper-mantle velocity corrections. How can they be certain that imaged discontinuities, such as the one at 700 km depth, are real and not a bias due to the chosen model? This is crucial because these absolute depths and depressions directly determine the subsequent mineralogical modeling and interpretations. Therefore, I suggest that the authors try to demonstrate the success of the 3-D corrections using 'TX2019slab', and further validate their findings by testing with other 3-D velocity models mentioned in the manuscript, such as 'FWEA23' and/or 'UU-P07', to verify that the outputs yield consistent features.

We have tested the corrections with different tomographic models TX2019slab UU-P07 and FWEA23 according to your suggestions. We used UU-P07 as a reference one. We updated Supplementary Fig. 4 and added a new section for the validation processes in Methods (Pages 15, lines 19-24, Page 16, Page 17, lines 1-3). Our tests show that the depression center may change from location '5' to '6', if we replaced model TX2019slab with FWEA23, but the maximum depression of the post-spinel discontinuity in the center of the stagnation zone remain unchanged.

Our tests show that our core conclusions remain unchanged no matter which tomographic model is used.

Furthermore, the reliability of the 'double seismic phases' is questionable. While the authors indicate their presence in the non-stacked receiver functions (Extended Data Fig. 3), I contend that their visibility is likely influenced by the specific strategy employed for selecting the receiver functions. This potential bias raises concerns about their robustness. It would also be valuable to know whether similar features are observable in other parts of the study region, beyond the two cross-sections presented in Fig. 2a and c.

We addressed this question in the response to reviewer #2 (also see page 6, line 21 to page 7, line 4). In the new Supplementary Fig. 3, we plotted individual receiver functions in the time-distance format, which show the differences in the moveout between the double seismic phases and major known noises.

While there are phases at similar depths of ~700 km (indicated by '7' and '8' in Fig.2), they are either too weak or near the margin of our data coverage to be confidently interpreted. Because of our uniform receiver function data processing and the fact that the "double seismic phases" is NOT observed under the majority of the study area, we believe phases 1 and 2 in our study are real and they reflect a limited spatial extend of basaltic assemblages.

In summary, to evaluate the robustness and reliability of the imaging results is paramount, as it is fundamental to all subsequent mineralogical modeling and interpretations.

Thank you! The comments and suggestions have been very helpful to improve the manuscript.

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Danian Shi
Chinese Academy of Geological Sciences
Email: shidanian@cags.ac.cn
May 30, 2026

Editor, *Nature Communications*

Dear Editor,

We have revised the manuscript in response to the comments we received. We believe that the main concern raised by reviewer #4 arose from the limited information provided in our previous Fig. S3. We have now moved this figure to the main text as Fig. 3 to better highlight the robustness of the observed double phases. Following the reviewer's suggestion, we also added two new panels: Fig. 3b, which shows the receiver functions stacked by back-azimuths, and Fig. 3c, which shows the back-azimuth coverage of all available receiver functions.

The revised figures demonstrate that the double phases are consistently observed over the full range of observed back-azimuths. They also show no significant systematic variation in receiver-function amplitude with back-azimuth. These observations help rule out anisotropic effects in the topmost lower mantle as the main cause of the double phases and strengthen our interpretation that the double phases are unlikely due to artifacts of random noise, heterogeneities along ray paths, or other-body-wave-generated noise. We have also addressed the reviewer's other comments and suggestions. We hope that the revised manuscript now meets the requirements for publication in the journal.

We are grateful to you and the reviewers for the thoughtful comments, which have again helped improve our manuscript. Below, we provide our point-by-point responses, with changes marked in green.

Sincerely yours,

Danian Shi, Yang Shen, Rongtao Zhao, Jianyu Shi, Changxin Chen, Xiaoming Xu, Yongqian Zhang, Yao Xu, Hua Xiang, Yan Liu & Yan Yang

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

The authors have satisfactorily addressed all my concerns regarding the uniqueness of the water interpretation, the impact of velocity model uncertainties, details of buoyancy calculations, and precision of wording. The sensitivity tests represent the most substantial improvement to the manuscript.

The revised manuscript now meets the high standards of scientific rigor, completeness, and clarity required for publication in Nature Communications.
Therefore, I recommend acceptance of this manuscript for publication.

We thank the reviewer for the positive assessment of our revised manuscript. We appreciate the constructive comments, which helped us improve the manuscript.

Reviewer #3 (Remarks to the Author):

All of my questions have been thoroughly answered. I have no further concerns and recommend acceptance.

We thank the reviewer for the positive assessment of our revised manuscript. We appreciate the constructive comments, which help us improve the manuscript.

Reviewer #4 (Remarks to the Author):

The manuscript provides an additional explanation to the mechanism of stagnant slab in the mantle transition zone in the western Pacific subduction zone. The authors highlight the buoyancy from the excess depressions in the pyrolite and harzburgite mantle and aided by buoyancy of the dry basaltic assemblage in the uppermost lower mantle. The leading evidence is the double positive phases beneath a local area in the eastern China.

This is a revision so that the authors have made considerable changes to the manuscript according to previous comments. Here I provide only some further concerns on the confidence of the results from seismological view.

The authors have proved that the double phases are clear in both individual and stacked images. My comment here is whether the second phase may be caused by a anisotropic layer right beneath the 660 km discontinuity. Previous results have shown that the top of the lower mantle may be strongly anisotropic. Therefore, in specific directions, the velocity in the anisotropic layer may be significantly low. Figure S3 is good, but from an event of specific direction. I suggest the authors to provide such examples of more events from different directions so that the influence of potential anisotropic structure may be excluded.

We thank the reviewer for these additional constructive comments. Following the reviewer's suggestions, we have moved the previous Supplementary Figure S3 to the main text as Figure 3. We have also added two new panels: Fig. 3b, which shows the RFs stacked by back-azimuths, and Fig. 3c, which shows the back-azimuth coverage of all the available RFs used to image the double phases.

The double phases can be consistently observed at different back-azimuths, as shown in the back-azimuthal stacks in Fig. 3b, and are also visible in the unstacked image sorted by epicentral distance in Fig. 3a. In addition, we do not observed significant systematic variation in receiver-function amplitude with back-azimuth. These observations suggest that the double phases are unlikely caused by an anisotropic layer at the top of the lower mantle.

We note that the high-quality data from the northwest back-azimuth are limited (Fig. 3C), which prevents us from carrying out a full azimuthal analysis of the double phases. Nevertheless, within the available back-azimuthal coverage, the revised figures show that the double phases are robust and are not limited to events from a single direction.

A related issue here, I would suggest the authors to conducted the bootstrapping procedures to estimate the uncertainties in the stacking receiver functions. It would help to convince the readers that the double phases are robust above the noises and uneven data samples. By the way, the authors should show the distribution of events used in this study.

We thank the reviewer for this helpful suggestion. We now have made the bootstrap test as suggested. Our results, presented in Fig. S2, show the uncertainties in the stacked receiver functions are low for the data used in our study and the low levels of uncertainties do not affect our interpretations and conclusions of the paper. The parameters of bootstrap sampling with replacement are described in the caption of Fig. S2.

We have also added Fig. 3c to show the back-azimuthal coverage of the receiver functions used to image the double phases and have also added Figs. S2d-e to further show the distribution of all the events used in this study.

Another major concern is the subjective selection of the cross-sections. In fact, the similar double phases are also notable in other regions, such as the left-most regions of Profile A and Label 4 in this profile. With the data set shown in Figure 1, it is possible to provide a comprehensive view of the distribution of the double phases, such as under the Hainan plume and Changbaishan volcano. It is necessary to confirm whether the double phases only exist under eastern China shown in this study or they are also visible in other regions. In fact many stations, particularly in NE China, are not used at all in the study. The data sets also provide nice coverage of the mantle transition zone in SE China, including the Hainan plume.

We thank the reviewer for raising this important point. We have carefully examined the weak double phases beneath Hainan near the left-most part of Profile A. However, these phases are too weak to be interpreted with confidence. Hainan and South China are characterized by a thin MTZ rather than thicker one (Wang & Huang, 2012; Wei & Chen, 2016; Yu et al., 2017； He & Santosh, 2021), and no slab stagnation is observed beneath Hainan or South China. Although we cannot exclude the possibility of weak basaltic enrichment in the topmost lower mantle related to mantle plume activity in this region, the current data coverage beneath Hainan and the South China Sea is insufficient for a conclusive interpretation.

Regarding phase ‘4’, the center portion is characterized mainly by a single phase, while no clear phase is observed at location ‘3’. Some weak and scattered phases are present near the margins of phase ‘4’, but they are not as clear or laterally coherent as the double phases interpreted in this study.

Previous studies have reported other double phases north of our profiles, and we cannot exclude the possibility that similar processes may also occur north of our study region. We focus on the present study area because it has better data coverage and displays a more pronounced subduction-related mantle structure. This allows us to interpret the observed double phases with greater confidence.

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Wang, C.Y., and Huang, J.L., 2012. Mantle transition zone structure beneath Hainan and adjacent areas derived from receiver function analysis. Chinese Journal of Geophysics, 55(6): 658–665.

Wei, S.S., and Chen, Y.J., 2016. Seismic evidence of the Hainan mantle plume by receiver function analysis in southern China. Geophysical Research Letters, 43: 8978–8985.

Yu, Y., Gao, S.S., Liu, K.H., Yang, T., Xue, M., and Le, K.P., 2017. Mantle transition zone discontinuities beneath the Indochina Peninsula: Implications for slab subduction and mantle upwelling. Geophysical Research Letters, 44(14): 7159–7167.

He, C., and Santosh, M., 2021. Mantle upwelling beneath the Cathaysia Block, South China. Tectonics, 40: e2020TC006447.

Review of Manuscript: NCOMMS-25-80697-T
Title: Slab stagnation sustained by wet harzburgite and dry basaltic slab assemblages
Journal: *Nature Communications* (NC)

This manuscript presents high-resolution seismic imaging and mineral physics modeling of stagnant slabs in the mantle transition zone (MTZ) beneath the northeastern margin of the Eurasian continent. The authors identify a deep discontinuity (~695 km) consistent with the akimotoite-bridgmanite transition in a dry basaltic assemblage and observe significant excess depression (~10-15 km) of the post-spinel phase boundary, likely induced by water. The main finding is that slab stagnation is primarily initiated and sustained by buoyancy generated from these water-induced excess depressions, with an additional contribution from dry basaltic assemblages localized at the center of the stagnation zone.
The work integrates advanced seismological methods with mineral physics modeling to provide important new insights into the long-standing geodynamic problem of slab stagnation, and the findings are significant for understanding mantle dynamics, water cycling, and phase transformations. Therefore the study is principle suitable for publication in NC. However, several major points require clarification and strengthening to make the argument more compelling prior to final acceptance.

Major Concerns and Recommendations:

1) The paper's key argument heavily relies on the interpretation that the "excess depression" of the post-spinel transition is caused by water. While this is a plausible and cited hypothesis, the authors have not fully addressed alternative explanations or the potential for systematic errors from seismic velocity models.
    a) The discussion should more explicitly acknowledge that the attribution to water is the preferred but not the only interpretation. Briefly discuss whether other minor elements (e.g., Al, Fe) could similarly affect the transition depth.
    b) Although the manuscript states that under-correction of velocity would cause "apparent shallowing," a more quantitative assessment of the impact of velocity model uncertainties on the interface depth estimates would be beneficial. Could an error range or sensitivity tests be provided to demonstrate the robustness of the depth results?

2) The authors propose two mechanisms, crustal detachment/peeling, and trapped subducted ridge, to explain the high basalt fraction observed at the stagnation center but do not evaluate which mechanism is more likely or provide evidence to distinguish between them. The discussion should be extended to argue which mechanism better fits the observed seismic geometry and images. Consider discussing how these two mechanisms might produce different thermo-chemical structures in geodynamic models and how future work (e.g., even higher-resolution tomography) could differentiate them.

3) Figure 4 and the associated buoyancy calculations are central to the main conclusion. However, the model assumptions are not described in sufficient detail, which affects the reproducibility and strength of the argument.
   a) Key parameters for the buoyancy calculation, such as slab thickness, temperature anomaly (ΔT), and the basalt/harzburgite ratio, should be clearly stated in the main text or methods section.
   b) It must be unequivocally stated whether the buoyancy from the "excess depression" calculation considers only the density change due to the phase shift, or if it also includes effects from adiabatic thermal expansion due to the downward displacement. The text mentions the latter is "ignored," but the justification for this and its potential impact should be clarified.
   c) The description of the brown and cyan shaded areas in Figure 4 ("approximately proportional") could be more precise. The methods section should detail how buoyancy is integrated or estimated from these density contrast profiles.

Minors:

1) When describing phases '1' and '2' in Figure 2, it would be helpful to more directly state which phase transition each is interpreted to represent.
2) The statement dismissing the role of a garnetite layer (lines 262-263) could be phrased more diplomatically, e.g., "Our observations do not support the presence of a widely distributed garnetite layer that plays a major role in slab stagnation in this region."
3) Suggest adding a simple schematic panel to Figure 3 or 4 that visually compares the buoyancy generation mechanism for a "normal" phase transition versus one with a "water-induced excess depression."

The author uses P-to-S receiver functions to image the topography variations of the 410 and 660 km discontinuities beneath the northeast margin of the Eurasian continent. The resulting CCP images demonstrates double peaks near P660s arrivals, centered at ~117.5º and ~32º within a localized region of ~400 km. The deeper one of these peaks is likely caused by the akimotoite-to-bridgmanite phase transition at adiabatic temperature of ~1000 K estimated from mineral physics modeling. Beneath the southern margin of the North China Craton (NCC), the 660-km discontinuity is moderately depressed by ~10-15 km, which can be explained by the elevated water content at the bottom of the mantle transition zone (MTZ).

This manuscript presents interesting results. However, there are significant work to be done before acceptance. I would like to suggest a major revision.

My main concerns are:

1. In the CCP stacking, it is difficult to determine whether the conversions at depths of ~670 km and ~695 km are caused by diffractions. The double peaks are visible at the center part of profile C. Such peaks (phases 1 and 2) are observed only at the western edge of the prominent depression in the A profile. Along these two files, the anomalous regions exhibiting the double peaks have relatively poor resolution due to sparse data coverage. The arrays of the NECsaids, NECESSArray and ChinArray-II does not provide enough coverage of the eastern NCC and the Trans-North China Orogen (TNCO). Consequently, the image may contain artifacts due to insufficient data, and the double peaks should be interpreted with caution.
2. According to the previous studies (Liu et al., 2015; Zhang et al., 2016; Zhang et al., 2022), a broad depression of the 660-km discontinuity can be seen at the extension of the deepest seismicity (region of 130º E, 40º -43º N). Is this feature also observed in the present study?
3. Compared with the depth variations of the corrected 410-km and 660-km discontinuities, lateral variations in the transition zone (TZ) thickness can offer key insights into thermal and compositional heterogeneity. The estimates on the transition zone thickness are suggested to be presented.
4. The seismic data section does not describe the waveforms recorded by temporary stations from the ChinArray-II.

References

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