

Reply to Referee Comment #1 (RC1)

Thank you for your remarks, comments, and suggested revision. We appreciate your time and effort in reviewing our manuscript.

We have considered the comments and revised accordingly. We have made changes to address the majority of the issues raised by the reviewer.

Specific comments

- Title: the title is totally un-representative of the content of the paper. A better title could be “Crustal recycled and carbonate components required for the genesis of petit-spot alkaline basalts”, or something similar.

- We revised the title as **“Petit-spot lavas on the western Pacific Plate: contribution of carbonatite and recycled oceanic crust”**

- Abstract: the data in the manuscript do not contribute to define the origin nor the structure of the LAB. The model provided is only petrogenetic. The opening of the abstract must be modified.

- We revised the opening of the abstract:

“The lithosphere....partial melts. The petit-spot...the LAB (lines 44 to 48 in previous ver.)”

to

“Petit-spot volcanism, which occurs owing to the plate flexure, have been reported from around the world. As the petit-spot melts ascent from the asthenosphere, they provide the essential information of the lithosphere–asthenosphere boundary (LAB).”

Please see the lines 43 to 45 in the revised manuscript.

- Lines 74-82: The list of alkali basalts from random worldwide localities is not very meaningful, as readers might be not aware of each specific geodynamic context. Better re-shape this section, discriminating between tectonic settings and inferred geodynamic context, i.e. plume vs. not plume-related origin, continental vs. oceanic, etc....as it is done for petit-spot volcanoes shortly below. Intracontinental alkali basalts are not occurring only in China and North America; intraoceanic plume-related alkali basalts are not occurring only in Hawaii

- Thank you for your remarks. We revised the first sentence of introduction as

“Among the upper mantle-derived alkali basaltic lavas in oceanic settings, those on thicker plates away from the mid-ocean ridge, could be divided into plume-related and non-plume-related volcanoes. For example, ...in Hawaii and Samoa (... Hart et al., 2004; Konter and Jackson, 2012; Koppers et al., 2008; Reinhard et al., 2019; ...Yang et al., 2003). Non-plume related ... magma (Hirano et al., 2006...Yamamoto et al., 2014, 2018, 2020).” Therefore, the occurrence of petit-spot volcanisms supports the presence of melt at lithosphere–asthenosphere boundary (LAB) below the area at least.”

Please see the lines 72 to 82 in the revised manuscript.

- Lines 87-88: references are needed here! Falloon & Green 1989 EPSL; Falloon & Green 1990 Geology; Foley et al. 2009 Lithos; Ghosh et al. 2009 ChemGeo

- Thank you for your kind instruction. References were put in this sentence. Please see the lines 88 to 89 in the revised manuscript.

- Lines 124-131: In this (long) sentence, the concepts of LAB nature and regional petit-spot volcanism are put together, but I feel some explanation is missing. Petit-spot volcanism provides info about asthenospheric melting as many other types of volcanism do. Why should the reader find this type of magmatism particularly useful? This section is the key of the scientific background, as it links the “big picture” to the case study!

- Thank you for your kind remark. The usefulness of petit-spot volcanism is explained by their tectonic setting, that is, they erupted on seafloor without hotspot and ridge activity. We added the phrase in the end of sentence:

“Although there is..., the occurrence of petit-spot...the region because they erupted on the seafloor without hotspot and ridge activities (Hirano et al., 2006....).”

Please see the lines 140 to 143 in the revised manuscript.

- Section 4.4 (geochemical modelling) should be removed from the methodology and included later in the modelling/discussion section, at line 679 (maybe 697). At the moment is out of context, and forces the reader to go back to 4.4 section while being in the middle of the discussion about the nature of the mantle source and the partial melting processes.

- Section 4.4 was moved to Section 6.3. In addition, we deleted the non-modal batch melting model of simple garnet lherzolite. Please see the lines 693 to 716 in the revised manuscript.

- Geochronology: $^{40}\text{Ar}/^{39}\text{Ar}$ dating on these samples is for sure problematic, due to the intense state of alteration.

- As mentioned in the method section 4.3, we removed the alteration factors for the ArAr specimens. The descriptions about “alteration” in the section 6.2 are just about whole rocks. It is not about the ArAr specimens. Any secondary materials in the ArAr specimens are not recognized in the crystalline samples. Please see the newly described part in section 5.3 (line 503 to 510 in the revised manuscript).

However, at the moment the presentation of the data is not convincing enough to be acceptable for publication: no discussion about K/Ca as a function of alteration is provided

- Total K/Ca ratio of both samples is estimated using irradiated $^{39}\text{ArK}/^{37}\text{ArCa}$ ratio as newly shown in Table S4, which are mostly correspond to the molar K/Ca ratio before irradiation estimated using the bulk compositions. Please see the newly described part in section 5.3.

the negative age (-0.11 Ma) obtained for the sample R01 is simply meaningless, and obtained with only 49% of released ^{39}Ar

- Yes, the value “-0.11 Ma” is meaningless and removed from the text as you designated the 49% released ^{39}Ar might be insufficient. Description of the plateau age of the sample #1522 (right) is reduced in Fig.10a as well.

why for sample 1466R6-001 was the inverse isochron age chosen?

- We adopted inverse isochron age of the sample 6K1466 R6-001 rather than plateau age because the initial $^{40}\text{Ar}/^{36}\text{Ar}$ ratio is higher than atmospheric value (excess Ar) as mentioned in the section 5.3.

More details are needed. On the other hand, the age ranges obtained using Fe-Mn crust or palagonite are interesting and useful.

- Please see the newly described part in section 5.3.

- Geochemistry: along the whole results and discussion sections, sample 1466-R6 and R7 are treated as “anomalous” Si-undersaturated rocks, more vesicular, ascribable to a genesis from a different magma suite and thus mantle source (lines 564). This is a crucial issue, because authors use this discrepancy to speculate about different mantle sources for the magmatism. However, from Table 2 it is clear that samples 1466-R6 and R7 have the highest LOI (up to 6 wt%), as reported by the authors themselves later on (lines 594-596; lines 604 “...alteration was not extensive...excluding sample 1466R7-001, 1466R7-003”). In Supplementary Fig. S1 we see that sample 1466-R7 contains the most Fe-rich olivine phenocrysts, but at the same time also altered peridotite xenoliths or xenocrysts are included in this sample (Fig. S4). The question is: is it possible that the whole-rock analyses are just effect of alteration, or have just been “doped” because xenoliths/xenocrysts fragments were simply mixed up with the host rock during sample preparation? This would also explain why Sr-Nd-Pb isotopes are not showing any difference with respect to the other samples (lines 575-576), not requiring any differential contribution of carbonatite flux to the source (lines 584-586).

- As you said, the 1466R6 and R7 basalts have some ultramafic xenoliths (~ 7 mm–10 mm) and xenocrysts. In fact, the majority of xenoliths are pyroxenite or pyroxene xenocryst (<5 mm; Mikuni et al., 2022PEPS) rather than peridotite. These pyroxenite xenocryst and pyroxenite xenoliths are composed of clinopyroxene with SiO_2 of 48–

52 wt% and orthopyroxene with SiO₂ of 51–54 wt%, and then, contamination of these xenoliths would not account for decreasing SiO₂ content of 1466R6 and R7 basalt.

The variable composition of olivine in the 1466R6-001 might be due to the degree of hydration of them (i.e., Fe-rich olivines might be slightly iddingsitized shown in Fig. 3a in previous manuscript), and we analyzed the olivine and clinopyroxene again following your later comment. Please see the new Table S1 and Fig. S1.

As large portions of fresh glass are analysable in these samples (Fig. 4a, BSE image), is it possible to compare the whole-rock analysis of sample 1466-R7 with the composition of its glass (as done for the other samples) and thus verify the difference between the two magmatic suites?

- Fresh glass was not observed in the 1466R6, R7 basalts, and then only whole-rock analysis was conducted for these samples.
- Chapter 6.2, containing the petrographic description of the samples and the evaluation of their alteration state and chemical representativeness, must be moved right at the beginning of the result section.
- We added the explanation of the lack of secondary phases in the petrography part of section 3.1, 3.2 and 3.3. The information of loss on ignition was moved in the section 5.1. Please see the lines 210-211, 234, 268, and 419-421 in the revised manuscript.
- Lines 670-673: Carbonatites are not depleted in U, Th and Nb. A typical feature is the high Nb/Ta, together with low Zr-Hf. Include reference, and/or plot an average carbonatite trace element pattern in Fig. 7 (using the same dataset as that for isotopes).
- Thank you for your correction. The sentence of lines 670 to 673 was modified and references were added as

“The depletions of specific elements (e.g., Ta, Zr, Hf, and Ti) of ...mantle (Bizimis et al., 2003;...Hoernle et al., 2002; ...).” Please see the lines 667, 669, and 670 in the revised manuscript.

- Why for major elements and isotopes are author using the whole carbonatites GEOROC database, but for the partial melting model (section 6.4 and Fig. 11) only the “average carbonatite” of Bizimis et al. (2003) is used?

- For the partial modelling of trace elements, we used the “average carbonatite” of Bizimis et al. (2003) because the trace element composition of carbonatite is very diverse as well as major and isotopic compositions. Bizimis et al. (2003) said that their “average carbonatite” is useful for geochemical modeling of trace element composition. In addition, we tried other carbonatite values which were commonly used in geochemical modeling as well (Hoernle et al., 2002), but they were not suitable to our petit-spot basalts and “average carbonatite” was fit to petti-spot.

- The “average carbonatite” of Bizimis et al. (2003) does not include the radiogenic isotopic compositions, then, in order to show the HIMU-EM1 trend of global carbonatites, we plot the whole carbonatites from GEOROC database in Sr-Nd-Pb isotopic plots.

We put the explanation about it. Please see the lines 716 to 719 in the revised manuscript.

- Lines 698-707: I agree the partial melting of garnet lherzolite with small crustal component + low carbonatite flux is the most plausible among the model performed, but still this model is not able to fully reproduce the Ta, La, Sm-Eu-Ti and Y concentrations. Is there a reason? Did authors try to change the modal composition of the source, melting contribution percentages and/or amount of recycled oceanic crustal material to better fit the petit-spot pattern?

- Thank you for your suggestion. We added the models using the smaller garnet modal ratio of the source, and Sm-Eu-Ti fit the petit-spot. For Y, the partition coefficient of White (2013) was not appropriate, so we changed the data to

McKenzie and O'Nations (1995), which fit well. Ta and La did not perfectly fit, but this may be a limit of such model with the assumption of carbonatite composition.

In addition to that, based on the comments of Reviewer #2, We added the model using the different melt connectivity threshold (α_c). The explanation about that was also added.

Please see the new Fig. 11 with its caption (line 798-809), and line 694-696. 708-709, 713, 719-737.

- Lines 732-747, Isotopic modelling: as authors in the previous section model the percentage of recycled crustal components, peridotitic fertile mantle and carbonatitic flux, it would be extremely interesting to verify if such percentages are also applicable to the isotopic mixing models. Day and Hilton (2011, EPSL; 2021, Geology) and Day et al. (2022, GCA) tried to perform such model for the Canary Island magmatism; the same was done by Markse et al. (2008, JPet) and Pietruszka et al. (2013, EPSL) for Hawaii.

- Thank you for your suggestion. We checked the papers you kindly introduced us.

It would be interesting if the percentages in the trace element model could be applied to isotopes as well, but it is difficult to determine representative isotopic values for carbonatite more than trace element compositions. It is also difficult to assume that “carbonatite is HIMU(or EM1)”, so the discussion here is limited to isotopic “trends” for petit-spots and carbonatites.

- Chapter 6.4 is entirely speculative, does not add value nor constraints to the model. I suggest to remove it up to line 785. The content of lines 785-792, that is the hypotheses made to reconcile the model with the geodynamic context of the area, can be incorporated at the end of section 6.3.

- Thank you for your suggestion. Considering the context at the end of Section 6-3 and comment of reviewer 2#, we included line 775 to 791 at the end of Section 6-3 and a few phrase were modified.

Please see the lines 772 to 796 in the revised manuscript.

- Lines 811-812: remove this sentence. In this paper the origin of LAB is not discussed at all.

- The sentence was deleted.

Technical corrections

- Line 73-74: the opening sentence is too broad, as it is having a poor meaning. Maybe correct to: “the petrogenesis of alkali basalts and the nature/evolution of their source mantle...”. Which tectonic settings? Be more specific.

- According to your comments above (about lines 74 to 82), we modified this part. Please see the new introduction part in the revised manuscript.

- Lines 104-105: Maybe better re-phrasing to: “...to understand the nature of the underlying lithosphere-asthenosphere system and model the geodynamic evolution of the region”

- Thank you for your suggestion. We rephrased this part as your suggestion. Please see the line 109 to 110 in the revised manuscript.

- Lines 109-112: Maybe the concept can be smoothed and made more concise: “In the last 20 years, the increasing knowledge of petit-spot volcanic settings has provided useful insights on the nature of the lithosphere-asthenosphere system, especially in the NW Pacific region (Hirano et al., 2006, Hirano & Machida 2022)”

- Thank you for your suggestion. We modified here as your suggestion. Please see the line 114 to 115 in the revised manuscript.

- Lines 117-121: Sentence too long, can be cut to clarify the concept

- We separated the sentence into two sentences as

“Petit-spot melts, which originated from the asthenosphere unrelated to mantle plume, could be a key to elucidating the nature of the LAB (Hirano and Machida, 2022). Their asthenospheric origin was supported by MORB-like noble gas isotopic

ratios, multi-phase saturation experiment, and geochemistry (Hirano et al., 2006; Hirano and Machida, 2022; Machida et al., 2015, 2017; Yamamoto et al., 2018).”

Please see the line 121 to 125 in the revised manuscript.

- Lines 121-122: Where? Provide some names of localities, as this might be of broad interest to the reader!

- Thank you for your kind comments. We modified here as

“Recently, worldwide including Java Trench, Tonga Trench, Chile Trench, Mariana Trench, Costa Rica, North American Basin and Range, and southern offshore of Greenland, implying the.....magmatism (Axen et al., 2018; ...).”

Please see the line 130 to 131 in the revised manuscript.

- Line 129: what does “hybrid factor” mean?

- Thank you for your remark. The phrase of “hybrid factor” and references were unsuitable.

The phrase of “..., or hybrid factor” was removed, and Audhkhasi and Singh (2022) and Herath et al. (2022) moved to the partial melting group (reference of “presence of partial melting”).

Herath et al. said that the LAB beneath offshore New Zealand were structured. They suggested that the LAB was composed of three distinct layers including radial anisotropic layer, azimuthally anisotropic layer, and melt-rich channel.

We also add and fixed references: Kang and Karato (2023) was added at reference of “the physical property of minerals” group, Hua et al. (2023) was added at reference of “presence of partial melting” group, and Katsura and Fei (2020) was fixed as Katsura and Fei (2021).

Please see the line 136 to 139 in the revised manuscript.

- Line 276: EPMA analyses were not made on glasses only. Please correct and distinguish in this section the analytical conditions for glasses (defocussed

beam?) from those used for minerals. How about matrix correction (Pouchou and Pichoir 1991)?

- Sorry for lack of information. Analytical condition of mineral was added (beam diameter of glass: 10 μ m, mineral: 2 μ m), and some part were modified as

“A peak counting time of 20 s and a background counting time of 10 s were used, except for Ni, for which a peak counting time of 30 s and a background counting time of 15 s. For Na analysis of glass, the peak counting time was 5 s and the background counting time was 2 s.”

Raw data were corrected using a ZAF online correction program, rather than PAP method. The description about it was also added.

Please see the method section in the revised manuscript.

- Line 417 and figure 4a: alkaline and sub-alkaline; dividing line is from Irvine and Baragar (1971)

- Thank you. We added the reference in the caption of Fig. 4a. Please see the caption of Fig 4 in the revised manuscript.

- Line 433: “...compared to the representative ocean island basalt (OIB)”

- This sentence was corrected as your comment. Please see the line 424 in the revised manuscript.

- Data from worldwide carbonatites presented in Fig. 9 are extremely scattered and not very useful, as they cover the entire diagram. Are they needed? If so, I recommend the authors to filter the dataset at least for the geodynamic settings or sample freshness/data quality.

- Thank you for your suggestion. We deleted the carbonatite data with $^{87}\text{Sr}/^{86}\text{Sr} > 0.706$ and added the description. Please see the Fig. 9 in the revised manuscript.

- Line 556: “petrography and geochemistry”.

- This sentence was corrected as your comment. Please see the line 559 in the revised manuscript.

- Line 607-608: not entirely true. Some samples are indeed altered, and are specifically those used to argue for the heterogeneity of the mantle!
- Thank you for your remark. The phrase was revised as:

“These observations showed that most of the petit-spot basalts were unaffected by seawater alteration with a few exceptions.”

Please see the line 607 to 608 in the revised manuscript. Please see reply to comment no. 8 for the major element composition of the 6K#1466R7-001 and R7-003 basalts.

- Lines 617-619: “mass balance calculation of fractional phases....could not be performed because of inadequate phenocrysts” what does it mean?
- Thank you for your question. The phrase was very unclear, sorry.

As described in lines 613–617, petit-spot basalts have no phenocryst (or have only micro-phenocryst), and petit-spot magma might have experienced crystal fractionation during its magma-stagnation within the lithosphere. Therefore, the calculation of primary composition of basalts using the mineral mode on the thin section cannot be conducted. The phrase was revised. Please see the line 613 to 618 in the revised manuscript.

- Lines 668-670: concept repeated.
- Thank you for your remark. This sentence was deleted.
- Line 729: Herzberg
- Thank you for your correction. Please see the line 752 in the revised manuscript.

Figures and Tables

- 1 can be re-organized in a 2x2 panel, as at the moment the panels are too small to be read properly. In the tomographic image to the right, maybe it would be useful to have also a more “zoomed” view of the upper 700-800 km.

- Thank you for your instruction. Figure 1 was changed to 2x2panel. Considering the composition of the 4 panels and the available space, we prepared the tomographic image as a vector file rather than cutting out an upper mantle portion, so that it can be clearly seen when enlarged (but it is pasted as an image file in the submitted file). Please see the new Fig. 1.
- Line 144: Replace “although” with “Notwithstanding”
- Thank you for your correction. Please see the line 156 in the revised manuscript.
- 3: mineral labels should be added to all the optical microscope photos and BSE images. Why not splitting the figure in 4 different figures, or move some material to the supplementary information? As it is, it cannot be presented as a single figure composed by 4 multi-panels...
- Thank you for your remarks. Mineral labels were added to all photomicrographs and BSE images. We think these figures can be vertically organized as Fig. 6 of Mikuni et al. (2022 *PEPS*) <https://doi.org/10.1186/s40645-022-00518-y>. If it is difficult, we will try another way.
- 4: Panel b does not look very helpful, could be replaced with a K₂O vs Na₂O diagram to discriminate Na- from K-affinity. Also, plotting the literature data for kimberlites causes a “squeezing” of the OIB data down to K₂O/Na₂O <1.5, with all the data overlapping with each other. Is this really necessary?
- Thank you for your suggestion. Fig. 4b was changed to K₂O vs Na₂O diagram, and data of kimberlite were deleted. OIB and MORB data from PetDB were changed to those from “Expert datasets” of Stracke et al. (2022) same as the supplemental Figure S3 and S7.
- 5-6: I would recommend to plot MgO wt% in reverse order on the x-axis, so that to increase the degree of differentiation rightwards. This would also mark better the differences with respect to NW Pacific rocks, clearly visible on the CaO/Al₂O₃ vs. MgO plot.
- Thank you for your suggestion. We revised Fig.5 and Fig.6 to plot MgO wt% in reverse order on the x-axis as you said. Please see the new Fig. 5 and Fig. 6.

- 7 caption: “Primitive-mantle- (PM, Sun & McDonough, 1989) normalized trace element.....”. Delete the last sentence.
- Thank you for your correction. Please see the caption of new Fig. 7 in the revised manuscript.
- 9: carbonatite symbols in light green are poorly readable. The details about the mixing model in the caption can be moved to the supplementary material.
- Thank you for your remark. We changed the symbol color of carbonatite to dark blue. Please see the new Fig. 9.
- 10b: provide the reference for the age of the detrital zircon in the Fig. caption.
- Thank you for your remark. Please see the caption of Fig. 10 in the revised manuscript.
- S9. Is it different from Fig. 11 in the main text? If not, why duplicate it?
- This figure is included in the supplement because the model for these samples (1203, 1206, 1542, 1544) was based on “if the trace element compositions of these samples were primary”. Please see the line 732 to 735 in the revised manuscript.
- Table 2: Is this dataset including both glass analyses performed by EPMA and LA-ICP-MS and whole-rock analyses performed by XRF? If so, it must be clearly defined, and possibly the 2 samples set must be distinguishable from each other, not mixed. Values of 0.00 have no sense. Are they not analysed (n.a.) or below detection limit (b.d.l.)?
- Thank you for your instruction. 0.00 was converted to “-“, and the explanation was described below each table (“-“: not detected or <0.001 for cation numbers).

The analytical method was described in each table, and table of major and trace element data were divided into Table 2 and 3.

- Supplementary Table S1: mineral chemistry analyses must be filtered for data quality, at the moment they are totally un-presentable. Some olivine analyses sum up to <98 or >101 (sometimes >102!!!!), must be discarded. Some olivine

analyses have Al₂O₃ >0.2 wt% (sometimes >1 wt%): must be deleted, are just mixed analyses. Filters must also be applied according to the quality of apfu calculations. Values 0.00 are not presentable: are they be not analysed (n.a.) or below detection limit (b.d.l.)? The same applies for cpx and plag, as many grains sum up to <98. Many spinel analyses have >0.8 wt% SiO₂, must be deleted. One spinel analysis has 15 wt% SiO₂, it is just a mixed analysis, delete.

- Thank you for your remark. The compositions of minerals were analyzed again using EPMA in GSJ, AIST. We selected olivine, clinopyroxene, and plagioclase (only 1466R3 basalt). The data of FeTi oxide and spinel would not be presented because they are not significant in discussion of the study.
- Please see the method section (line 290-298), Fig. S1, Tables S1, S2 and S3 in the revised manuscript. For clinopyroxene, we present the Mg# vs. TiO₂ and ternary diagram, and the Al₄ vs Al₆ plot was deleted due to lack of sense in discussion. We also added the description of another EPMA (JXA-iHP200F) for mineral re-analysis in the Section 4.1.

The revised points other than the reviewer's pointed out.

- We verified that the glass analysis data obtained through EPMA represented an average of 10 points. Subsequently, this information was incorporated into the method section. Additionally, Table 2 includes 2-sigma values, and the notation "n=10" has been included.
- The term "detrital zircon" was amended to "zircon in peperite."
- Certain passages were changed from past tense to present tense.
- Symbols representing NW Pacific petit-spots were standardized.
- The PM-normalized trace element range of NW Pacific petit-spots (Fig. 7g) was adjusted using data from Hirano and Machida (2022).