

Assimilation of upper plate rocks at convergent margins contributes to the low $\delta^{18}\text{O}$ isotopic signature of erupted magma

Corresponding Author: Professor Xiaohui Li

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

Dear Professor Li,

Your manuscript titled "Light oxygen isotopes in Mariana magmas indicate assimilation of overriding plate crust" has now been seen by 3 reviewers, whose comments are appended below. In the light of their advice we regret to inform you that we cannot publish your manuscript in Communications Earth & Environment.

You will see that the reviewers raise substantive concerns, including several limitations and flaws in the way the geochemical data is being analyzed and interpreted, as well as the outstanding limitations that the numerical modeling has for supporting your conclusions. Taking these points together with our editorial considerations, we are unable to conclude that your manuscript represents a sufficiently novel and compelling advance over the body of related work in the literature. Unfortunately, these reservations are sufficiently important to preclude publication of this study in Communications Earth & Environment.

We are sorry that we cannot be more positive on this occasion and thank you for the opportunity to consider your work.

Best regards,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor
Communications Earth & Environment

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

Reviewer comments

Light oxygen isotopes in Mariana magmas indicate the assimilation of overriding plate crust. The manuscript presents interesting and novel results on the assimilation processes of the overriding crust in subduction zones. The manuscript is well-written and without any major flaws. I recommend publishing it with some minor revisions.

The naming of oxygen isotopes is a little bit confusing. The authors should clarify and be more consistent. E.g., they are talking about light, heavy, depleted, and enriched.

In the discussion section (Line 157 and following) the authors need to explain better how they would identify the freshness of the olivine. Which elements would differ in the zoning patterns?
In line 205, the authors mention radiogenic isotopes without naming the reference or explaining in detail which radiogenic isotopes are being discussed.

The discussion of the generation of light oxygen isotope production needs more details. So far, it is not 100% clear how the isotopic composition changes. Which hydrothermal alteration reactions are occurring? Specific reactions should be stated. The description of Fig. 3 is missing the description of O18 versus Fo and the order (A, B, C, D) is a bit mixed up.
Line 633: delete "with"

Reviewer #2 (Remarks to the Author):

"Light oxygen isotopes in Mariana magmas indicate assimilation of overriding plate crust" by Xiaohui Li and others presents oxygen isotope and major element data for olivine crystals collected from lavas erupted in three settings in an island arc system: arc, rift, and backarc spreading. The main conclusion of the paper is in the title, and if better justified, it will be of some interest. More importantly, this dataset is unique, which may allow for new insight into magma generation and differentiation across an island arc system. The paper will be of interest to the communities of petrologists studying island arcs and oceanic crust. Thus, I recommend publication after significant revision. Many of the interpretations of the data are sound, but major revision is needed to address several issues with its discussion sections. The issues I have identified are outlined in the annotated comments below. For example, some potential data interpretations are ruled out seemingly based solely on conjecture (see comments associated with Lines 168 and 192-194). In addition, the authors do not consider the unusual composition of the mantle beneath the Philippine Sea Plate in their interpretation (see comment associated with Lines 121-122). The comment associated with Lines 121-122 also suggests adding a discussion of CaO – oxygen isotope relationships in olivine, which may provide information about the water contents of associated magmas.

Title – The title should indicate to readers that the data were primarily collected on olivine.

Lines 42-43 – This sentence seems superfluous as written. In my opinion, it needs to be clarified or deleted.

Lines 50-52 – The elevated values cited are significantly higher than those of mantle. I suggest using a different adverb here, e.g., modestly.

Lines 53-54 – Suggestion: replace "been slightly modified by" with "contributions from"

Lines 55-56 – Suggestion: replace "challenging due to complex petrogenetic processes involving" with "complicated by"

Lines 60-61 - Equilibrium O isotope compositions of olivine crystals are typically ~0.5 per mil less than those of coexisting melts (e.g., Bindeman et al., 2004 GCA). This fact needs stating and it needs to be used in the interpretation.

Lines 65-66 - Suggestion: replace "processes" and "such as crustal assimilation" with "inputs" in place of "processes."

Lines 66-69 – The sentence starting with "Recent" seems superfluous and I recommend deleting it.

Lines 121-122 – This is not correct. The CaO concentrations in the olivines analyzed for this paper are within the typical range for lavas from arcs and other tectonic settings. Also, why compare only with mantle xenocrysts?

Somewhere around this part of the paper, the relationship of CaO concentrations to delta 18O and Fo values in the olivine should be explored. The dataset forms a triangular field on a plot of CaO wt% v delta 18O. Rift olivines mostly plot along the high delta 18O side of the triangle between apex 1 (~0.25, 5.5) and apex 2 (~0.09, 5.2). Backarc olivines plot largely between 1 and apex 3, which is defined by the arc olivines with the lowest delta 18O values (~0.15, 4.1). Arc olivines cluster towards apex 3 but extend towards the other two apexes.

A few thoughts to consider: Water contents of magmas influence CaO concentrations of coexisting olivine crystals (higher water = lower CaO, Gavrilenko et al., 2016), and olivines plotting near apexes 2 and 3 might reflect higher water concentrations in magmas versus those near apex 1. Water added during AFC might explain apex 3, although, if so, then the wide variation in CaO concentrations and delta 18O values for ~Fo 70 olivines imply heterogeneous assimilation or variations in the delta 18O values of assimilants. Perhaps high concentrations of subducted water explain apex 2. Olivines from the rift lavas have a particularly wide range in CaO concentrations but relatively homogeneous Fo abundances. Perhaps this range reflects variations in primary water abundances. Finally, the delta 18O value of ~5.5 for olivine crystals defining apex 1 implies an even higher magmatic value (~6.0). This value is above typical primary mantle-derived magmas with the exception of some OIB with EM2-like signatures (see Eiler et al., 1997, GCA). This may reflect the enriched mantle generally reflected in IBM basalts (see Hickey-Vargas, 1992, EPSL; Hickey-Vargas, 1998, JGR; Pearce et al., 1999 J. Pet.; Li et al., 2019, EPSL).

Lines 151-153 - There may be a better way to arrange these possibilities. (1) is fine, (2) perhaps should be "mantle composition and melting", and (3) perhaps should be "crystal fractionation and assimilation".

Around line 157 – Could any of the low delta 18O olivine crystals be mantle xenocrysts where serpentinite had been heated and metamorphosed back to olivine?

Line 168 – The statement: "but no evidence for that was found" needs some further explanation - what additional evidence would be needed to rule out this possibility?

Line 174 - unless the peridotite was affected by subducted fluids

Lines 192-194 – This sentence is not convincing. I could easily envision ways that slab components could cause heterogeneous isotopic compositions, as subducted crust may have wide-ranging oxygen isotopic compositions over relatively short distances. That said, low $\delta^{18}\text{O}$ slab components should have other geochemical characteristics that would distinguish them from high $\delta^{18}\text{O}$ slab components. Also, the Wang et al. (2015) study (ref. 13) has similar $\delta^{18}\text{O}$ -Fo relationships as are shown here, and those relationships are attributed to a slab influence.

Line 196 – The range of $\delta^{18}\text{O}$ values is 5-2-5.5.

Line 204 – Again - Fo70 olivines have wide-ranging $\delta^{18}\text{O}$ values.

Line 205 – But there are no radiogenic isotope data reported here. This needs some further explanation.

Line 211 - But there are enriched components in the mantle beneath the Philippine Sea Plate that could have relatively high $\delta^{18}\text{O}$ values.

Lines 239-240 - Fig. 4a already has a subduction zone extending to more than 150 km depth. How can this be a new subduction zone? In addition, it is well known that subduction began in this system at ~52 Ma (e.g., Reagan et al., 2019, EPSL). At minimum, the ages used in this discussion and the uniqueness of the model need clarification. My opinion, however, is that this entire discussion seems extraneous to this paper. It may be better off in its own paper focusing on geodynamics, where the model can be adequately explained and justified.

Line 269 – Add “>” before “(84)”

Lines 270 and 300 - Something is amiss here. The crystallization temperatures for the relatively forsteritic olivine crystals analyzed in this study must have been higher than these calculated values. Also, the stated 0.2 degree C error is meaningless ($n=2$, and the error for the when $n=4$ is 178 degrees!). I recommend either using different geothermometers or deleting this part of the discussion.

Line 271 - The citation to reference 41 seems inappropriate here.

Line 363 - Were the oxygen isotope analyses done on the same spots as the microprobe analyses, or just the same grains? This needs clarification.

Reviewer #3 (Remarks to the Author):

In this manuscript by Li and collaborators a new hypothesis on the processes leading to $\delta^{18}\text{O}$ differences in oceanic arc magmas. I am not an expert on oceanic arc magmatism and hence cannot assess the novelty of presented ideas.

Regardless, there are several concerning aspects within this manuscript:

1. Authors use a limited array of 6 samples from existing publications to build their arguments, from which only one belongs to arc magmatism itself. While the latter has clear differences with the rifting samples, the argued similarities between samples from arc and back-arc are not that obvious. It appears that back-arc population has a significant scatter in most of the presented diagrams. Although olivine analyses are abundant within each sample, I am not convinced that the sample population is sufficient to support the authors' claims.

2. Samples are not properly identified in the text, and diagrams do not point what setting each sample belongs to. Although fractionation degree of the samples is a major point on the discussion, major elements from these samples are not reported in this study. The authors heavily rely on a former work, where they published these same samples. I believe the presented data is new (lack of identifiers makes this needlessly hard to assess), but some key information for the authors' discussion is missing in this manuscript.

3. Authors use a well-known numerical code to conduct their numerical study, so I am not concerned about the theoretical aspects of their numerical models. However, they rely on one single numerical model to support their conclusions. This is especially problematic considering they mostly focus on the overriding plate deformation, widely known to be heavily parameter dependent. While a full parameter study deserves its own separate paper, a basic work on the effects of, at least, convergence velocity and age of the oceanic lithosphere, should be supplied in a manuscript with this scope. This work is visibly absent.

4. The chosen parameters do not have any justification either. Authors use an asymmetrical convergence of 1 and 5 cm/yr, and ocean age of 40 Myr. These values are common within the modelling community, but so is anything ranging from 1-7 cm/yr and 40-100 Myr, influencing the outcome of the models. They do not provide any references on existing modelling works on oceanic subduction to support their selected methods/parameters or conclusions either. As it currently stands, there is no assessment on the robustness of the author's modelling work, and hence there is no way to tell whether this is a numerical construct of the preferred scenario from the authors.

I do not necessarily disagree with the conclusions, but I believe they are not sufficiently supported by the provided amount of evidence to justify publication in a Nature journal. Thus, I cannot recommend this manuscript for publication in its current state.

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Professor Li,

Your revised manuscript titled "Light oxygen isotopes in olivine of Mariana convergent margin lavas indicate assimilation of overriding-plate crust" has now been seen by the original reviewer #1 and a new reviewer (#4), who evaluated your responses to the original reviewer #2 (who couldn't provide further comment). Their comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. 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,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor, Communications Earth & Environment
Consulting Editor, Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors carefully revised their manuscript and answered the majority of my comments and the other reviewers' comments.

The authors removed the section about the numerical model. It would have been good to strengthen this part of the discussion based on other reviewers' comments. But maybe this is beyond the scope of this manuscript. I recommend publishing the manuscript.

Reviewer #4 (Remarks to the Author):

The responses look comprehensive, and I have no concerns that the reviewer's questions have been fully responded to. I would edit Lines 196 to 198 to read 'The radiogenic Sr-Pb isotopic compositions of whole rocks from the studied three tectonic settings are similar, which indicates... '.

My only question relates to the thermal constraints of melting mafic crust during assimilation fractional crystallisation (Lines 250 to 254). This will have a high melting temperature, which may be greater than the temperature of evolved magmas. It would be helpful to discuss this in the manuscript.

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Reviewers' comments:

Reviewer #1 (Remarks to the Author):

Reviewer comments

Light oxygen isotopes in Mariana magmas indicate the assimilation of overriding plate crust. The manuscript presents interesting and novel results on the assimilation processes of the overriding crust in subduction zones. The manuscript is well-written and without any major flaws. I recommend publishing it with some minor revisions.

Response: We are very grateful for these constructive comments, which are very helpful to improve the quality of our manuscripts. We have taken some of your suggestions and made substantial revisions to the manuscript.

The naming of oxygen isotopes is a little bit confusing. The authors should clarify and be more consistent. E.g., they are talking about light, heavy, depleted, and enriched.

Response: Thank you for your comment. In the revised version, we standardized the description of oxygen isotopes. We adopt light and heavy oxygen isotopic compositions and low and high $\delta^{18}\text{O}$ values.

In the discussion section (Line 157 and following) the authors need to explain better how they would identify the freshness of the olivine. Which elements would differ in the zoning patterns?

Response: In this study, we first conducted a detailed microscopic observation of olivine. Most olivines are euhedral to subhedral. No obvious olivine alteration was observed under the microscope. In addition, when we selected olivine under a binocular microscope, we carefully chose only fresh olivine with clean surfaces to ensure the freshness of the olivine. If olivine has obvious component zonation, the contents of Mg and Fe in the zonation may vary. We have added “In this study, olivines were carefully selected under a binocular microscope to ensure their freshness...” in lines 148-149.

In line 205, the authors mention radiogenic isotopes without naming the reference or explaining in detail which radiogenic isotopes are being discussed.

Response: Thank you for your comment. We have changed “Consideration of radiogenic isotopic data indicates that this is unlikely to be a mixing trend, and more likely reflects contamination of primary magma by low $\delta^{18}\text{O}$ material during differentiation.” to “Consideration of radiogenic Sr-Pb isotopic data of whole rocks remain stable in the studied three tectonics^{24,36}, which indicates that this is unlikely to be a mixing trend, and more likely reflects contamination of primary magma by low $\delta^{18}\text{O}$ material during differentiation” in lines 196-200.

The discussion of the generation of light oxygen isotope production needs more details. So far, it is not 100% clear how the isotopic composition changes. Which hydrothermal alteration reactions are occurring? Specific reactions should be stated.

Response: Thank you for your comment. In this study, we propose that the production of low- $\delta^{18}\text{O}$ olivine is mainly due to the assimilation of low- $\delta^{18}\text{O}$ wall rocks during fractional crystallization, namely the assimilation fractional crystallization (AFC) process. In the discussion section “Origin

of the low $\delta^{18}\text{O}$ magma”, we excluded the influence of post-magmatic alteration, partial melting and fractional crystallization, and mantle source metasomatism on the oxygen isotopes of olivine, and clearly identified that assimilation was the main cause of the decrease in oxygen isotopes of olivine. Regarding the formation of low- $\delta^{18}\text{O}$ wall rocks, we believe that this is mainly due to the alteration of lower- to mid-crustal wall rocks by high-temperature hydrothermal fluids. The process of high-temperature hydrothermal alteration is complex, and it is difficult for us to provide a specific reaction process. This process mainly involves the exchange of oxygen isotopes, resulting in a low oxygen isotope composition of the wall rock. A large number of studies have shown that the lower oceanic crust with high-temperature hydrothermal alteration has the characteristic of low oxygen isotope compositions (Gregory and Taylor, 1981; Eiler, 2001). The focus of this study is not to clarify the specific reactions that lead to the change of oxygen isotopes in the wall rock, but to emphasize the process of assimilating the wall rock during the evolution of the island arc magma. Hope you will be satisfied with our explanations and clarifications.

Eiler, J. M. Oxygen isotope variations of basaltic lavas and upper mantle rocks. Rev. Mineral. Geochem. 43, 319-364 (2001).

Gregory, R. T. & Taylor Jr, H. P. An oxygen isotope profile in a section of Cretaceous oceanic crust, Samail Ophiolite, Oman: Evidence for $\delta^{18}\text{O}$ buffering of the oceans by deep (> 5 km) seawater-hydrothermal circulation at mid-ocean ridges. J. Geophys. Res. Solid Earth 86, 2737-2755 (1981).

The description of Fig. 3 is missing the description of O18 versus Fo and the order (A, B, C, D) is a bit mixed up.

Response: We have modified the Figure 3 and the figure caption.

Line 633: delete “with”

Response: We have deleted “with” in this sentence.

Reviewer #2 (Remarks to the Author):

“Light oxygen isotopes in Mariana magmas indicate assimilation of overriding plate crust” by Xiaohui Li and others presents oxygen isotope and major element data for olivine crystals collected from lavas erupted in three settings in an island arc system: arc, rift, and backarc spreading. The main conclusion of the paper is in the title, and if better justified, it will be of some interest. More importantly, this dataset is unique, which may allow for new insight into magma generation and differentiation across an island arc system. The paper will be of interest to the communities of petrologists studying island arcs and oceanic crust. Thus, I recommend publication after significant revision. Many of the interpretations of the data are sound, but major revision is needed to address several issues with its discussion sections. The issues I have identified are outlined in the annotated comments below. For example, some potential data interpretations are ruled out seemingly based

solely on conjecture (see comments associated with Lines 168 and 192-194). In addition, the authors do not consider the unusual composition of the mantle beneath the Philippine Sea Plate in their interpretation (see comment associated with Lines 121-122). The comment associated with Lines 121-122 also suggests adding a discussion of CaO – oxygen isotope relationships in olivine, which may provide information about the water contents of associated magmas.

Response: We are grateful for your constructive comments that helped us strengthen our arguments. We have revised the manuscript according to your comments and suggestions. We hope you will be satisfied with our response and efforts in the revision process.

Title – The title should indicate to readers that the data were primarily collected on olivine.

Response: We have changed title “Light oxygen isotopes in Mariana magmas indicate assimilation of overriding plate crust” to “Light oxygen isotopes in olivine of Mariana convergent margin lavas indicate assimilation of overriding-plate crust”.

Lines 42-43 – This sentence seems superfluous as written. In my opinion, it needs to be clarified or deleted.

Response: We have deleted this sentence.

Lines 50-52 – The elevated values cited are significantly higher than those of mantle. I suggest using a different adverb here, e.g., modestly.

Response: Thank you for your suggestion. We have changed “slightly elevated values” to “modestly elevated values” in this sentence (line 49).

Lines 53-54 – Suggestion: replace “been slightly modified by” with “contributions from”

Response: Thank you for your suggestion. We have changed “been slightly modified by” to “contributions from” in this sentence (line 52).

Lines 55-56 – Suggestion: replace “challenging due to complex petrogenetic processes involving” with “complicated by”

Response: Thank you for your suggestion. We have replaced “challenging due to complex petrogenetic processes involving” with “complicated by” in this sentence (line 54).

Lines 60-61 - Equilibrium O isotope compositions of olivine crystals are typically ~0.5 per mil less than those of coexisting melts (e.g., Bindeman et al., 2004 GCA). This fact needs stating and it needs to be used in the interpretation.

Response: Thank you for your suggestion. We have added this sentence “Equilibrium oxygen isotope compositions of olivines are typically ~0.5 per mil less than those of coexisting melts” in lines 57-58, and used it in the discussion (lines 160-162).

Lines 65-66 - Suggestion: replace “processes” and “such as crustal assimilation” with “inputs” in place of “processes.”

Response: Thank you for your suggestion. We have changed “can reveal crustal processes accompanying fractionation, such as crustal assimilation” to “crustal inputs accompanying fractionation” in this sentence (lines 63-64).

Lines 66-69 – The sentence starting with “Recent” seems superfluous and I recommend deleting it.
Response: We have deleted this sentence.

Lines 121-122 – This is not correct. The CaO concentrations in the olivines analyzed for this paper are within the typical range for lavas from arcs and other tectonic settings. Also, why compare only with mantle xenocrysts?

Response: Thank you for your comment. We want to determine whether the studied olivines are crystallized from arc magma or are mantle xenocrysts. If they are mantle xenocrysts, the crystallization and evolution processes of island arc magma cannot be inferred. The contents of CaO in olivines can be used to infer the source of olivines. If they are mantle xenocrysts, then the contents of CaO are usually low, < 0.1 wt.% (as shown in the figure below; Thompson and Gibson, 2000;). As you said, the CaO contents of our studied olivines fall within the typical range of island arc lava, indicating that most studied olivines originated from the crystallization of island arc magma. We have added “indicating that these olivines crystallized from arc magma” in lines 114-115. Hope our explanation can satisfy you.

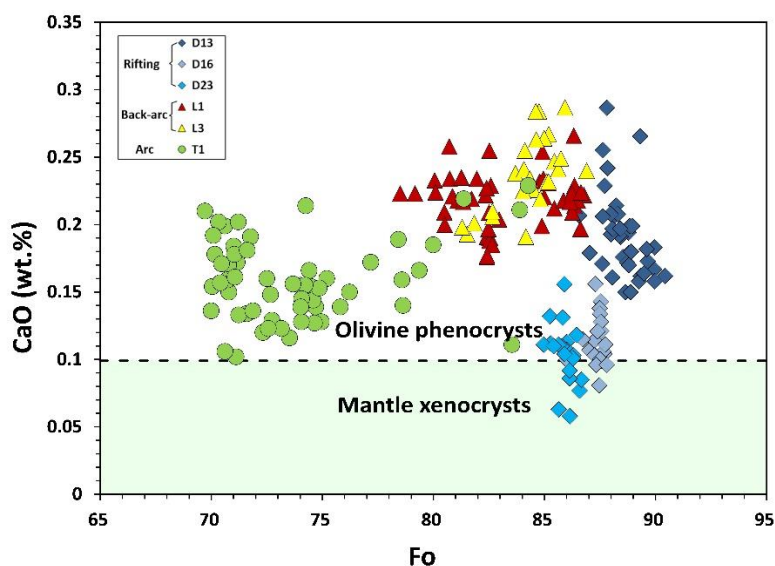


Figure 1. CaO contents versus Fo values of studied olivines from the Mariana convergent margin. The dashed line that separates olivine phenocrysts from mantle xenocrysts based on CaO is from Thompson and Gibson (2000).

Thompson, R., and Gibson, S. 2000. Transient high temperatures in mantle plume heads inferred from magnesian olivines in Phanerozoic picrites. *Nature*, 407, 502–506.

Somewhere around this part of the paper, the relationship of CaO concentrations to delta 18O and Fo values in the olivine should be explored. The dataset forms a triangular field on a plot of CaO wt% v delta 18O. Rift olivines mostly plot along the high delta 18O side of the triangle between apex 1 (~0.25, 5.5) and apex 2 (~0.09, 5.2). Backarc olivines plot largely between 1 and apex 3, which is defined by the arc olivines with the lowest delta 18O values (~0.15, 4.1). Arc olivines cluster towards apex 3 but extend towards the other two apexes.

A few thoughts to consider: Water contents of magmas influence CaO concentrations of coexisting

olivine crystals (higher water = lower CaO, Gavrilenko et al., 2016), and olivines plotting near apexes 2 and 3 might reflect higher water concentrations in magmas versus those near apex 1. Water added during AFC might explain apex 3, although, if so, then the wide variation in CaO concentrations and delta 18O values for ~Fo 70 olivines imply heterogeneous assimilation or variations in the delta 18O values of assimilants. Perhaps high concentrations of subducted water explain apex 2. Olivines from the rift lavas have a particularly wide range in CaO concentrations but relatively homogeneous Fo abundances. Perhaps this range reflects variations in primary water abundances. Finally, the delta 18O value of ~5.5 for olivine crystals defining apex 1 implies an even higher magmatic value (~6.0). This value is above typical primary mantle-derived magmas with the exception of some OIB with EM2-like signatures (see Eiler et al., 1997, GCA). This may reflect the enriched mantle generally reflected in IBM basalts (see Hickey-Vargas, 1992, EPSL; Hickey-Vargas, 1998, JGR; Pearce et al., 1999 J. Pet.; Li et al., 2019, EPSL).

Response: Thank you for your comment. The presence of water can affect the content of CaO in olivine, thereby reflecting the possible assimilation process. According to your suggestion, we have added relevant discussions in the revised manuscript (lines 266-274, 589-592).

Lines 151-153 - There may be a better way to arrange these possibilities. (1) is fine, (2) perhaps should be "mantle composition and melting", and (3) perhaps should be "crystal fractionation and assimilation".

Response: Thank you for your comment and suggestion. We have modified the relevant descriptions in lines 142-143.

Around line 157 – Could any of the low delta 18O olivine crystals be mantle xenocrysts where serpentinite had been heated and metamorphosed back to olivine?

Response: No, we think this is unlikely, for 3 reasons. Firstly, these low- $\delta^{18}\text{O}$ olivines have lower Fo values, which differ from the high Mg# of serpentinite. In addition, these low- $\delta^{18}\text{O}$ olivines have high CaO contents, which differ from the low CaO contents of mantle xenocrysts. Thirdly, we conducted a strict microscopic selection of the analyzed olivines to ensure their freshness. We have added "In this study, olivines were carefully selected under a binocular microscope to ensure their freshness" in lines 148-149. Hope you are satisfied with our explanation.

Line 168 – The statement: "but no evidence for that was found" needs some further explanation - what additional evidence would be needed to rule out this possibility?

Response: We have changed "but no evidence for that was found" to "but olivines with high Fo values have mantle-like oxygen isotope compositions" in this sentence (lines 158-159).

Line 174 - unless the peridotite was affected by subducted fluids

Response: Yes, we assume that the mantle peridotite is fresh. If affected by subducted fluids, all olivines should exhibit different oxygen isotope compositions from that of the mantle, which is inconsistent with our observations.

Lines 192-194 – This sentence is not convincing. I could easily envision ways that slab components could cause heterogeneous isotopic compositions, as subducted crust may have wide-ranging oxygen isotopic compositions over relatively short distances. That said, low delta 18O slab

components should have other geochemical characteristics that would distinguish them from high $\delta^{18}\text{O}$ slab components. Also, the Wang et al. (2015) study (ref. 13) has similar $\delta^{18}\text{O}$ -Fo relationships as are shown here, and those relationships are attributed to a slab influence.

Response: Thank you for your comment. Bindeman et al. (2005) has studied the oxygen isotope composition of slab melting in modern subduction zones. Some archetypical adakites interpreted to be nearly pure melts of basaltic and gabbroic rocks in subducting slabs have $\delta^{18}\text{O}$ slightly higher than those of normal MORBs, suggesting that it is difficult for the slab melting to produce low-oxygen isotope melts in mantle wedge. This is mainly attributed to the fact that different parts of the slab can typically melt during subduction, so that higher- and lower- $\delta^{18}\text{O}$ components average out to have no net difference from average mantle. The Mariana subduction zone is close to the trench, and the mantle wedge is influenced by fluids and melts from different parts of the subducted slab (low-T altered upper oceanic crust and high-T altered lower oceanic crust), which should mainly show mantle-like oxygen isotope compositions. This is consistent with the studied high Fo olivine, which has a similar oxygen isotope composition to the mantle. We note that Wang et al. (2015) studied intraplate basalts, which are far from any subduction zone and are mainly influenced by the material from the mantle transition zone, where ancient lower oceanic crust may have been tapped to contribute to intraplate magma. In the modified version, we have added “In addition, thermal models show that different parts of the slab can typically melt during subduction, forming mantle-like or higher $\delta^{18}\text{O}$ melts rather than low- $\delta^{18}\text{O}$ melts in mantle wedge” (lines 186-188).

Line 196 – The range of $\delta^{18}\text{O}$ values is 5-2-5.5.

Response: We have revised it in line 184.

Line 204 – Again - Fo70 olivines have wide-ranging $\delta^{18}\text{O}$ values.

Response: Thank you for your comment. Here, we emphasize the endmembers with low oxygen isotopes. We cannot determine the exact $\delta^{18}\text{O}$ of this end-member, but based on the evolution trend of olivine, it can be determined that the $\delta^{18}\text{O}$ of this end-member is less than 4‰, so we write it as <4‰ in line 196.

Line 205 – But there are no radiogenic isotope data reported here. This needs some further explanation.

Response: Thank you for your comment. We have changed “Consideration of radiogenic isotopic data indicates that this is unlikely to be a mixing trend, and more likely reflects contamination of primary magma by low $\delta^{18}\text{O}$ material during differentiation.” to “Consideration of radiogenic Sr-Pb isotopic compositions of whole rocks remain stable in the studied three tectonics, which indicates that this is unlikely to be a mixing trend, and more likely reflects contamination of primary magma by low $\delta^{18}\text{O}$ material during differentiation” in lines 196-200.

Line 211 - But there are enriched components in the mantle beneath the Philippine Sea Plate that could have relatively high $\delta^{18}\text{O}$ values.

Response: Thank you for your comment. We have added this sentence “the enriched components in the mantle beneath the Philippine Sea Plate that could have relatively high $\delta^{18}\text{O}$ values” in lines 204-205.

Lines 239-240 - Fig. 4a already has a subduction zone extending to more than 150 km depth. How can this be a new subduction zone? In addition, it is well known that subduction began in this system at ~52 Ma (e.g., Reagan et al., 2019, EPSL). At minimum, the ages used this discussion and the uniqueness of the model need clarification. My opinion, however, is that this entire discussion seems extraneous to this paper. It may be better off in its own paper focusing on geodynamics, where the model can be adequately explained and justified.

Response: Thank you for your comment and suggestion. Indeed, we originally intended to provide some evidence of numerical simulation, but precise numerical simulation is difficult to describe simply and requires more professional explanations. Therefore, in the revised version, we adopted your suggestion and have deleted the relevant discussions on numerical simulation. However, this does not affect the overall conclusion of this article.

Line 269 – Add “>” before “(84)”

Response: We have revised it.

Lines 270 and 300 - Something is amiss here. The crystallization temperatures for the relatively forsteritic olivine crystals analyzed in this study must have been higher than these calculated values. Also, the stated 0.2 degree C error is meaningless (n=2, and the error for the when n=4 is 178 degrees!). I recommend either using different geothermometers or deleting this part of the discussion.

Response: Thank you for your comment and suggestion. We have deleted the application of geothermometer for arc and back-arc olivines and deleted the related discussion.

Line 271 - The citation to reference 41 seems inappropriate here.

Response: In the revised version, we have re-examined the citations of the references.

Line 363 - Were the oxygen isotope analyses done on the same spots as the microprobe analyses, or just the same grains? This needs clarification.

Response: Thank you for your comment. We have added “Most of the spots analyzed by oxygen isotopes are the same as those analyzed by EPMA” in lineS 339-340. Due to the relatively uniform composition of olivine, the multiple oxygen isotope data of the same grains are basically consistent.

Reviewer #3 (Remarks to the Author):

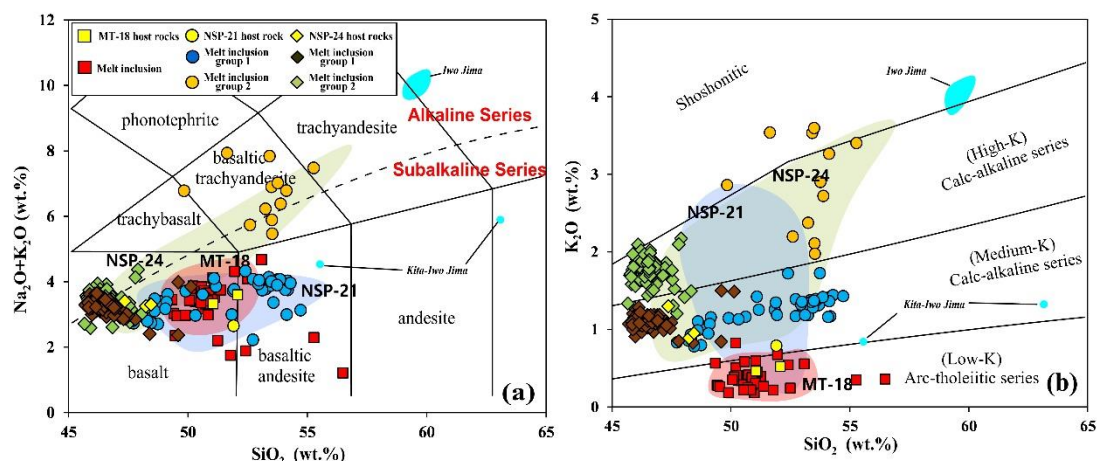
In this manuscript by Li and collaborators a new hypothesis on the processes leading to $\delta^{18}\text{O}$ differences in oceanic arc magmas. I am not an expert on oceanic arc magmatism and hence cannot assess the novelty of presented ideas.

Regardless, there are several concerning aspects within this manuscript:

1. Authors use a limited array of 6 samples from existing publications to build their arguments, from which only one belongs to arc magmatism itself. While the latter has clear differences with the

rifting samples, the argued similarities between samples from arc and back-arc are not that obvious. It appears that back-arc population has a significant scatter in most of the presented diagrams. Although olivine analyses are abundant within each sample, I am not convinced that the sample population is sufficient to support the authors' claims.

Response: Thank you for your comment. These six whole rock samples were reported in our previous article (Li et al., 2024). We have conducted research on the olivine-hosted melt inclusions in these samples. Additionally, we combined the research of the whole rock in the studied region to prove the rationality and representativeness of our samples. When plotting whole rock data (as shown in the figure below; Li et al., 2024), we collected all published data for igneous rocks of the study area, which span a compositional range covering our whole rock samples and indicating that our studied whole rock compositional data is credible and representative. In addition, we focus on the evolution process of olivine. The composition of olivine varies greatly, representing a reasonable and possible process of magmatic evolution. Unlike the analysis of whole rock which requires a large number of samples, micro-analysis focuses on the magmatic evolution process and needs more micro-analysis data rather than too many host whole rock samples. Hope you will be satisfied with our explanation.



The Figure S2 in our previous article (Li et al., 2024), which clearly shows that the studied host whole rocks fall within the compositional range of olivine-bearing rocks from the study region, indicating that the analyzed samples are representative.

Li, X. et al. A HIMU-like component in Mariana Convergent Margin magma sources during initial arc rifting revealed by melt inclusions. *Nat. Commun.* 15, 4088 (2024).

2. Samples are not properly identified in the text, and diagrams do not point what setting each sample belongs to. Although fractionation degree of the samples is a major point on the discussion, major elements from these samples are not reported in this study. The authors heavily rely on a former work, where they published these same samples. I believe the presented data is new (lack of identifiers makes this needlessly hard to assess), but some key information for the authors' discussion is missing in this manuscript.

Response: Thank you for your comment. We have modified the relevant diagrams to present the geological setting corresponding to each sample. This study mainly reports the elemental and oxygen isotope changes of olivine, and the data are presented in the supplementary tables. The data

of the host whole rocks and detailed sample information are quoted from our previous article (Li et al., 2024) and are easily accessible. We hope our approach can be accepted by you.

3. Authors use a well-known numerical code to conduct their numerical study, so I am not concerned about the theoretical aspects of their numerical models. However, they rely on one single numerical model to support their conclusions. This is especially problematic considering they mostly focus on the overriding plate deformation, widely known to be heavily parameter dependent. While a full parameter study deserves its own separate paper, a basic work on the effects of, at least, convergence velocity and age of the oceanic lithosphere, should be supplied in a manuscript with this scope. This work is visibly absent.

Response: You are an expert in numerical simulation. The problems you mentioned are indeed those we need to solve, but this might require a separate paper for analysis and explanation. In our current study, numerical simulation merely provides one aspect of the evidence for the possibility we hypothesize. Our hypothesis is mainly based on the olivine element and oxygen isotopes. Without numerical simulation, our inference is also reasonable. Reviewer #2 also suggested that the numerical simulation was not strongly related to our geochemical research. Therefore, in order to avoid misunderstanding, we deleted the numerical simulation section and its related discussions in the revised version. From a geochemical perspective, this will not affect the integrity and novelty of our manuscript. Later, we plan conduct a detailed discussion on numerical simulation and expect to form an independent article. Hope to receive your recognition and support.

4. The chosen parameters do not have any justification either. Authors use an asymmetrical convergence of 1 and 5 cm/yr, and ocean age of 40 Myr. These values are common within the modelling community, but so is anything ranging from 1-7 cm/yr and 40-100 Myr, influencing the outcome of the models. They do not provide any references on existing modelling works on oceanic subduction to support their selected methods/parameters or conclusions either. As it currently stands, there is no assessment on the robustness of the author's modelling work, and hence there is no way to tell whether this is a numerical construct of the preferred scenario from the authors.

Response: Thank you for your comment. In order to present our article more directly from a geochemical perspective and to avoid unnecessary misunderstandings, in the revised manuscript, we have deleted the numerical simulation section. However, the valuable suggestions you raised will be taken into consideration in our future numerical simulation work, and we hope to complete another independent paper on numerical simulation. Hope you can understand and support.

I do not necessarily disagree with the conclusions, but I believe they are not sufficiently supported by the provided amount of evidence to justify publication in a Nature journal. Thus, I cannot recommend this manuscript for publication in its current state.

Response: Thank you for your comment. In the revised version, we made detailed revisions to the manuscript based on the reviewers' comments. Using olivine oxygen isotopes, we demonstrate for the first time that the overlying plate contributes to the generation of island arc magmas, which has been neglected in previous studies. We think our work is worthy of attention and it makes a useful point about both oceanic-arc volcanism and the need to consider neglected factors in the interpretation of arc element and isotope data. As advocated by Reviewers #1 and #2, this should be a novel article. We sincerely hope to receive your recognition and support.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors carefully revised their manuscript and answered the majority of my comments and the other reviewers' comments.

The authors removed the section about the numerical model. It would have been good to strengthen this part of the discussion based on other reviewers' comments. But maybe this is beyond the scope of this manuscript.

I recommend publishing the manuscript.

[Response: We thank the reviewer for this supportive feedback.](#)

Reviewer #4 (Remarks to the Author):

The responses look comprehensive, and I have no concerns that the reviewer's questions have been fully responded to. I would edit Lines 196 to 198 to read 'The radiogenic Sr-Pb isotopic compositions of whole rocks from the studied three tectonic settings are similar, which indicates... '.

My only question relates to the thermal constraints of melting mafic crust during assimilation fractional crystallisation (Lines 250 to 254). This will have a high melting temperature, which may be greater than the temperature of evolved magmas. It would be helpful to discuss this in the manuscript.

[Response: Thank you for your positive comments and suggestions. What we want to emphasize in this sentence is that the Sr-Pb isotopes of different samples in the same area are similar, but there are differences in the Sr-Pb isotopes among different study areas. We have changed "Consideration of radiogenic Sr-Pb isotopic compositions of whole rocks remain stable in the studied three tectonics, which indicates..." to "The radiogenic Sr-Pb isotopic compositions of whole rocks from the studied three tectonic settings remain stable, which indicates..." in lines 196-198. Your suggestions on the discussion of temperature are very constructive and important. However, since spinel inclusions are rarely present in olivine with low oxygen isotopes, it is difficult to precisely limit the temperature of the evolving magma. We calculated the temperature of high-Fo olivine with mantle-like oxygen isotopes in initial rifting. Their crystallization temperature is \$\sim 1195 \pm 70\$ °C, so the temperature of the evolved magma should be lower. However, it is very difficult to compare it with the temperature of the melting mafic crust. Therefore, we did not discuss the relationship between temperatures in the revised manuscript. Reviewer #2 also suggested in the previous comments that we delete the discussion on temperature. Hope you will be satisfied with our modification.](#)