Reply to Editor Comment

Dear Dr. Massimo Coltorti and Editorial support of *Solid Earth*

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

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

The figures applied to the Color Blindness Simulator, and were entirely created by the authors.

The figures in the manuscript appear to be low resolution due to being compiled into a single Word file as an Image file, but each figure is actually prepared as a Vector file. Please excuse any difficulty in readability.

Reply to comments (grey: review comment, black: reply)

・I consider your answer to their comments appropriate and pretty exhaustive. The case study is very interesting and your modelling rather robust, providing information and models useful to the scientific community.
Before it goes public however I also had some comments and suggestions to submit to your attention.

- Thank you for your kind comments and effort in reviewing our manuscript.

・As first I would suggest lightening the weight of your belief all along the text, making the role of the good data you have less important. As an example you state right at the beginning that melt is present at the LAB, giving your idea on the nature of this discontinuity. But this means that melt is physically already present or may be derived from the asthenosphere upwelling due to the flexure of the plate or mantle can melt in response to carbonatite influx? From my scientific (and philosophical) point of view it is nicer to first consider the various hypotheses and at the end explain why you favour one, that is yours.

- The origin of melts at LAB is indeed an important issue. Solving the problem from the chemical composition of our rocks is a challenging. Studying petit-spot volcanism, we have ever been grappling with this problem. The role of our model lies in demonstrating the definitive contribution of carbonatite to LAB melt. Given the understanding derived

from years of research that carbon-rich components can lower the mantle solidus, we believe it is certain that carbon-rich components play a role in the generation of melts at LAB.

- Based on your comment, lines 83 to 87 (in previous manuscript) in the *introduction* was modified as follows:

From (before the revisions)

"The presence of melt in the uppermost asthenosphere could be due to small-scale convection, heating, or the presence of hydrous or carbonatitic components (Hua et al., 2023; Korenaga, 2020). In particular, the presence of CO_2 and carbonated/carbonatitic materials is key in the formation of alkaline, silica-undersaturated melt in the upper mantle (Dasgupta and Hirschmann, 2006; Dasgupta et al., 2007, 2013; Kiseeva et al., 2013; Novella et al., 2014). ……."

To (after the revisions)

"The occurrence of melt in the uppermost asthenosphere could be attributed to small-scale convection, the presence of hydrous or carbonatitic component, or the uplift of the lithosphere in response to plate flexure; however, the possibility of such an occurrence remains ambiguous (e.g., Bianco et al., 2005; Hua et al., 2023; Korenaga, 2020). The presence of CO2 and carbonated/carbonatitic materials is a significant factor in the formation of alkaline, silica-undersaturated melt in the upper mantle (Dasgupta and Hirschmann, 2006; Dasgupta et al., 2007, 2013; Kiseeva et al., 2013; Novella et al., 2014). ……"

Please see the line 83 to 89 in the revised manuscript. (highlighted as yellow are modified part)

By having this sentence, it connects to the last sentence in the discussion part (Sect 6-3), which reads,

"Although the multiple origins of carbonatite are merely suggested and remain unclear, carbon-rich components play a key role in the partial melting of mantle at the LAB (Sifré et al., 2014), constituting the source of petit-spot magma."（line 803-805 in the revised manuscript）

Thank you for your suggestion.

・I found appropriate the change of the title, as suggested by the first reviewer, and with respect to the new title I will go a bit further, changing the order of the two sentences making the tiel even more focussed, that is “Contribution of carbonatite and recycled oceanic crust to petit-spot lavas on the western Pacific Plate”.

-Thank you for your suggestion. We revised the title on your (and reviewer#1’s) comments as follows: “Contribution of carbonatite and recycled oceanic crust to petit-spot lavas on the western Pacific Plate”.
Please see the new title of our revised manuscript.

・I also suggest homogenising the labelling of the samples and always using the same label along the text. At the beginning of chapter 5 you say that dive 6K#1521 samples was labelled 1521 samples or 1466 for dive 6K#1466, but after I see various labels in the text, such as 6K#1466R3-001 and R3-004, which become 1466 R3 in the figures, or simply R3-001 or R3 basalts or 6K#1466R3 series, and so on… In this way it is going to be quite difficult for the reader (at least it was for me) to understand which is which.

- Thank you for your kind remarks. In the Section 5 and beyond, we have removed the notation “6K#” from the descriptions in the samples and unified the expressions like “1466R3 basalts, 1522 basalts, etc.”.
- In the case of listing information about submersible dives, we have standardized the expression to “6K#1542 and #1544 dives”.
  Please see the revised manuscript and highlighted (tracked) parts in the tracking file.

・Part of your modelling needs to be more circumstantial. For example, put directly in the text how much olivine you are adding to your least differentiated basalts, avoiding for the reader to jump in the supplementary and decipher it from the figure and table (row 755 →606?).

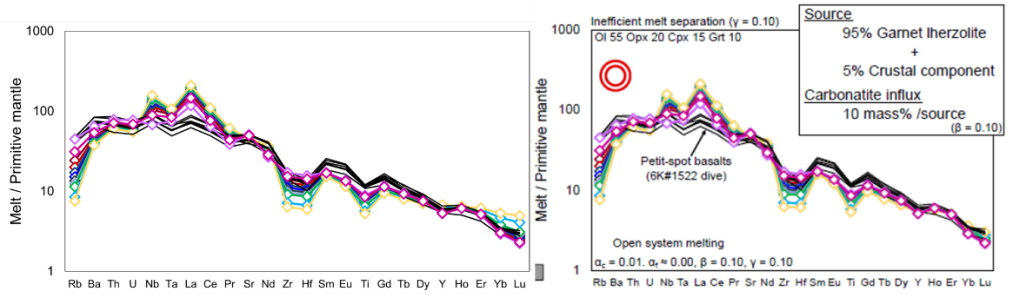
- Thank you for your comments.
  We rephrased the description of olivine maximum fractionation model in the line 660–662 (previous file) as:

“By applying the olivine maximum fractionation model (Takahashi et al., 1986; Tatsumi et al., 1983) to test two samples, it was noted that 7–9% olivine addition was required to achieve the olivine composition corresponding to “Mantle olivine array” in the NiO and Fo# spaces (Figs. S6a, b). The calculated primary trace element contents did not considerably differ from those of the analytical compositions (Table S5 and Fig. S6).” Please see the line 664 to 669 in the revised manuscript.

・Or when you speak about different composition or degree of partial melting (row 660→755?), which of the two parameters you think is acting? If you can explain the compositional variations varying the degree of partial melting, you do not need two different sources. Or when your melting mode foresees negative, that is recrystallization of opx, if I understood correctly (row 806 →706?), which I found somehow peculiar in order to generate primary melt with 47% of silica.

- Thank you for your remarks. Unless the degree of partial melt is extremely large (>~10%), it does not significantly influence the model results.

The model results did not vary significantly due to the melting reaction (e.g. ol 0.03, opx 0.03, cpx 0.44, grt 0.500; Johnson et al., 1990).



Left: Melting mode of Johnson et al. (1990) Right: This study (melting mode of Walter (1998).

All other parameters are the same.

"The calculated primary SiO2 for one of the 1522 basalts, obtained through olivine addition, was 45.8%. This may be slightly higher than that expected, but this is comparable to the case of alkaline lavas on the rejuvenated stage in Hawaii. The Hawaiian rejuvenated lavas modeled for trace elements during Opx crystallization reaction showed an average SiO2 content of 45.9 wt% and a maximum of 47.6 wt% (Borisova and Tilhac in 2021).

Considering the observed HIMU-EM-1 trend in radiogenic isotopes in global petit-spot basalts, our samples appear to have a smaller contribution of EM-1 components compared to NW Pacific petit-spot basalts. As mentioned in the main text, it is uncertain to definitively determine the specific contributions of each mantle endmember. However, the differences in isotopic ratios correspond to the variations of trace element patterns between NW Pacific petit-spots and our samples, which means that the trace element characteristics reflect the source. Here, we would like to emphasize the potential contribution of both carbonatite and the crust to the melting source of petit-spot lavas at least.

・Please consider also the opportunity to have the manus being read by a mother language person, that could improve the reading and the appreciation of your work.

- Thank you for your suggestion. The manuscript was revised by English Correction Service of “Enago” again.
  Please see the revised manuscript and highlighted (tracked) parts in the tracking file.

Other revises

・We added the reference (Wessel et al., 2019) about “GMT”, the tool using making the bathymetric maps. Please see the captions of Figs. 1, 2 and 3, and reference list.
・All other corrections, except where noted in the peer review, were made in accordance with English Editing (Enago).